

## ***Interactive comment on “Turbulence Kinetic Energy budget during the afternoon transition – Part 1: Observed surface TKE budget and boundary layer description for 10 intensive observation period days” by E. Nilsson et al.***

**Anonymous Referee #2**

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### **1 Main Comments**

The paper describes the TKE budget during the afternoon. The authors emphasize the transitional nature of the afternoon boundary layer. The main results in the paper are about the TKE budget in the surface layer. The analysis doesn't appear to reveal features that reflect non-stationarity. The other significant result is the dependence of the dissipation rate on the depth of the boundary layer.

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The results are interesting and worth publishing. However, I would like to see the analysis of the TKE budget extended to include more of the stable data. This may move the paper away from the afternoon transition but should help in determining the neutral values of shear production and dissipation. As part of this more detail of the local area is needed since Fig 1 suggests that it is not simple with trees and buildings in the immediate area of the masts.

In the analysis of the dissipation rate profiles the authors use averages over the whole afternoon. This implies there are large changes in stability as the buoyancy flux decreases, and changes in the shear production which may not be directly related to the diurnal cycle. I'd suggest that the authors check that they can reproduce the result using a shorter averaging period, one that is restricted to periods when the buoyancy flux is large.

I didn't find section 4.1 particularly useful. It seems to just say that the time variations are a bit complicated and that it is necessary to account for shear production and buoyancy production. This is done using surface layer similarity in the next section. The important question for this paper is whether the similarity theory provides a reasonable description of the data, or whether there are problems that appear to be related to the non-stationarity during the afternoon transition. Section 4.1 doesn't help answer these questions.

### **2 Specific points**

Page 29749, Lines 10-14. The length of the afternoon 'transition' is 5-6 hours which is significantly larger than the turbulence timescale. This suggests that turbulence could be described as quasi-steady, at least for a significant portion of the afternoon. The evening transition is more of a transition period, but is not really part of the afternoon transition or the subject of the paper (e.g. 29754 Lines 19-21). Why do the authors

C11087

think the transitional nature of the afternoon should be emphasized.

Page 29750, Line 13. Grant (1997) is about the evening transition after the surface buoyancy flux changes sign, as is Pino et al (2006). The problem is that the afternoon transition has not really been defined but seems to include various periods, such as quasi-steady evolution and the evening transition which are distinguished by different physical processes. I think the authors need to clarify this.

Page 29752, Lines 18-20. This seems a bit obvious, the smoothing is just giving weight to the present observation. Why should there be a robust value for  $S$ .

Page 29752, Line 22-24. The early morning and the period before sunrise are not relevant to this study. They are sufficiently early that they are unlikely to have any effect on the afternoon data, even with smoothing. Periods over night are also not relevant for the study (although see main comments).

Page 29752, Line 25. Why filter the wind-direction, given that wind direction is likely to be very variable for light winds (as mentioned). Wouldn't filtering the components of the wind vector be better ?

Page 29753, Lines 18-22. Again stable conditions not relevant to the present study.

Page 29753-29754, Lines 27-3. Is the run of wind implied by the averaging time really relevant to the effects of topography on the data. Irrespective of the averaging time the air flow will have been determined by the same upstream topography.

Page 29755, Lines 10-15. In the list of causes of fluctuations in the 10 min averages of  $E$  statistical sampling error is not included. This may well be the most important source of variability compared to the physical sources given, so some comment seems to be needed.

Page 29756, Lines 20-21. Would it be better to average the log of the dissipation rates from the 8 periods since  $\epsilon$  must be positive. This will make a difference if the variability of the eight periods is large.

C11088

Page 29757 Line 10. How were the vertical gradients of the third-order moments calculated, over the individual 10 min periods or the hourly averages. Could you calculate the contribution of the vertical component of the TKE flux to the transport term and compare it to your estimate of the transport term as a residual.

Page 29760, Lines 1-9. Is the shallow night time drainage flow relevant to this study. My impression is that it isn't. It would be better to focus only on the afternoon period that is relevant.

Figure 5. These plots are rather unclear given the number of lines on them.

Page 29762 Lines 1-9. I don't understand this. Quasi-steady only requires  $\partial E/\partial t$  is small. Production=dissipation is not required for equilibrium or quasi-steady conditions, it is only a definition of a local balance which is known not to hold for the convective boundary layer.

Page 29762-29763, Lines 25-18. Is this discussion of the dimensional values of the TKE budget really needed. Surely the question to be answered is whether the terms in the TKE budget can be scaled using surface layer similarity. If they can then this automatically takes account of the variations in shear production and buoyancy in the TKE budget.

Page 29765-29766, Lines 5-20. Is the discussion of the terms in the TKE budget at the level of individual points really necessary.

Page 29766 Line 24. Spell out MO (Monin-Obukhov)

Page 29767 Equation 5 is not really a very good fit to the shear production data in near neutral conditions. Most of the points fall below the curve. Some discussion of the low shear production should be given. The obvious explanation is that the von Karman constant is smaller than 0.4, although the present data suggest that it would have to be smaller than the Kansas value of 0.35. The other possibility is that it reflects the local heterogeneity of the site. Figure 1. shows buildings and trees not far from the mast,

C11089

what effect might these have ?

Page 29768 Figure 9. I would think that Fig 9 is consistent with  $\phi_m(0) = 1$  and that the low value given by Eq. 5 is because the data are not really neutral. The authors should comment.

Page 29768 Equation 6. This is not a good fit to the dissipation data in near neutral conditions, most of the observed points fall below it.

Page 29769 line 19-20. The comparison with Hogstrom (1990) needs to be considered more carefully. The imbalance in Hogstrom (1990) could be taken to represent the pressure transport plus errors, since the turbulent transport is accounted for explicitly. Hogstrom's results could, therefore, be interpreted as implying a transport of energy into the surface layer, which differs from the interpretation of the present data.

Page 29770 Line 6. Why is the length scale used in the relationship  $E^{3/2}/z$  not taken to be a function of stability which might explain the variation with height in Fig. 10a. The results in Fig. 10a suggest that each height could be fitted by a line through the origin. The authors should comment.

Section 4.3 This is an interesting section and the final results in Fig. 12 are reasonable in suggesting that including the boundary layer depth in the parametrization of the dissipation rate works. The authors should discuss whether averaging over the afternoons is reasonable given the variations in the TKE budgets shown in Figs 5. Could they obtain this by averaging over a shorter period when the buoyancy term is large ? Do the authors consider that this result is related to the failure of MO similarity for the horizontal velocity components in unstable conditions.

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Interactive comment on Atmos. Chem. Phys. Discuss., 15, 29747, 2015.

C11090