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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Turbulence Kinetic Energy budget during the afternoon transition – Part 2: A simple TKE model" *by* E. Nilsson et al.

Anonymous Referee #2

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GENERAL COMMENTS

This work is a part II of boundary layer studies based on BLLAST field campaign. In the first paper they have studies the problem of the Turbulent Kinetic Energy budget during the afternoon transition. The BL description is based upon experimental profiles while the TKE budget is calculated from surface observations that are acquired during the BLLAST field experiment. Here, the authors have presented a simple TKE model for sheared/convective atmospheric conditions. TKE depends on four budget terms that are parameterized following "idealized mixed layer approximation and a simplified near-surface TKE budget". The principal goal is to study the TKE budget during the afternoon transition. However, I think that the paper may be improved in several aspects, so I recommend publication under major revision, as described below.

SPECIFIC COMMENTS

The legends of almost all figures are in general too long, please report only the description of the figure itself and put comments in the text.

Pag.29813, line 16 Where the “Plateau de Lannemezan” is located ????

Pag.29816, line 19 I think that here authors should explain why they have used this approach (simple parameterization ....) and why not the classic first order closure (eddy diffusivity/viscosity) !!

Pag. 29817 ch 3.2 What is the relations between B0 (eq.2) and w’T’ in the definition of L.

Pag. 29822 The transport term is usually modeled considering turbulent and pressure transport terms in the TKE budget (Stull, 1988). Here it is parameterized following a methodology (Mangia et al, 2000) that was applied for dispersion parameters in a Gaussian Model for tall stacks. So I think that a more complete description should be provided. Furthermore, there is a recent paper with LES that have considered this term in greater detail. I think that it should have been taken into consideration (Puhales et al., Physica A: Statistical Mechanics and its Applications 392.4 (2013): 583-595.)

Pag.29822, lines 7-8 Please provide references of such “sheared convective large-eddy simulations”.

Pag.29825, lines 15-16 Please provide references “LES for this day did not show a pronounced maxima in dissipation rate”.

Pag. 29826 Ch. 3.3.5

The time evolution of TKE is calculated by a finite difference forward in time, with dt=1sec and dz=1m, in which the budget terms (S, B, T and D) are parameterized considering: “idealized linear profiles of buoyant production for a quasi-steady, horizontally homogeneous boundary layer following Lenschow et al. (1980)”. So, we have

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a numerical model with a one second time step, but the budget terms on the rhs of eq.17 are steady-state ???? I wonder if it is not the case to put d(TKE)/dt=0 following Lenschow (1974). Please explain better this point !!

Pag.29826 Colors in figure 7 are not easily distinguished

Pag.29828 The middle row of figure 8 is unclear

Pag.29828, lines 18-20 “In this case, it is clear that wind gradients shift rapidly and the model captures some of the low frequency variability of the observations.”

Change with In this case, it is clear that wind gradients shift rapidly and the model, as a consequence of our simplifications, captures only some of the low frequency variability of the observations.

Pag.29830, lines 3-4 “This is probably mostly related to uncertainty in the way we define initial profiles of TKE for neutral morning conditions.” I wonder if it is possible to start the numerical procedure with experimental neutral morning conditions.

Pag.29831 – Chapter 5 This chapter is it strictly necessary ??? In its current form, the manuscript is rather long and hard to absorb. I think some rearranging would improve the clearness of the paper.

Pag.29834 – Chapter 6 In this section are discussed three aspects that in my opinion are not very well correlated each other, so please give an exhaustive introduction.

Pag.29834 – lines 8-9 “The sensible heat flux used in these model runs are provided by a cosine function as in Sorbjan (1997) and several other earlier studies:”

Change with: The sensible heat flux used in these model runs are provided by a cosine function as in Sorbjan (1997) and several other earlier and subsequent studies:

Pag.29834 – lines 15-16 Is there any reference ??? I meant for the zi modeled with a sine function.

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Pag.29835, line 19 Why table II and table III are splitted ???

Pag.29838, lines 12-14

“Whereas early LES studies (Nieuwstadt and Brost, 1986; Sorbjan, 1997) lead to decay exponents of 1.2 and 2, surface layer measurements (Nadeau et al., 2011) pointed out the existence of a range of exponents (e.g., 2 through at least 6). “

change with: “Whereas early LES studies (Nieuwstadt and Brost, 1986; Sorbjan, 1997) lead to decay exponents of 1.2 and 2, surface layer measurements (Nadeau et al., 2011) and recent LES (Rizza et al., 2013) pointed out the existence of a range of exponents (e.g., 2 through at least 6). “

Pag.29838, lines “Therefore, and in the light of the above simulation results, which show both faster and slower than linear decay rates (and even increasing TKE for afternoons with increasing wind speed), we conclude that at heights near the surface there is unlikely any general simple decay exponent value for turbulence kinetic energy.”

I don’t agree with this conclusion. There are some important aspects that merit to be mentioned. First at all the LES studies have concerned with the bulk averaged TKE (<TKE>), while Nadeau et al (2011) in his modeling just considered a single point in the surface layer but he pointed out the necessity that “LES simulations need to be run to confirm if this behaviour persists after averaging over the entire boundary-layer depth.” Another point is that all the LES results have evidenced that the convective decay of turbulence starts slowly, then the influence of stable stratification causes a rapid collapse of <TKE> at the early evening transition” (Nadeau et al, 2011), this means that the “reality” reproduced with LES is a bit more complex than that described here with this simplified model.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 29807, 2015.

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