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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Turbulence vertical structure of the boundary layer during the afternoon transition" *by* C. Darbieu et al.

Anonymous Referee #1

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"GENERAL COMMENTS"

The paper deals with an interesting problem of the boundary layer meteorology. The turbulence decay at the sunset is reproduced and analyzed using a LES model coupled with experimental data that are acquired during the BLLAST field experiment. This experiment is an international cooperation between some European Institutions and NCAR. The results show some interesting and new aspects that merit to be published. Besides that, I have the following two questions.

- The first main conclusions drawn from this study is that the decay process is generically divided in two phases, which is the early afternoon (0-0.75tau_f) and late afternoon (0.75 tau_f - 1.0 tau_f). In which way this is compatible with the recent findings in

which the exponent of the decay rate has instead three different scaling regimes (t^^-1, t^^-2, t^^-6) ????

- The second important point concerns the "turbulent evolution along the vertical". An important conclusion is that the decay occurs first at the top of the boundary-layer then it propagates downward toward the surface. But as the authors declared, there are some "noticeable differences" (pag.32503, line 15) between the observed and simulated mean profiles (fig.4) and for the TKE (fig.7) as well. Furthermore, the estimation of zi underestimates the observations while the surface stability conditions are not discussed at all. So, I think that these aspects should be more deeply investigated and in particular if this conclusion is not influenced by the poor LES prediction of mean and turbulent quantities.

Regarding the formal aspect of the manuscript; (i) the references, especially in the introduction are not really exhaustive, there are recent works that are not mentioned at all and (ii) the description of LES model is quite concise: which SFS modeling and geostrophic forcing ????

"SPECIFIC COMMENTS"

pag.32494, line 10 There are recent works that should be mentioned, among the others:

- Carvalho JC, Degrazia GA, Anfossi D, Goulart AG, Cuchiara GC, Mortarini L (2010) Simulating the characteristic patterns of the dispersion during sunset PBL. Atmos Res 98:274–284

- Taylor, Alexander C., Robert J. Beare, and David J. Thomson. "Simulating Dispersion in the Evening-Transition Boundary Layer." Boundary-Layer Meteorology 153.3 (2014): 389-407.

pag.32494, line 13 I would mention also the analytical studies of: A.G. Goulart, G.A. Degrazia, U. Rizza, D. Anfossi, A theoretical model for the study of the convective

turbulence decay and comparison with LES data, âĂİBoundary-Layer Meteorology 107 (2003) 143–155. âĂİ

A.G. Goulart, B.E.J. Bodmann, M.T.M.B. de Vilhena, P.M.M. Soares, D.M. Moreira, On the time evolution of the turbulent kinetic energy spectrum for âĂİdecaying turbulence in the convective boundary layer, Boundary-Layer Meteorology 138 (2010) 61–75. âĂİ

pag.32494, line 25

I would add: Rizza et al (doi:10.1016/j.physa.2013.05.009) as 2013b

pag.32497, line 20 Please explain acronyms Ibimet and Isafom

Pag.32499 – chapter 3 LES - Which SFS model have been used ? the standard NCAR code use: P.P. Sullivan, J.C. Mc Williams, C.-H. Moeng, A subgrid model for Large-Eddy Simulation of planetary boundary layer flows, Boundary-Layer Meteorology 71 (1994) 247–276. âĂİ

- The NCAR-LES code use the geostrophic wind as a surrogate of the large-scale horizontal pressure gradient, please comment which value is being used.

Pag.32500 – line 15 What “simplified” does it mean exactly ???

Pag.32500 – lines 19-23 Concerning the large-scale advections, why (u,v) predictions from AROME model are not used ????

Pag.32500 – chapter 3.2 - Figure 4 is a bit confusing, please use less profiles with a different choice of colors (red-blue-black . . ...) and thicker lines. I think that there are significant differences between LES and OBS profiles for all variables, especially for the wind speed. For example for the 1800 UTC (yellow line) profile, the observed wind speed at 1100 m is almost zero while the LES prediction is 6 ms-1, while at the same hour the DT (LES-OBS) is almost 3K. - Furthermore it would be interesting to see a zoomed view of WS in the first 100m. - The LES vertical domain is 3072 m while the figures are up to 2000m.

Pag. 32502 - Lines 19-25 – comments about zi

- After 1800 UTC zi (RS-UHF) is almost 1100 m, while LES prediction is 800 m. Anyway, after 1800 UTC the surface heat flux should have reversed its sign and the PBL should be under stable conditions. Is it realistic ??? I think that authors should provide also a description of stability conditions, perhaps introducing the Richardson number and/or the MO length/velocity scales (L, ustar). These surface parameters are important because in the following it is introduced the concept of delay time between the surface and upper TKE. In these conditions it is important to verify that surface parameters are well reproduced by the simulation.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 32491, 2014.