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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Numerical modelling of microburst with Large-Eddy Simulation" *by* V. Anabor et al.

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Reviewer #2:

We appreciate your comments. Thank you for check so carefully our writing and scientific work. Line 1: "3-D time dependent" it is a common-place feature of the LES technique. - Agreed, we changed the text accordingly.

Line 5: should be "minutes", not "min". - Modified.

Line 7: meaning of full-cloud models is not explained. - We changed to cloud-resolving model which is a more common nomenclature.

Lines 8 and 9: "... principal features observed by Doppler radar and others observational full-scale downburst events”. Style and grammar! - Modified.

Line 12: “... capability of LES to reproduce complexes phenomena”. Style and grammar! - Modified.

Lines 13 to 15: “... potential of LES for utilization in atmospheric phenomena situated below the storm scale and above the microscale, which generally involves high velocities in a short time scale”. Style! - Modified.

1. Introduction

Page 3, lines 2 to 4: “A microburst is defined as a small-scale downburst with its outburst and winds extending for only 4 km and less”. Style and grammar! - Modified.

Page 3, lines 10 and 11: “sampled hundreds of microbursts with ... data”. Style! - Modified.

Page 3, lines 26 and 27: “2-D Large-Eddy Simulation of a microburst on a building model”. Style! Also, the relevance and meaning of “2-D Large-Eddy Simulation” is doubtful. - Modified.

2. Mechanism driving microburst (Style of this section heading should be revised!)

Page 4, lines 25 and 26: “model of evaporatively and melting precipitation driven downdraft”. Style! - Modified.

Page 4, line 27. Lapse rate is defined as the rate of decrease of some quantity (usually with height), So, the increase of the mixing-ratio lapse rate means that the mixing ratio drops increasingly fast with height. Is it what the authors mean here? - We mean an increase in the mixing ratio and in the environmental lapse rate (of temperature). We have changed the text accordingly.

Page 5, lines 1 and 2. The lapse rate (see previous comment) cannot “approach a profile”. - Agreed. Modified,.

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Page 5, line 3: “As thermal stratification and stability increases”. Grammar! Also, what does the word “and” mean hear? Do the authors want to say that the stable stratification gets stronger? - Yes. The stable stratification gets stronger. We have changed the text accordingly.

Page 5, lines 5 and 6: “microburst environment should present a dry-adiabatic like subcloud temperature profile”. Style! - Modified.

Page 5, Eq. 1. Comma after the equation is missing, while the first line following the equations should start with the lower-case w. The buoyancy term should be shown explicitly and explained in more detail, with all dependencies made clear. Particularly, this clarification is needed “because it contains the most important part of the microburst producing phenomena” (see the end of the corresponding paragraph). - Modified to include an explicit form of the buoyancy term.

3. Model description Equation 2. Relation between the resolved potential temperature obtained from this equation and the buoyancy acceleration term in Eq. 1 should be shown and explained. - The equation describing the mechanisms driving a microburst is eq.1

The equation for the resolved vertical velocity component and the virtual potential temperature in the Moeng (Moeng 1984) model are:

It may be seen that the buoyancy term in eq.1 is contained in the equation for the vertical velocity, and indirectly in the equation for temperature through the term . Since these are exactly the same equations employed in Moeng (1984) and usually found in the literature concerning atmospheric convection we see no reason to repeat them in the manuscript.

Boundary conditions. Zero-flux conditions at the top of the domain are known to be insufficient to prevent reflection of gravity waves at the upper boundary and associated spurious effects in the domain. Was this an issue in the study? - We did not find any

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spurious numerical noise in the simulations that could be related to the upper B.C.

The bottom boundary conditions, as known from Moeng (1984) and Moeng and Sullivan (1994), are much more complicated than the ones described in the reviewed paper. Were those the same conditions? - Yes, we use the same bottom B.Cs. as in the referred papers. In this work we tried to keep the LES code of Moeng (1984) in its original form. The only modification is for the additional term (Q) in the temperature equation.

4. Methodology

Page 7, line 1: "spatial-temporal cooling function is parameterized". A function cannot be parameterized. - Modified.

Page 7, line 10. Expression for g(t) is inconsistent with the plot in Fig. 1. Note that square of cos function is always non-negative. - Thank you for pointing that out. We have modified the expression in order to be consistent.

Page 7, line 14. It should be explained how the maximum cooling rate of -0.008 KS-1 was incorporated in the cooling parameterization. Generally, it should be discussed how realistic the parameterized cooling is. - This maximum cooling rate was incorporated exactly as indicated above, following Orf et al (1997). In the text we added a comment about how realistic is the magnitude of such cooling rate.

Page 7: "deep dry adiabatic profile". A profile cannot be deep. Also, the extended region with adiabatic temperature profile could be a characteristic of a convectively mixed layer, not necessarily of a neutral layer. How would the authors distinguish between these two situations? - We agree with the reviewer that a deep dry-adiabatic layer could be representative of an unstable mixed layer. However, because the mixed layer is nearly in a dry-neutral condition throughout most of its depth, the specification of a neutral PBL is our choice for the background environment for the idealized numerical simulation of a microburst. We added this exact comment in the text.

Page 7, last line. Throughout the paper, there is an excessive usage of quotation marks (in this case, in the word “prototype”). - We have removed them.

Page 8, line 9. Capitalization of first letters in “convective boundary layer” is not needed. According to what I know, the convective boundary layer (CBL) in LES of the described kind never really reaches a steady state. What do the authors want to say here? - Capital letters were removed. We agree with the referee, we should have used instead the term quasi-stationary, that according to Nieuwstadt and Brost (JAS, Vol.43, No.6, pp.532-546, 1986) is defined as follows: “In the Planetary Boundary layer (PBL), the time rate of change of turbulence quantities is often small compared to the dominant production and loss terms and thus can be ignored. We will refer to this condition as quasi-stationarity.” A sufficient condition for such a steady state is that the time scale of turbulence is much less than the time scale of the mean flow. LES of a CBL is usually satisfies this condition when the integration is carried out for a sufficient long time.

Page 8, line 13: “profiles of “sheared” wind speed”. Fix the style and avoid abuse of the quotation marks! - Modified.

Page 8, line 14. What does “constant potential temperature profile” mean? Is it about constant temperature or constant profile? - It is a profile of constant potential temperature. The text was modified accordingly.

Page 8, lines 18 and 19. The position of the cooling forcing at the top of domain is not clear from the presented description. Concluding from Table 1, it was placed at the very top of domain (that is above the capping inversion, see Fig. 2) and it was spreading beyond the upper boundary of the domain. Is it physical? How was the forcing affecting boundary conditions at the top? - The cooling function is the same 3D structure used by Orf(1997). The reviewer is right, its center is placed above the cap inversion. However it does not spread beyond the upper B.C. In other words, the upper B.C. is not influenced by the specified cooling function.

5. Results and discussion

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Page 10, line 17: “surface velocity increase rapidly reaching 35 m/s”. Style! - Modified.

Page 11, line 10: “this oscillation may be manifestation of internal gravity waves”. Two questions arise here. What kind of internal gravity waves may be expected in a neutrally stratified environment below the capping inversion? - No gravity waves are expected in a neutrally stratified environment, and we never meant to state that in the text. Once the microburst is developed it modifies the PBL by stabilizing it and from that point internal gravity waves are possible in this microburst-modified environment. We modified the text to make this point clear.

Could this oscillation be an artifact of the numerical scheme? See also my remark regarding boundary conditions in section 3. - Given that the detection of the oscillation is limited to the microburst-modified environment we argue that it represents a physically meaningful phenomenon and not a numerical noise.

Page 11, lines 14 and 15: “a microburst is characterized when the downward current hits the ground”. Style! - Modified.

Page 11, lines 17 and 18: “This simulation close reproduces”. Grammar! - Modified.

Page 11, line 26. Why is U in Umax capitalized and w in wmax is not? - Modified.

Page 11, lines 27 and 28: “All simulated wind speed were normalized”. Grammar! - Modified.

Page 12, line 11: “in a self-similar coordinate”. Better to write “using normalized coordinates”. - Modified.

Page 12, lines 16 and 17: “overlapping ... portrays a good agreement”. Style! - Modified.

References Page 14, line 4. The name of Nieuwstadt is misspelled. Page 15, lines 15 to 19. Words in journal paper titles are starting with upper-case letters, contrary to other references in the list. - Thank you. Modified.

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Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/10/C12845/2011/acpd-10-C12845-2011-supplement.pdf

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