Review of the *Atmospheric Chemistry and Physics* manuscript

Numerical modeling of microburst with Large-Eddy Simulation

by

V. Anabor, **U. Rizza**, **G. A. Degrazia**, and **E. de Lima Nascimento**

Recommendation: needs major revisions

The reviewed paper describes an application of numerical large eddy simulation (LES) to study dynamics of a microburst. The described research represents an obvious scientific and practical interest. However, before the manuscript may be accepted for publication, it would require a considerable revision, primarily with respect to fixing style and grammar (both are rather sloppy), and substantial improvement of presentation of the technical aspects of the study.

Below I list examples of poor grammar and style that need to be fixed. This list is probably incomplete for I am not sure that I managed to catch all the flaws. Correcting the text by a native English speaker would be advisable. In the comments below, I also discuss several ambiguous technical points that have to be addressed in the process of the manuscript revision.

Title

I would suggest reformulating it as **Large eddy simulation of a microburst**. It should also be noted that using words “simulation” and “model” together, as in the current title and in many places throughout the text, is confusing and redundant at the same time, depending on the context. It would be nice if the authors find a way to distinguish between modeling and simulation throughout the paper.

Abstract

Line 1: “3-D time dependent” it is a common-place feature of the LES technique.

Line 5: should be “minutes”, not “min”.

Line 7: meaning of full-cloud models is not explained.

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

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

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!

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!

Page 3, lines 10 and 11: "sampled hundreds of microbursts with … data". Style!

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.

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!

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?

Page 5, lines 1 and 2. The lapse rate (see previous comment) cannot "approach a profile".

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?

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

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).

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.

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? 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?

4. Methodology

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

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.

Page 7, line 14. It should be explained how the maximum cooling rate of -0.008 K s^{-1} was incorporated in the cooling parameterization. Generally, it should be discussed how realistic the parameterized cooling is.

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?

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

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?

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

Page 8, line 14. What does “constant potential temperature profile” mean? Is it about constant temperature or constant profile?

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?

5. Results and discussion

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

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? Could this oscillation be an artifact of the numerical scheme? See also my remark regarding boundary conditions in section 3.

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

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

Page 11, line 26. Why is U in U_{max} capitalized and w in w_{max} is not?

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

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

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

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.