**Review of manuscript GMD-2020-84**

# On the suitability of general-purpose Finite-Volume solvers for the simulation of Atmospheric-Boundary-Layer flow

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**Recommendation:** revisions

The study described in the reviewed paper definitely makes sense, as it provides valuable insights regarding capabilities of a class of numerical schemes broadly used for simulations of atmospheric boundary-layer flows. It should be specifically mentioned (so far, it is not clear from the title) that techniques specifics are considered only for dynamic part of the problem (that includes Navier-Stokes and continuity equations for incompressible fluid). The study is reported in sufficient detail, and the results are analyzed quite comprehensively and candidly.

The only major issue I have with the study is associated with desperation that the reader feels when – guided by the authors – she/he goes through the figures showing the results and their interpretations, and comes (together with the authors) to a conclusion that the whole situation with application of considered second-order-accurate FV schemes for LES of ABL flows in even basic setup is rather bleak (at resolutions investigated), which brings in question the entire feasibility of such schemes. The authors make some comments on what one may expect from the employed scheme with respect to its ability to reproduce particular features of the ABL turbulent flow. In my view, to make sense out of the paper findings, this discussion needs to be significantly expanded in order to provide the reader with a clear guidance regarding performance of the scheme and explain how its specific deficiencies are associated with its properties.

I have also several minor, mostly editorial, suggestions.

1. Page 1: It should be directly indicated in the title, or at least, in the Abstract and Introduction, that only dynamic subset of ABL governing equations is considered, so that the reader will not have hopes for seeing applications of these solvers for heat and scalar transfer equations.
2. Line 96: “proposed” is a wrong word here.
3. Line 105: Such constancy of density is usually associated with the Boussinesq approximation, which should probably be mentioned directly.

4. Line 122: replace “observation” with “assumption”.

5. Line 138: “approximation”. This is more correctly called the Boussinesq hypothesis or analogy (to distinguish from the Boussinesq approximation that refers to the density constancy).

6. Line 199: replace “herein proposed” to “presented herein”.

7. Line 288: replace “statements” by “findings”.

8. Line 333: you need to specify correspondence between the u,v,w notation for velocity components and your standard u_1,u_2,u_3 notation (also in other places, where needed).

9. Figure 8: reduce font size of tick labels in x_2 direction.

10. Line 360: speaking of “solvers”; actually, it was a single solver that was investigated.

11. Line 383: the verdict regarding FV-solvers; sounds too general... maybe still not all of them (a grain of optimism)?