Review of the paper

Open Boundary Conditions for Atmospheric Large Eddy Simulations and the Implementation in DALES4.4

by Franciscus Liqui Lung, Christian Jakob, A. Pier Siebesma, and Fredrik Jansson, submitted to *GMD*

My opinion about this paper is split. On the one hand, I find the topic and the numerical experiments interesting, due in particular to the very high resolution. On the other hand, there are several aspects that I feel need to be reworked:

A Several points in the description of the open boundary conditions are unclear or even not mentioned.

B The reference test case is not sufficiently described, which prevents from really evaluating the performance of the boundary conditions.

C Many statements may seem rather weak, or even quite obvious, in the comments of the simulation results. I think that the conclusions should be strengthened

A - Description of the open boundary conditions

1. Eq. (1) does not make sense, since it adds scalar values, like $\partial u_n/\partial t$ or ϵ, and a vector value $\hat{z}$

2. Line 139: $x_n - \hat{x}.\hat{n}\,\Delta x_n$ is a location, not a cell.

3. Line 143, *Equation (2) is discretised using a second order forward scheme*: what does it mean exactly? Please provide the expression of the numerical scheme. Idem for the discretisation of (1).

4. Line 145, *a Dirichlet boundary condition is used for the boundary-normal velocity components*: I do not agree. A Dirichlet boundary condition for the boundary-normal velocity component would read $u_n = u_n^B$. And a Dirichlet boundary condition for the tendency of the boundary-normal velocity component would read $\dfrac{\partial u_n}{\partial t} = \dfrac{\partial u_n^B}{\partial t}$. (3) is actually some kind of nudging of u_n towards u_n^B, with a relaxation time scale equal to Δt.
   Moreover the time discretisation of (3) should also be indicated.

5. Eq. (4): $S(B)$ is not defined.

6. Eq. (5): S^{int} is not defined. I understand that it is a patch around the boundary, but it should be defined exactly.

7. Eq. (6) is definitely unclear to me. Is ϵ a constant or does it depend on space and time? Is the $\epsilon(S^{\text{int}})$ the same as ϵ? If ϵ is a constant, (6) is indeed only the time derivative of (5), which does not involve any ϵ. The way ϵ is actually estimated should be rewritten clearly.

8. Eq. (7): why do you choose a zero normal flux condition at outflow for all variables but u_n? You could have made other choices: please elaborate a little bit.

9. Line 206 and Eq. (9), *advection over an inflow boundary nudges the boundary value to a given input value*: this sentence corresponds to the equation

$$\frac{\partial \psi}{\partial t} + u_n \frac{\partial \psi}{\partial n} + \frac{\psi - \psi^B}{\tau} = 0$$

which is different from what is implemented. Actually (9) corresponds to the nudging inflow condition for u_n (3) (without ϵ, and with a more general relaxation time scale). But since ψ is discretised one half-cell into the domain and not on the boundary, you have to decide what the value of ψ is on the boudary. For this, you assume that ψ is locally transported at speed u_n, i.e. $\frac{\partial \psi}{\partial t} = -u_n \frac{\partial \psi}{\partial n}$.

B - Reference test case

The reference test case is not really described. It is only said that it is a simulation of *the development of a dry convective boundary layer*, along with a three-line description of the vales of parameters.

10. A better overview of the solution should be given (e.g. some snapshots), and aspects which could have an impact on the performance of the OBCs should be emphasized (e.g. fluctuations in time of the direction — incoming or outgoing — of the flow near the open boundaries).

11. The objectives should be explained: what do the authors want from the OBCs ? What are the key properties and diagnostics that should not be impacted by open boundaries? In particular, do you expect to reproduce the behavior of the reference solution from a statistical point of view or from a deterministic point of view? What are then the quantitative criteria that will be used to assess the performance of the OBCs?

Some details:

12. line 271 *with periodic boundary conditions*: I suppose that periodicity is achieved in the x and y directions, but not in the z direction?

13. lines 280 and 306: *boundary conditions* should be *boundary data*

C - Weak statements and conclusions in Section 4

In my opinion, the critical presentation of the numerical results (Section 4) should be improved, and the conclusions should be strengthened.

12. All figures visually compare reference fields with other ones obtained in simulations with OBCs, but no difference is never quantified.
For instance: *The TKE field near the outflow boundary is not affected by the smoothing* (line 387) , or *the wavelet cross-section remains close to the periodic cross-section* (line 390). Please quantify.

13. Figures 3 to 6: Those figures could be complemented with the difference between the two panels. And the conclusions fully depend on the criteria: do you want a statistical matching or a deterministic matching between the two panels? How could you quantify it?

14. Lines 358-360, ... *shows similar results for both simulations... no clear differences visible...*: in my opinion, this is exaggerated. One should better explain why we can consider that the differences are not significant, which again depends on the criteria that have been chosen.

15. Section 4.1: boundary data are perfect in this experiment, with the same spatial and temporal resolution as the reference simulation. Dirichlet boundary conditions everywhere would therefore give a perfect result. So it is not surprising that the results are good in the vicinity of the inflow boundary. It is what happens near the output that is a priori the most interesting.

16. Several statements are quite obvious: smoothing the input data results in a reduced TKE downwind of the inflow boundary, and deteriorates the solution; adding synthetic turbulence helps to generate developed turbulence faster... Again defining, from the beginning, clear desirable quantitative criteria would help.

17. Lines 385 and 421: it is mentioned that a burst ok TKE is observed, but is there an explanation for it?

18. Section 4.3: the goal of this section is not clear to me. Do you expect for the solution to reproduce the reference solution from a deterministic point of view, or to have a correct level of turbulence? The key question is perhaps the following: which scales are closer (in some sense to be defined) to the reference ones when this artificial turbulence is added?

19. A suggestion: To the best of my knowledge, the introduction of a variable timescale τ for the inflow condition (Eq. (13)) is something new. In my opinion, this is a possible contribution, that is worth being discussed and emphasized.
    In other words, you could discuss more in depth this aspect, by comparing results with a Dirichlet inflow condition on u^B ($\tau_0 = 0$), a Dirichlet condition for the tendency $\dfrac{\partial \psi}{\partial t} = \dfrac{\partial \psi^B}{\partial t}$, and intermediate conditions with several values of τ_0 and p, including $p = 0$ (fixed timescale $\tau = 2\tau_0$). Relevant diagnostics should make it possible to decide if the time and space variability of the timescale has a significant effect.