Review of the manuscript "Large Eddy Simulation of Thermally Stratified Atmospheric Boundary Layers with a Lattice Boltzmann Method" by Henry Korb, Henrik Asmuth, Martin Schonherr, Martin Geier, and Stefan Ivanell, submitted for publication in Wind Energy Science.

In the manuscript "Large Eddy Simulation of Thermally Stratified Atmospheric Boundary Layers with a Lattice Boltzmann Method" the authors present development and testing of a large-eddy simulation (LES) model for simulation of flows in an atmospheric boundary layer based on the Lattice Boltzmann method. The model is evaluated by simulating a conventionally neutral boundary layer (CNBL) and a stably stratified atmospheric boundary layer and comparing the results to previous studies by Berg et al. (2020), Kuhn et al. (2025), and Gadde et al. (2021).

General Remarks

The manuscript is written well, and it clearly conveys the characteristics of the Lattice Boltzmann model, VirtualFluids, and results of the study. Important new development presented in the manuscript is introduction of lower boundary condition for atmospheric boundary layers based on the Monin-Obukhov similarity theory. The numerical study is based on the design of previous studies. The results of the simulations of a conventionally neutral and a stably stratified atmospheric boundary layer (ABL) with the Lattice Boltzmann model are in general comparable to the results obtained with models that use finite volume/finite element discretization. The flow is, as expected, less well resolved in comparison to pseudo-spectral model.

The development of the Lattice Boltzmann model is motivated by the computational performance, however, limited assessment of model performance is presented. The simulations are executed on one GPU. Since the VirtualFluids code also includes parallel processing capability based on the Message Passing Interface (MPI), it would be important to demonstrate the scaling properties of the model as it is commonly done (e.g. Min et al. 2024, Int. J. HPC App.; Sauer and Muñoz-Esparza, 2020, JAMES). Furthermore, the definition of buoyant forcing in the manuscript is not correct. Considering that the simulations of the stably stratified ABL produced reasonable results it is likely that the Equation (5) where buoyancy force is defined is not correct and not the implementation in the code. There are also several statements that would need to be clarified or made more precise as outlined below under Specific Remarks.

Taking all the above into account the manuscript can be accepted for publication in the journal Wind Energy Science after comments and suggestions for major revisions are addressed.

Specific Remarks

Abstract – line 9 – The statement about real time execution needs to be qualified, since real time execution depends on the grid cell size and wind speed.

We have added some qualifying remarks.

Lines 16-18 – The statement starting with "The example..." should be qualified. RANS models may not be able to accurately capture complexity of marine boundary layers, but large-eddy simulations models perform well (e.g., WES; Santoni et al. 2025, Phys. Rev. Fluids; Chatterjee et al. 2025, PRX

Energy).

We have rephrased the whole section to be more concise and more relevant.

Lines 19-22 – The statement about gravity waves and blockage is irrelevant for the paper and should be omitted. Although there are several papers claiming that gravity waves excited by a wind farm can result in significant power production reduction recent study by Khan et al. (2025, WES) demonstrates importance of proper design of a numerical simulation including Raileigh damping. Also, LES of a finite size wind farms by Sanchez Gomez et al. (2023, WES) did not exhibit impactful gravity waves. This study was conducted using a compressible numerical weather prediction model in LES mode.

We have removed this part of the introduction.

Line 36 – It is not clear what "populations" are referenced to.

We have added some clarification.

Line 74 – Reference Stoll et al. (2020) is not the first one where Boussinesq approximation was used in LES.

We chose this reference because it is a review of LES of ABLs. We have added some remarks to emphasize that it is a review.

Line 84 – Coriolis parameter depends on the speed of Earth's rotation and latitude, so these should be used as parameters.

We now note the dependency in the text, however we don't want to formulate everything in terms of latitude since it is common practice to give the Coriolis parameter, for example in Berg et al. 2020 and Beare et al. 2006.

Equation 5 – The definition of buoyancy force for incompressible flow where buoyancy effects are introduced through the Boussinesq approximation is not correct. The numerator should be a difference between local temperature and reference temperature, not the horizontal average of the temperature. Simulations with buoyancy defined as in Equation 5 would not result in correct simulations of a stably stratified ABL.

The buoyancy force is indeed calculated according to equation 5. There are also several other studies employing the same formulation to simulate stably stratified boundary layers and obtain correct results, see, for example Gadde et al., 2021. A very similar formulation is also used in the solver PALM. We choose this formulation to avoid a net force on the whole domain. Preliminary studies with a constant reference temperature led to spurious oscillations.

Line 173 – Instead of "fluid" it should be "momentum."

We have corrected the error.

Equations 30, 31, 32, and 33 – Superscripts (1 and 2) for momentum and heat stability functions can be confused with powers, it would be better to use subscripts instead.

The superscripts indeed denote powers. The subscripts H and M differentiate the stability functions for momentum and heat. To improve legibility we have changed the notation.

Line 209 – It is not clear why the constants from Beare et al. (2006) and Arya (2001) where combined, why were they not taken from a single study. This should be addressed.

We use the values from Beare et al. since we are trying to reproduce the results from that study in section 3.2, however, no values for γ_M and γ_H are mentioned. We have added some clarifying remarks.

Algorithm 1 – Superscript "t" over q_w should be omitted – this is not a tangential flux.

The superscript refers not to tangential quantities but the timestep. We have added some clarification.

Algorithm 1 – Turbulent surface stress can also have a second component in cross-flow direction because of wind veering due to the Coriolis force.

$\mathbf{u}_1$ denotes a vector, so a cross flow component can be present.

Line 224 – It is not clear what is meant by "linkwise-manner."

It is a term from the LBM community referring to the fact that all populations can be computed independently.

Line 229 – Instead of "Coriolis parameter" it should be latitude.

See our response to previous comment. Berg et al only give the Coriolis parameter.

Line 242 – Instead of Allaerts and Meyers (2017) more appropriate reference would be Khan et al. (2025, WES) since it is not likely that the damping was applied well in Allaerts and Meyers (2017).

We have changed the reference.

Line 248 – Smagorinsky (1963) is not the correct reference – Smagorinsky used the strain rate magnitude as a numerical viscosity to make sure that his global simulations are stable, the actual development of a subgrid model for parameterization of inertial range turbulence in large-eddy simulations was presented by Lilly (1966, https://opensky.ucar.edu/islandora/object/manuscripts

We have added a reference to the manuscript by Lilly. However we also retain the reference to the paper by Smagorinsky as this is the work the actual formulation of the model for eddy viscosity originates from.

Line 271 – It is not clear what is meant by "free [atmosphere?] lapse rate." Where is this laps rate applied?

As stated in line 275: the free lapse is used to initialize the potential temperature. Furthermore it is applied as gradient at the Neumann boundary condition at the top of the domain. We have rephrased this paragraph for clarity.

Line 274 – What is "original description" should be made more explicit.

We have clarified the statement.

Figure 1 – The difference between left and right panels should be explicitly addressed in the caption.

We have clarified the caption.

Lines 277-279 – It is not clear why is the NCAR LES described here – a reference would be sufficient.

We want to emphasize the difference in numerical models.

Line 283 – Why were C and D grids used? Why not B and C? This must be addressed.

We compare to grids C and D since only these resolutions are available from both Berg et al. and the AMR-Wind.

Figure 2 – Same as Figure 1.

We have clarified the captions.

Figure 4 – The total stress and TKE should be shown, resolved + subgrid, like in Berg et al. 2020, Fig. 9.

Unfortunately no reference data of subgrid scale stresses is available from the AMR case. We have added an additional plot comparing to the total stress / turbulence intensity from Berg et al.

Line 296 – Based on Equation 4 the geostrophic wind and mean wind should be aligned above the boundary layer. Some plausible explanation should be offered for the reason why they are not aligned.

We have given a more detailed explanation of this offset in a new appendix D. The inaccuracy at small forces is unfortunately an inherent problem of the LBM. However, as we state, the offset is small and does not seem to have any effect on the direction below the inversion.

Line 307 – Considering that the model does not include an equation for subgrid kinetic energy the TKE defined here is only the resolved component.

We have clarified the statement.

Line 310 – The statement in parentheses is not correct, TKE is shown in Fig. 12 of Berg et al. 2020.

We have corrected the statement and added the data from Berg et al.

Figure 7 – Again, total turbulent stresses and fluxes should be presented. The ratio of resolved to subgrid stresses depends on gird size and numerical scheme used (i.e., effective resolution) and the SGS model used.

We have changed the plot.

Figure 8 – Why are these not compared to results in Beare (2006)?

We have added the results from the references.

Line 390 – These are not "observed" rather "predicted."

We have corrected the statement.

Line 399 – It would be good to try to provide more information about computational performance.

We have added a new appendix with both a weak and strong scaling study. However, please note that multi-GPU is not at all trivial and the implementation has not yet been optimized for such cases. Nevertheless we achieve satisfactory results in the weak scaling study.

Appendix C – The title should be "Convergence study."

We have corrected the title.