Review: Enabling the use of unstructured meshes for the Large-Eddy Simulation of stable atmospheric boundary layers

Summary

The authors present a validation study of the YALES2 finite-volume code for simulating atmospheric flow under neutral and stable conditions using unstructured grids. Results from structured and unstructured grids are evaluated against established benchmark studies, and grid-spacing sensitivity is also examined. The study concludes that unstructured grids can reproduce mean flow and turbulence statistics with reasonable fidelity, comparable to structured-grid results and observational data. Overall, the manuscript is well written and the findings are sound. However, the introduction and discussion would benefit from additional context on the respective strengths and limitations of unstructured versus structured grids, as well as further clarification of certain results. I recommend the manuscript for major revision.

Major comments:

1. One of the main motivations of this work is to accurately simulate atmospheric flow over complex terrain and under stably stratified conditions. There is a substantial body of literature demonstrating that LES codes with structured meshes have also been successfully used in such contexts. For example, finite-difference LES codes with both fixed (e.g., WRF-LES, EllipSys3D) and adaptive (e.g., AMR-Wind, ERF) grids has been applied extensively to atmospheric and wind turbine wake modeling under a wide range of stability and terrain conditions (e.g., Berg et al., 2018; Dar et al., 2019; Lattanzi et al., 2024). Some of these studies even include wind turbine simulations in regions with complex terrain and stably stratified flow conditions (e.g., Wise et al., 2022). The authors should acknowledge this body of literature. In addition, some of the statements regarding the limitations of structured meshes for simulating atmospheric flow over complex terrain (e.g., Lines 2–6, Line 338) come across as stronger than necessary. These could be rephrased more carefully or moderated to avoid overstating the case.
2. The results from Section 4.2.2 for the coarse unstructured grid (U1) merit further discussion. It is concerning to see such large deviations between U1 and S1. The time series in Figure 10 shows U1 does not reach stationarity. Moreover, the vertical structure that develops in U1 appears quite different from the other cases. It would be helpful if the authors could clarify whether these findings suggest a more general issue, namely if unstructured grids may be less reliable when the grid spacing is not small enough to resolve most of the inertial subrange of the flow? If smaller grid spacing is indeed required for unstructured grids to achieve results comparable to

structured grids, it would be useful for the reader to better understand what the practical advantages of unstructured grids are in this context. Also, the authors state in several places (e.g., Figure 3, Figure 7, Figure 8, Lines 248-250, Lines 252-254) that the unstructured grid can resolve more turbulence than the structured grid. However, Figure 14 shows that U1 very little turbulence, whereas S1is capable of resolving turbulence to some degree. Could the authors comment on why this is the case?

3. The comparison of turbulence fluxes and variances across grid resolutions could be clarified. As written, it appears that the manuscript reports only resolved turbulence fluxes. Since these are expected to vary substantially with grid resolution (from $\Delta x = 12.5\ \mathrm{m}$ to $\Delta x = 2\ \mathrm{m}$), a more complete comparison of turbulence statistics in Section 4.2.2 should also include the SGS contributions. This would give a clearer picture of the total (resolved + modeled) turbulence stresses in the flow and how each grid performs. In addition, it would help readers if the authors could explicitly state whether the comparisons with observations and other LES codes are based on resolved fluxes alone or on total fluxes.

Minor Comments:

1. Indeed, unstructured grids offer great flexibility to adapt to complex geometries. However, in practice, high-resolution terrain data can be obtained 10-30 m resolution (Farr et al., 2007; USGS, 2021). So, how necessary is it to have unstructured grids if the underlaying terrain data is much coarser than the grid spacing of the model that is required to resolve turbulence in stably stratified flow? I recognize this is out of the scope of this manuscript, but I am curious about the authors' opinions.
2. Line 38: Wind veer also affects wake recovery (Abkar et al., 2018), wind turbine performance (Sanchez Gomez and Lundquist, 2020), and structural loads on turbines (Wu et al., 2025).
3. The authors should include the temperature equation from the model, especially since this manuscript focuses on thermally driven changes in turbulence and mean flow conditions.
4. Caption and labels in Figure 3: Please clarify if turbulence variances calculated using space and time averages ($\mathrm{i.e.}, \langle\overline{\cdot}\rangle$) or only time averages ($\mathrm{i.e.}, \overline{\cdot}$).
5. Figure 10 (bottom) and Figure 5 (bottom): The authors should consider re-scaling the panel for the Monin-Obukhov length so that differences in L are observable in the figure.
6. Appendix and associated discussion: The authors suggest that initial temperature perturbations contribute to simulation results and variability among models (Lines 229-233). I agree that if large enough perturbations are added, then the mean flow conditions may start to deviate. However, the results provided in the Appendix only

show differences in turbulence fluxes and not mean quantities. These differences are of the order of 5% and are largest above ~20 m. The authors should conduct a more thorough analysis before making such generalizations.

References

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