December 17, 2025

Reply to comments by Anonymous Referee #2

Manuscript wes-2025-114 by Olivares-Espinosa, H. and Arnqvist, J.

We would like to thank the reviewers for the time they have invested reviewing this work as well for the insight they provide. Their remarks and suggestions have contributed meaningfully to the improvement of this work.

We begin by noting that we have made a change to the title to add the word *footprint* which we believe better reflects the scope of the investigation. A large part of the manuscript revolves around this topic and we wish to display this from the title, something that was missing in the first submission. We also think that this change reflects some of your comments about emphasizing this study.

Below we address the comments one by one. Reviewer's comments and questions are in slanted font and each point is appended by the letter **R** in boldface with an answer appended by **A.R** in boldface red.

Recommendation*: Minor revision.* **Summary:** *This paper presents a large-eddy simulation framework for modeling wind flow over realistic forested terrain using airborne laser scan data to derive detailed plant area density and topography. The topic is of considerable interest, and the methodology is robust. This paper makes a contributions to the field of wind energy by quantifying how terrain and vegetation heterogeneity influence wind flow and turbulence characteristics relevant to wind resource assessment. However, several issues must be addressed before the manuscript can be recommended for publication. My comments are categorized as either 'Major concerns' or 'Minor concerns', with the former focusing on conceptual technical critiques, and the latter highlighting grammatical and spelling errors.*

Major concerns:

R2-1 *Please emphasize more clearly in the Introduction how this work advances beyond earlier studies such as Ivanell et al. (2018), Arnqvist et al. (2019, 2024), or Boudreault et al. (2017). A short paragraph summarizing specific new insights would strengthen the positioning.*

A.R2-1 Thank you for pointing this out. Below is a list of what we consider being the most important contributions of the study. We added a paragraph near the end of the introduction to reflect this.

(a) Comprehensive model verification compared to above listed references

(b) More detailed validation against observations than above listed references

(c) Quantified footprint study (lacking from above mentioned references)

- Complementary method to estimate footprint to existing methods
- The relative importance of terrain and heterogeneity

– Comprehensive recommendations for modelling choices based on the above points as well as the scaling arguments in Section 2.1

R2-2 *The discussion in Section 2.1 provides useful scaling arguments, but the justification for the selected simulation length (20,000 s) would benefit from including convergence diagnostics, such as the time evolution of the mean and variance at representative heights. It would also be helpful to express the simulation duration in terms of large-eddy turnover times, which would facilitate comparison and reproducibility by other researchers.*

A.R2-2 In principal we agree with the reviewer on this suggestion. However, as stated in Sec. 4.2.1, simulations were started with initial conditions from a previous simulation to speed up convergence. This means that the rate of convergence is somewhat dependent on the specifics of this initial field and would be of limited use for others repeating the study. After initial tests were the rate of convergence was observed, the spinup period was not saved in order to save storage space, subsequently we do not have the spinup for all of the cases. Nevertheless we are happy to provide the plots for case F2 where the spinup was saved.

Fig. 4 below displays the evolution of the components of velocity and their variances during the 120×10^3 s of the simulation run previous to the sampling period, i.e. the spinup. Next to the evolution of the said values, the curves also add a cumulative average of each quantity, to demonstrate that after some period of time, the mean value has converged and more data will not alter the average. For the heights shown in the figure, this occurs sometime after 15 h of simulation. For the other cases, even if convergence took longer to be achieved, it is highly unlikely that it would be reached after the spinup period employed.

Finally, we would like to clarify that the purpose of the analysis in Section 2.1 is not to discuss the convergence of the LES in the canonical sense. Instead, we focus on the requirement to converge enough to facilitate detection of small differences in the wind statistics arising from slightly different upstream forest cover. This is a stricter requirement than what one typically used. This, together with the point above and the fact that the paper is already on the long side has contributed to our decision of leaving the plots out of the manuscript.

For the most relevant cases discussed in the Verification Section 6.1, F1, F6 and F9, the evolution of the means of velocities and variances during the acquisition period can be seen in Figures 5, 6 and 7. In those, it can be seen that the cumulative means stabilize fairly quickly, but as shown with the case F2, this sampling period follows a flow statistically converged. The values of the spinup and sampling periods in eddy-turnover times have been added to the text in Sec. 4.2.1.

R2-3 *The formulation of the additional subgrid dissipation term ("spectral short-cut") is clearly described. It would be helpful to show its quantitative impact—for example, a comparison of vertical profiles with and without the term for one case (perhaps as supplementary material).*

A.R2-3 We believe that what the reviewer suggests is displayed in Figure 5. As per Table 2, the cases F4 and F5 lack the enhanced dissipation, but it is included in F3. As you can see in Figure 5 (c) and (d), this has implications for the TKE close to the

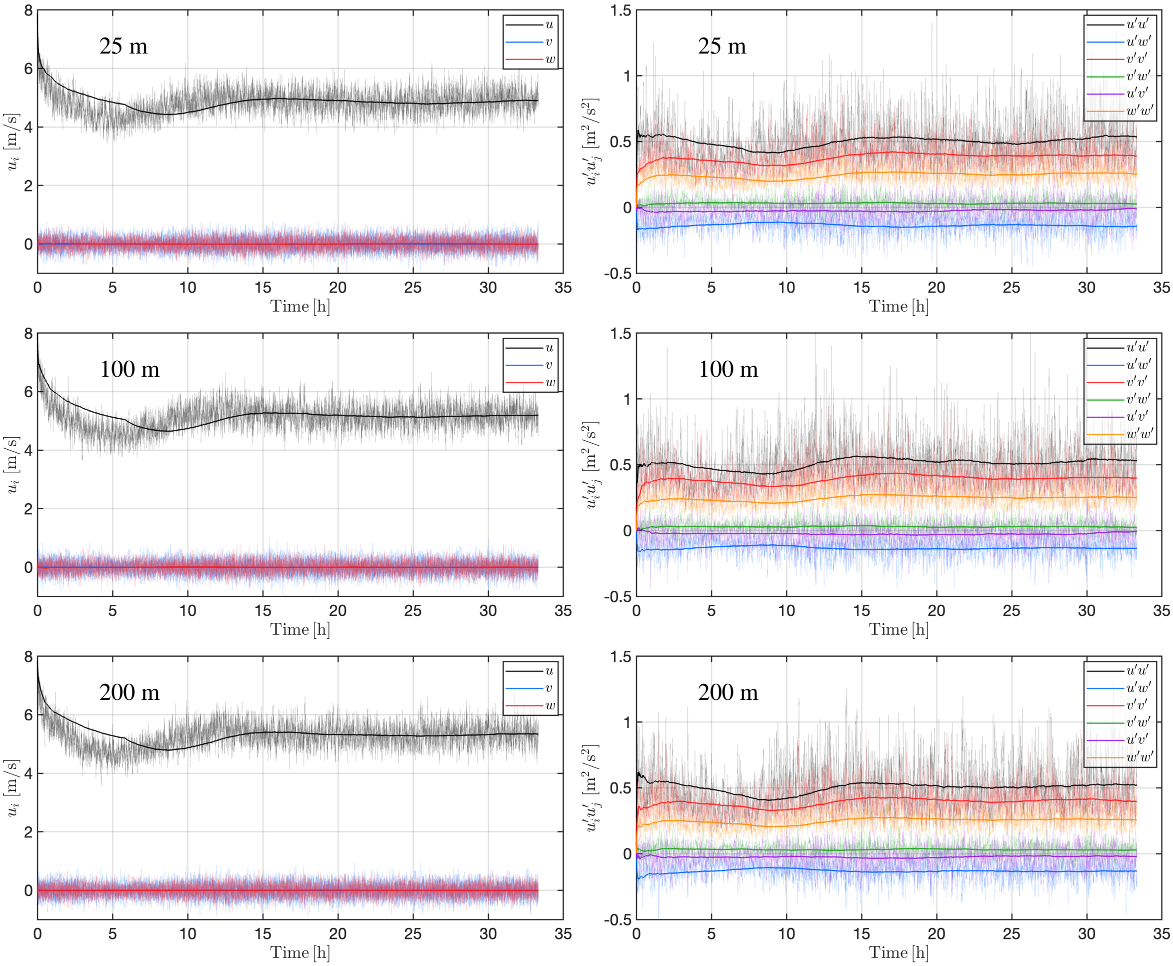

Figure 4. Evolution of velocity components (left column) and variances (right column) for case F2 at different heights for spinup period. The high-frequency value, resulting from the ensemble average of the 9-column data, is shown with low opacity whereas the solid lines correspond to their cumulative average.

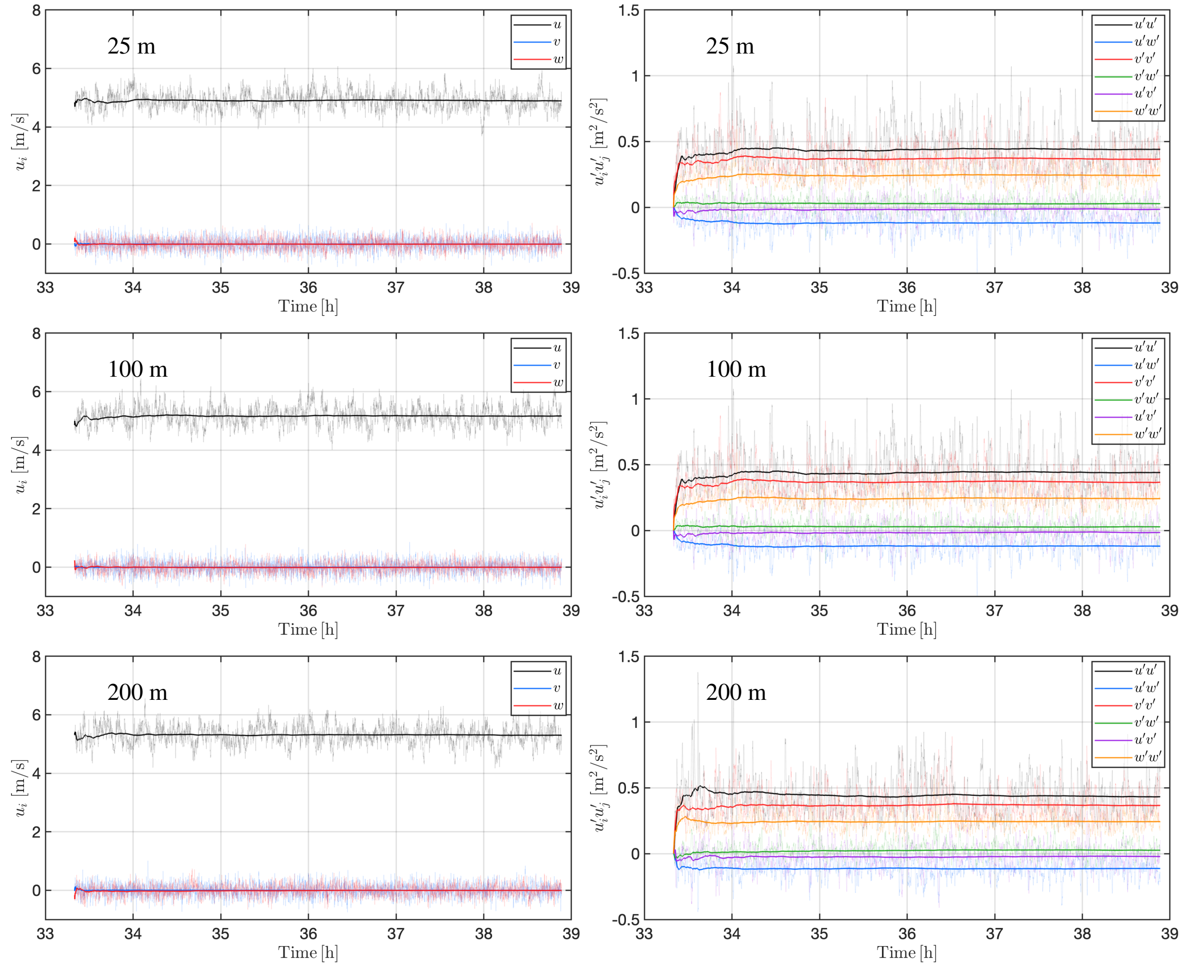

Figure 5. Evolution of velocity components (left column) and variances (right column) for case F1 at different heights during the sampling period. The high-frequency value, resulting from the ensemble average of the 9-column data, is shown with low opacity whereas the solid lines correspond to their cumulative average.

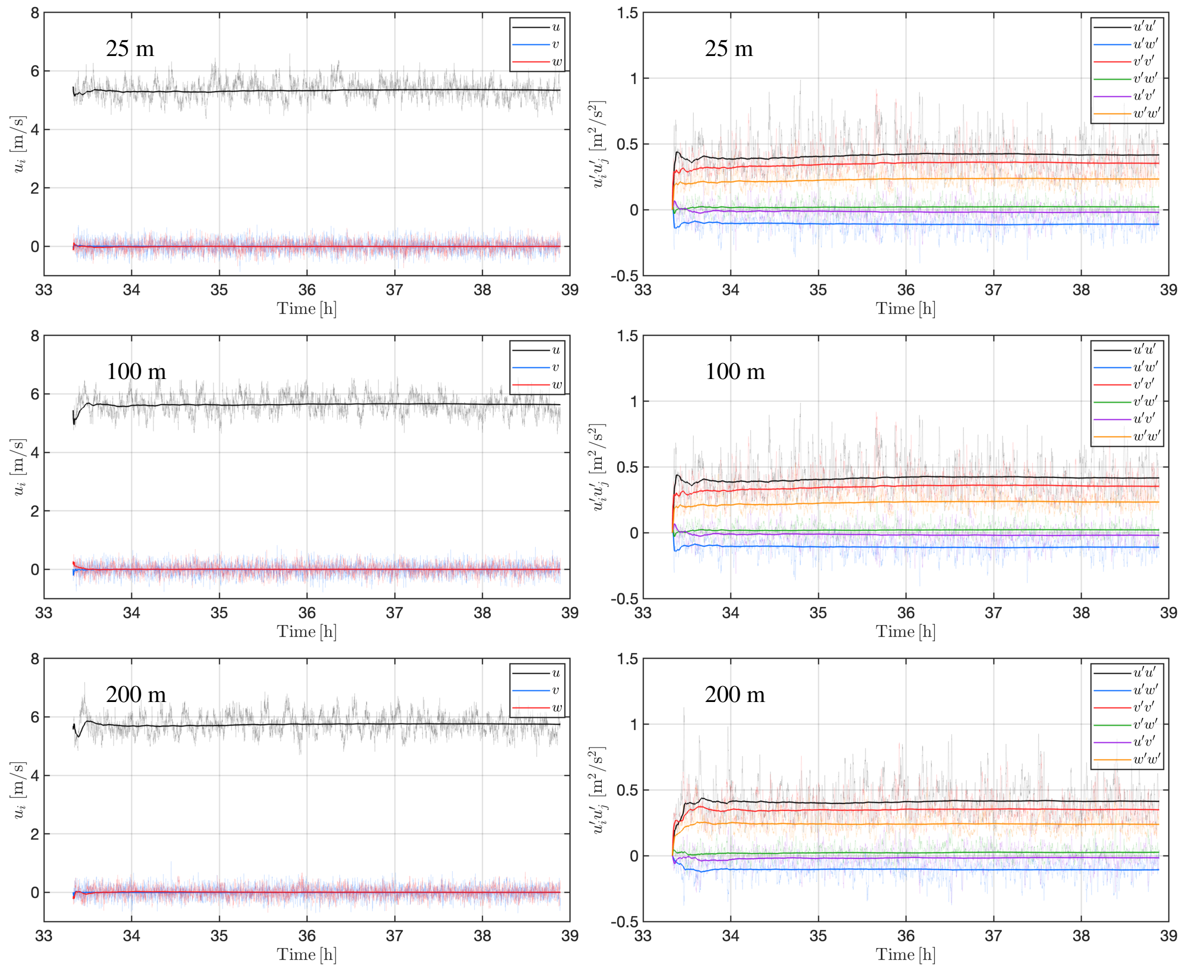

Figure 6. Evolution of velocity components (left column) and variances (right column) for case F6 at different heights during the sampling period. The high-frequency value, resulting from the ensemble average of the 9-column data, is shown with low opacity whereas the solid lines correspond to their cumulative average.

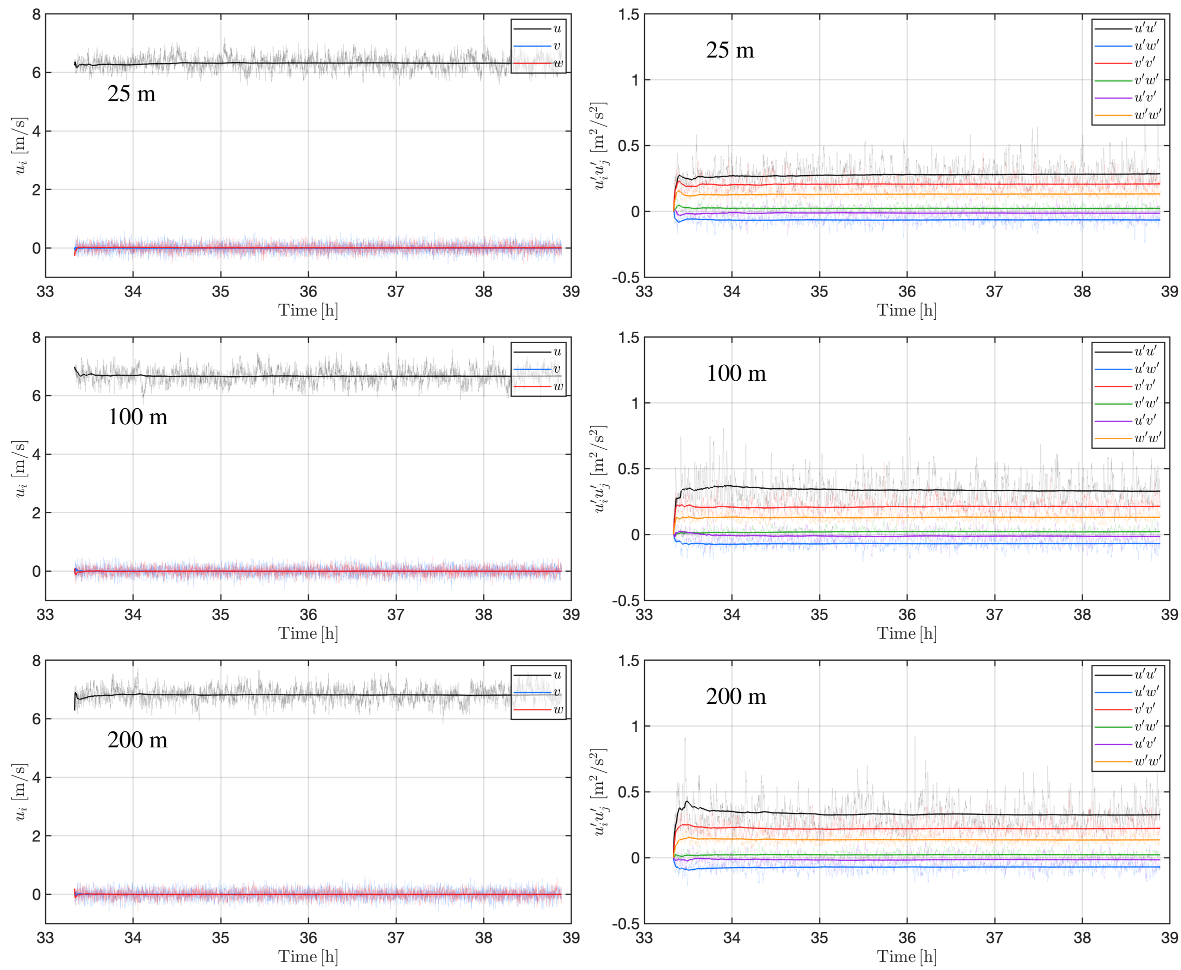

Figure 7. Evolution of velocity components (left column) and variances (right column) for case F9 at different heights during the sampling period. The high-frequency value, resulting from the ensemble average of the 9-column data, is shown with low opacity whereas the solid lines correspond to their cumulative average.

canopy, a relatively smaller impact on wind-energy relevant heights (b) while (a) shows that for wind speed the effect is very small, hardly noticeable at all.

R2-4 *The fixed drag coefficient Cd=0.2 may not capture species or height variability; a short justification or sensitivity test could be valuable.*

A.R2-4 Thank you for the suggestion. We acknowledge that the value of the drag coefficient is probably the most significant remaining weakness in the modelling strategy of flows through canopies. We have updated the introduction and discussion accordingly. We don't have a strong justification of the constant value used in the study, other than that there is, to our understanding, a lack of suitable, well founded, parametrizations for the variation of C_D. Our suggestions for future work reflect this opinion. Since we don't have any good testable hypothesis for the variation of C_D, we believe there would be relatively little contribution from a sensitivity study. The results of which would likely not be general anyway. We are aware that candidates for the cause of variation of C_D naturally exist: Reynolds number sensitivity, streamlining effects, effects of atmospheric stratification, resolution issues for both PAD and the flow, etc. but since there are still so many uncertainties we maintain that the problem is best tackled experimentally and/or with dedicated high resolution simulations at this stage.

R2-5 *The footprint analysis (Section 6.3) provides interesting insights regarding upstream influence. This is a strong result—consider relating it to prior footprint models to highlight consistency or differences.*

A.R2-5 Thank you for the suggestion. We have added references to previous work on footprint and blending height (mainly in the introduction) and highlighted some complementary information brought by this study in Section 6.3 and 6.4.3. We have also added a discussion on the impact of very large scale motions to the estimation of the footprint.

Minor concerns:

R2-6 *Ensure consistency in the reference list—some entries include DOIs while others do not. Please include DOIs for all references where available.*

A.R2-6 DOIs have been added to all reference entries. In the case where a DOI could not be found, URL have been used instead.