Reply to comments by Anonymous Referee #1

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. A bibliography section of citations made exclusively for the reviewers' replies follows.

This is a well-structured and comprehensive study that tackles a highly relevant problem in wind energy and atmospheric boundary-layer research: accurately simulating wind flows over heterogeneous forested terrain using Large-Eddy Simulation (LES). The paper effectively combines theoretical considerations, numerical modelling, and validation against field measurements. It also provides valuable insights into how forest heterogeneity and topography affect turbulence and wind resource estimation.

The clarity of presentation, strong literature grounding, and systematic approach make it a strong contribution. However, several areas need refinement:

R1-1 *Whether or not the forest is modeled explicitly, if the imposed roughness length z0 in the wall model is truly representative of a forest canopy, the resulting profiles well above the canopy should converge. The fact that they differ indicates that the specified z0 is not forest-equivalent. Figures 8 and 9 should be rerun with a truly forest-equivalent z0.*

A.R1-1 We agree with this observation, the $z_0 = 0.65$ in case F9 is not an equivalent roughness value to the PAI/PAD employed in F1. As mentioned in the manuscript, the "equivalent" z_0 and d values were estimated following the principles found in Mohr et al. (2018). A better estimate could be made by applying roughness-sublayer corrections for the flux-gradient relationship, but doing so would still not solve the issue, as the magnitude of the roughness length would introduce limitations in the grid resolution of the LES. For $h_f = 20$ m, Arnqvist et al. (2015) reported a value of $z_0 = 3$ m and $d = 13$ m for neutral conditions, obtained by fitting wind velocity profiles to the logarithmic Monin-Obukhov relationship. The usage of roughness lengths of this magnitude is an important practical problem since standard flux-gradient relationships hold only within the inertial sublayer. Indeed, the suggestion of Basu and Lacser (2017) to maintain the applicability of the Monin-Obukhov similarity theory is to set the first cell above the ground $z_1 > 50z_0$, a suggestion that comprises the simulation of stable ABLs. For neutral boundary layers –albeit not atmospheric–, the collection presented by Huang et al. (2016) of DNS and wind tunnel experiments translate into a threshold of $z_1 > 22z_0$ after applying the same principles Basu and Lacser (2017) –essentially the relation $h \approx 10z_0$ from Townsend (1976)–. To set z_1 at such heights is not desirable for wind energy applications.

To correct the misleading "equivalent roughness", we have added the description of how the z_0 and d are obtained. It is now mentioned that these are calculated based on parametrizations that aim at providing tuning-free estimations based on PAI and tree height, referred to as "PAI-derived". All previous instances of "PAD-equivalent" have been changed to fit this terminology, including Table 2.

On an additional note, wee acknowledge that a suitable combination of z_0 and d together with roughness sublayer correction would exist, especially in an analytical sense, but that using such a setup has the drawback of increased subjectivity because of the empirical nature of the roughness sublayer correction. We have added to the discussion on subjectivity in Page 3 of the introduction and Section 6.1 and Section 6.4.1.

R1-2 *At the Reynolds numbers considered, one would expect to resolve the -1 spectral scaling rather than the classical -5/3 scaling. The reference scaling should be adjusted, and additional discussion of the -1 scaling is needed.*

A.R1-2 Thank you for pointing that out. We agree with the reviewer that parts of the spectrum should likely be influenced by -1 scaling and we have subsequently added the -1-exponent reference to Figure 6. We have referred to Katul et al. (2012) on the theory on -1. We have limited the -1 reference to the LES inter-comparison section, with the idea that the -1 scaling will be less evident in less idealized simulations (and certainly in the measurements) where varying atmospheric stratification and meso- as well as synoptic-flow modulations add variance at low frequencies.

R1-3 *The present method of computing SGS TKE assumes isotropy of the unresolved turbulence. This assumption is unlikely to be valid for canopy-driven flows. The discussion on SGS TKE could be removed, or at least significantly qualified.*

A.R1-3 This is valuable observation and we thank you for noting it. Certainly, the anisotropy caused by the plant elements does also occur at the SGS scales and its inaccurate representation into the model can affect the of production, dissipation and transport of subgrid TKE. The use of eddy-viscosity modelling that relates the SGS stress tensor to the resolved rate of strain and the subgrid viscosity entails the assumption that the dissipation rate is the same in all directions. In directionally dependent environments such as canopy flows, the subgrid dissipation could be underestimated in some directions while overestimated in others (this could include backscatter but it is not considered in our subgrid models). Another issue could be the damping of coherence structures relating to canopy phenomena like sweeps and ejections. The usage of a TKE transport equation as the one employed in our work (Yoshizawa and Horiuti, 1985; Yoshizawa, 1986) can help to alleviate some of these issues as it can capture —in principle— the changes in SGS magnitude across different spatial directions, especially in comparison with Smagorinsky modelling. Yet, it cannot reproduce the missing anisotropy as it is based on the same modelling of the SGS stress tensor.

Naturally, these issues would become more important for coarse resolutions as the proportion of bulk energy in the subgrid range increases. If the resolution is sufficient, the omission of anisotropy in the assumptions of subgrid model should not have a major effect in the energy transfer. Inagaki and Kobayashi (2023) study the effects of considering the anisotropic part of the SGS stress in channel flows and report that coarse (sharp-cut Fourier) filters have an effect on the reproduction of spanwise velocity fluctuations and the generation of coherent structures. Moreover, Bhuiyan and Alam (2020) show that including the effects of vortex stretching and coherent structure magnitude in a dynamic SGS model leads to a noticeably increase in subgrid TKE in comparison with SGS models with a TKE transport equation (note this work remains unpublished and it is available only as a preprint).

Yet, it should be noted that wind statistics acquired from forested areas reveal that variances are sharply reduced inside the forest and around the canopy top in comparison with the flow above. An example of this can be found in the work of Arnqvist (2013, Fig. 4.3 in page 26) where the variances not only become much smaller towards the interior of the canopy but also they become similar in magnitude, in contrast to their anisotropic characteristic above.

A discussion to the effect of the above as been included in the description of the model in Sec. 4.1 and in the discussion of the SGS modelling in Sec. 6.1.1.

R1-4 *At times, the paper reads more like a comprehensive technical report than a sharply focused scientific article. A sharper emphasis on novelty would strengthen the contribution.*

A.R1-4 We acknowledge that the content of the manuscript is extensive and above the average number of pages. There is a substantial amount of material that perhaps could have been split into two separate submissions, for example, one focusing on the verification of the model and another on the characteristics of the flow over realistic forests, including its footprint. In contrast, presenting all this work in a single submission carries the intention of strengthen the confidence in the results obtained with the methodology, in particular those regarding the observations about the usage of ALS-derived PAD fields, domain size and forest footprint.

We have made our best effort in highlighting the most relevant results of our investigation. This is why we have included separate subsections in the conclusions for *Verification, Validation* and *Capturing the footprint of the forest* which also carries the intention of emphasizing the novelty of the study. We kindly ask the reviewer to let us know if she/he believes that further reorganization of the text is necessary to better highlight the novelty of our results.

R1-5 *The long lifetime of streamwise-elongated streaks in turbulent boundary layers is well established. Consequently, the footprint of upstream terrain should not be expected to scale with forest height, but rather with O(10) times the boundary-layer height. The authors should acknowledge this existing work and place their findings in that context.*

A.R1-5 Thank you for pointing out this oversight in the first manuscript. We agree that long streaks and/or Very-Large-Scale Motions (VLSMs) should contribute with an outer length scale that in effect extends the footprint. We have added relevant references to the introduction which we come back to in the discussion. In addition, we now also acknowledge this effect on the spectra Section 6.1.2. We want to clarify though that we still consider the main contribution to the footprint will be from surface related scales and that this seems to be supported by literature, see for instance Paleri et al. (2022). While streaks are clearly visible in the simulations, their impact on the footprint would be limited to large scales since the ratio u/σ_w varies considerably less than u or σ_w individually. Finally, some of the streaks do seem to be connected to specific upstream surface features and would then be partly different from the streaks discussed in the literature of VLSM.

References in reply to reviewer's comments

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Inagaki, K. and Kobayashi, H.: Analysis of anisotropic subgrid-scale stress for coarse large-eddy simulation, Physical Review Fluids, 8, 104 603, https://doi.org/10.1103/PhysRevFluids.8.104603, 2023.

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