Response to Reviewers

July 9th 2026

Please find out responses to the reviewers' comments below in blue.

Reviewer 1

Dear authors,

Thank you for the effort you put in the improvement of the manuscript. I agree that splitting the MS in two is a good strategy. The current MS improved significantly, the concepts and the model's turbulence scheme are now explained in an understandable way and the results are now more easy to follow. However, there is still some improvement necessary for the ABL height determination, the figures, and some minor issues.

We thank the reviewer for this positive assessment of the revised manuscript, and for taking the time to review it a second time. We have now addressed the remaining concerns in the manuscript, with point by point responses below in blue.

Determination of the ABL height

The EDR determination method seems to be a very promising tool to determine the mixed-layer height. However, my concern is, why do you use such an outdated methods as like the Richardson number method (Sorensen, 1996)? I am asking because even within the Ri# method there are nowadays more sophisticated methods (Vogelezang and Holtslag (1996); Seibert, 2000) and the authors even mention an up-to-date reference. The Vogelezang and Holslag method was also implemented in the COSMO Code as a ABL height diagnostic, therefore I see no reason why such a primitive method is chosen to determine the ABL height so stable boundary layers. I think setting the ABL height to a constant for stable boundary layers is very low-effort and not suitable for a scientific publication. You might really want to adapt these Richardson height ABL height diagnostics for the stable boundary layer (Liu and Liang, 2010), especially because there are plenty of improvements (see aforementioned references) present in the literature.

The reason we chose the rather outdated Richardson number method (with assumed constant values under stable conditions) is not due to low-effort, but because it reflects the operational status quo in ICON/ at DWD. This method (including a fixed value during stable conditions) is applied in post-processing and distributed as the operational MLH diagnostic. Part of the point we wanted to make here is how unsuitable this method is, and that better alternatives should be pursued - one of which may be an EDR-based method. There are efforts underway to update the post-processing diagnostic with a more current Ri-number based method. Part of this effort involved reviewing existing (but unused) implementations in other parts of the code, and from literature. This work is not finalized yet, but we have access to an offline (post-processing) version of MLH diagnostic based on Vogelezang and Holtslag (1996) and have included the MLH diagnosed thus in Figure 7. This version differentiates between unstable, neutral and stable conditions, and includes the friction velocity in the shear production term of the Ri equation (Vogelezang and Holtslag 1996, their equation 3). We have modified the corresponding discussion in the text.

Figures

- Fig1b,f; Fig5; Fig7a; : Please avoid so-called "rainbow colormaps" like "jet" etc, because they are not perceptionally uniform. You can find an overview on why to avoid these colormaps in Stauffer et al. (2015)

  We thank the reviewer for pointing this out. We have updated the relevant figures in the manuscript by replacing the non-uniform rainbow colormaps with perceptually uniform alternatives, as recommended by Stauffer et al. (2015).

- Fig 7: please adjust your height axis s that values are not cut off. Done

Minor comments

- Title: You might mention that you test ICON's operational setup at 2.1km in the manuscript title.
- Please mention ICON's horizontal resolution on the abstract.

  We have now included a mention of the horizontal mesh size in the abstract (and conclusions), but kept the title as is to keep it concise.
- line 206: "discarding the first 12 hours of simulation": Why? Spinup?

  The main reason for discarding the first 12 hours of forecast days 2 through 5 is because we want to stitch the individual forecasts together at 12UTC, rather than at 00UTC or 6UTC. We are particularly interested in examining the stable night-time boundary layer, and this allows us to have an uninterrupted simulation each night, and through the morning growth phase. There may be some spinup (simulations for day 1 are shown from the start at 00UTC), but this does not take 12 hours. We have added a sentence in the text to motivate this choice.
- line 208: "typical summertime convective boundary layer": you might specify what you mean by "typical", ie. with a reference.

  We have changed the wording slightly to "The period chosen encompasses five days with a convective boundary layer evolution during daytime, while low level jets (LLJ) were observed during nighttime". While convective boundary layers are indeed common in the summertime Lindenberg, we do not want to make an explicit claim that these five days are particularly representative of some type of climatology. This then also addresses the remark below.
- line 210: "intense convective cells": are they also typical for CBLs?
- line 280: "represents the total turbulent energy": Please be careful, in dynamical meteorology, total turbulent energy (TTE) is actually the sum of turbulence kinetic energy (TKE) and turbulent potential energy (TTE). But I assume you mean the sum of resolved and SGS-TKE.

  Good point. We have adjusted the wording.
- line 414 "ABL scheme's parameterization": the parameterization is the ABL scheme itself, right?

  Yes, we have adjusted the wording.
- line 436: "atmospheric layers with strong turbulence": you mean the atmospheric boundary layer, it's its definition?

  Yes, we've adjusted the wording here as well to make clear that we mean the ABL.
- line 549: Please add the horizontal mesh size of the model here.

  Done.
- line 557: "convectively mixed layer": Do you mean convective boundary layer?
- line 557: "stable atmospheric conditions": Most of the atmospheric column is stably stratified, do you mean "stable boundary layer"?

  We have adjusted the wording for the two comments above. All wording changes are highlighted in the tracked changes version of the updated manuscript.

References

- Liu, S. and Liang, X.-Z.: Observed diurnal cycle climatology of planetary boundary layer height, J. Climate, 23, 5790–5809, https://doi.org/10.1175/2010JCLI3552.1, 2010
- Seibert, 2000: Review and intercomparison of operational methods for the determination of the mixing height, Atmos. Env., https://doi.org/10.1016/S1352-2310(99)00349-0
- Sørensen, J., Rasmussen, A., and Svensmark, H.: Forecast of atmospheric boundary-layer height utilised for ETEX real-time dispersionmodelling, Phys. Chem. Earth, 21, 435–439, https://doi.org/10.1016/S0079-1946(97)81138-X, 1996
- Stauffer, R., G. J. Mayr, M. Dabernig, and A. Zeileis, 2015: Somewhere Over the Rainbow: How to Make Effective Use of Colors in Meteorological Visualizations. Bull. Amer. Meteor. Soc., 96, 203–216, https://doi.org/10.1175/BAMS-D-13-00155.1

- Vogelezang, D.H.P., Holtslag, A.A.M. Evaluation and model impacts of alternative boundary-layer height formulations. Boundary-Layer Meteorol 81, 245–269 (1996). https://doi.org/10.1007/BF02430331

Reviewer 3

The revised manuscript presents a Doppler-lidar-based evaluation of wind and turbulence properties in the operational ICON-D2 model, with emphasis on the Turbdiff TKE scheme and diagnostics to make model TKE comparable to the scales of the lidar retrievals. The authors have substantially addressed the major concerns raised in the previous review round by narrowing the scope of the manuscript and removing the comparison between the Turbdiff and Smagorinsky schemes. The revised manuscript benefits from the rewritten sections on the Turbdiff scheme and the methodology of the TKE diagnostic. The quality of the lidar retrievals is also discussed in greater detail. The authors reasonably clarify that the section on potential applicability for the wind energy community is intended to be exploratory, and that a discussion of formal acceptance criteria lies outside the scope of the manuscript. Only a few technical comments remain. I suggest that, after technical corrections outlined below have been considered, the paper can be accepted for publication.

We sincerely thank the reviewer for evaluating our revised manuscript. We appreciate the reviewer's recognition that the concerns raised in the previous review round have been comprehensively addressed and that these revisions have strengthened the manuscript. We are grateful for the reviewer's positive assessment.

Technical Corrections

1. L318: Correct the figure reference from "Figs. 1 [c1]-[c4]" to "Figs. 4 [c1]-[c4]".

   Done.

2. Fix minor grammatical issues: Examples include "too rapidly dissipated" instead of "to rapidly dissipated", "quite large" instead of "quiet large", "a weaker, more distant cold pool" instead of "weaker, more distant cold pool", and "June" instead of "june".

   Thanks for finding these typos, they have now been corrected.

3. Correct the caption of figure 2: "Displayed USA-1 sonic (Metek GmbH) data at a height of 90 m" instead of "Displayed USA-1 sonic (Metek GmbH) data from a 99 m tall mast".

   Done.