

Overall Comments:

The manuscript “Resolution-dependent uncertainties in global potential dust emissions: A comparison of convection-resolving ICON (version 2.1.02) and parameterized ICON-ESM (version 1.0) configurations” by Kunze et al. analyzes how grid resolution and convection representation matter for dust simulations. Using a 200-km configuration with parametrized convection and a 2.5-km convection-permitting configuration, they look at the wind speed differences and their impacts on dust uplifting potential (DUP) in several regions including Africa, Asia, and Australia. They focused on the summer period (DYAMOND1 period in boreal summer 2016 for the Northern Hemisphere and DYAMOND2 period in austral summer 2020 for Australia). The authors used the CMIP6 experiment archive of ICONv1 for the 200-km configuration and the DYAMOND experiment archive of ICONv2.1 for the 2.5-km configuration for the comparison. The analysis finds that ICON’s CMIP run can sometimes generate higher mean wind speed than its 2.5-km configuration, suggesting that the coarse-resolution model may overestimate large-scale winds and the breakdown of low-level jets. Meanwhile, the high-resolution run shows higher tails of wind speed, suggesting that the coarse resolution simulation may not be able to capture the extremes. The analysis is overall reasonable, but the major issue is that the model versions are different between the low-res and high-res runs. There may be physics and dynamics updates that people outside of the ICON community do not understand. I suggest that the authors use the ICONv2.1 with the same configuration and rerun the coarse-resolution runs for the two DYAMOND1 and 2 periods, which is just ~80 days in total (not counting the spinup period) to make the discussions and results more convincing. Another issue is that the authors use the 2.5-km configuration as a gold standard and keep saying that the low-res run has overestimates and underestimates here and there. I do not agree with these wordings, because papers have shown that the km-scale runs have their own biases (see detailed comments below) and can subsequently lead to biases in dust simulations. I think both runs need some model evaluations with observations (for their spatial variability and diurnal cycle representation) so that we know the strengths and weaknesses of both configurations.

Detailed comments:

Line 145: Using $u_t = 7 \text{ m s}^{-1}$ may bias the DUP maps. For instance, for China, u_t is usually at 8–10 m s^{-1} in JJA (e.g., see Fig. 1 in Pu and Ginoux, 2020; <https://acp.copernicus.org/articles/20/55/2020/>). The choice of u_t will affect the spatial distribution and diurnal variability of, for instance, Fig. 11 and Fig. 12. Why would u_t be a global constant? I think a regionally constant u_t would make sense since we want to focus on wind variability and not account for the regional spatial variability of u_t , but we may not want u_t to be too far off from the observationally constrained value such that DUP maps become less realistic.

Line 222: I think ICON model version difference can be a big issue. While the 200-km configuration uses ICONv1, the DYAMOND run uses ICONv2.1. There must be some dynamics and physics updates in the newer version. For instance, when I look at Fig. 16a-b for DUP differences in Australia, it looks like there may be physics changes (especially

boundary-layer physics). I am not sure if all changes can be attributed to either grid resolution differences or convection differences when the model version itself has changed. Running a 40-day simulation in the 200-km configuration with ICONv2.1 will be quick and cheap, so I suggest that you do that. If you see pretty much the same results as the ICONv1/CMIP6 runs, the conclusions and attribution to grid resolution or convection will be a lot more convincing.

Line 233: The whole manuscript is trying to refer to the 2.5 km run as the realistic reference, and the RMSE is built off of this assumption. It makes sense that the 2.5-km configuration is a more advanced configuration, but I do not think we can say that the 2.5-km run is realistic without comparison against observations or reanalyses. You should at least use the ERA5 or surface meteorological sites as the references in the RMSE term and actually see if the 2.5-km run is matching the reference better in terms of spatial variability and diurnal cycle. Papers have shown biases in km-scale convection-permitting configurations in the DYAMOND experiment, e.g., km-scale MPAS can overproduce mesoscale convections associated with African Easterly Waves (Spät et al., 2024; <https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2024GL108856>). I think model validations against observations or reanalysis in the 40-day period for spatial variability and diurnal cycle are needed.

Line 518-526: It is not clear to me why the 200-km configuration has higher wind speeds over Perth than the 2.5-km configuration. Is it due to differences in sea-breeze/trough circulation, the coastal pressure gradient, boundary-layer mixing/turbulence, or surface roughness? Also, in my mind, this could be due to the model version difference too.

Figures 10, 12-13: I think it is Tian Shan instead of Tien Chan. Please correct the spellings.

Figure 17: I think it's a graph for Australia, not central Asia. Please correct the typo.

Figure 17: Also, why does the diurnal cycle time series not connecting themselves to 06:00 to 09:00?

Other: Overall, the manuscript is written in a way that is a bit repetitive, since the findings are roughly similar across regions (e.g., LLJ breakdown and missing convection). I suggest shortening the text for China and Australia, as well as Sect. 4 a bit, since they are to some degree repeating what is covered in the Africa and the Middle East / Central Asia subsections.