

Review of “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”

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1 General Comments

This article attempts to look at how dust emission events in climate models differ between model resolutions, specifically in the ICON model. The authors compares simulations ran with the low-resolution version 1 ICON-ESM model for the Climate Model Intercomparison Project 6 (CMIP6) ScenarioMIP setting, and simulations with a high-resolution version 2 ICON model ran for the the high resolution DYnamics of the Atmospheric General Circulation Modeled on Non-hydrostatic Domains (DYAMOND) experiments. They use the dust uplift potential (DUP) metric to speak specifically to the differences of dust uplift potential as driven by different representations of surface winds between the two models. They find systematic differences between the two models, including differences in representations of the diurnal DUP and local persistent wind systems. They claim that the differences between the models are due to the representation of convection.

This topic is definitely under-explored in the climate modeling literature, even though dust is a known important climate driver. Understanding more about how these dust emissions are represented in models across the world is super important, and I commend the authors on focusing so much on important desert regions and learning about the local dynamics in each of them. For example, I really enjoyed the discussion of the known prevailing winds occurring in various desert regions around the world, as these winds seem like really good candidates for global models to consider parameterizing. However, this analysis has a number of significant methodological hangups that make me unwilling to trust the final conclusions. The two biggest issues that I'll note here, although I mention more below: (1) the comparison across both different model resolutions and model versions, but then solely attributing the differences to model resolution (and therefore convection) (2) the suggestion that the high resolution version accurately represents these systems without any robust comparisons to observations.

2 Specific comments

1. Model version vs resolution comparison problem: Comparing simulations ran with ICON v.1 to ICON v.2 for resolution attributions without addressing the differences between the ICON versions can easily lead to an over-emphasis on the resolution differences rather than physical structural differences between the two. To remedy this would either require choosing only one of the ICON versions and finding two different resolutions of the same ICON version, or doing a full multi-model comparison between the entirety of the CMIP6 and DYAMOND ensembles. On the other hand, if it is reasonable to compare across ICON v.1 and ICON v.2, that needs to be explained and justified thoroughly in the methods section. After looking at the ICON DYAMOND experiments described in Hohenegger et. al. (2020), I would suggest to just replace the CMIP6 analysis with the coarser resolution version of ICON v.2 2 ran for the DYAMOND experiments - namely the one with an 80km horizontal resolutions. Performing the analysis with that data would be a much cleaner comparison, where we could ensure that differences are coming solely from model resolution rather than model structure.
2. Imprecise phrasing about inclusion of surface properties in the DUP: There are a number of times throughout the paper that the claim is made that the DUP "depends solely on near-surface wind speed exceeding a threshold for dust emission" (lines 140-141). However, there is a note that the authors "incorporate a

dependence on vegetation cover following Tegen et. al. (2002)" (line 103). I am not sure which analysis was actually conducted and whether or not vegetative surface properties were included. If they were, that would make this analysis not able to distinguish between convective or vegetative processes in the models, making interpretation of results much less clear, so I recommend against it. The methodology mirrors this confusion, as it is noted that surface characteristics are primarily instituted through the wind speed threshold for dust uplift u_t and that the author assume it to be $7m/s$ globally. A constant assumption here would indicate a lack of incorporation of surface properties, but then there is a discussion of calculations of an effective dust source area A_{eff} , a parameter dependent on vegetation surface properties. How A_{eff} is incorporated (or not) into the DUP is not made clear. To fix this, I would either (1) clarify how the assumption of a constant u_t neglects of surface properties or (2) remove the claims that the study is solely dependent on atmospheric dynamics, and add some clarifying information about what is and is not included, as well as additional discussion about how different vegetation cover models could change results.

3. Choice of u_t constant: Zhao et. al. (2022) and Marsham et. al. (2013) claim that the threshold of $7m/s$ is reasonable for North Africa, but I don't see any reasoning for why this threshold is okay to use as a constant around the globe. This nuance should be fleshed out, and if it is an assumption, it might be interesting/necessary to conduct a sensitivity analysis of the choice of this threshold on results. For example, on line 340 it is noted that some of the "CMIP6" data fell just below the threshold, so the coarse resolution may have been in agreement with the high resolution if a different threshold had been chosen.
4. Missing discussion of validity of surface vegetation model in desert regions: As the authors note, "Previous studies have shown that uncertainties in prescribed surface characteristics contribute substantially to uncertainties in dust emissions" (line 159). If vegetative properties are included in the calculation of DUP, then there is a need for more discussion surrounding misrepresentation of surface vegetation in models in these areas and the impact that could have on these results.
5. Confusing assumptions about snow cover: The authors note that "Due to the limited availability of snow-related variables [...] near-surface air temperature is used as a proxy" and that DUP is masked when air temperatures are below 0 "during which dust emission is unlikely" (lines 180-185). I am confused by the implication that being at temperatures below 0 automatically means that the ground must be covered by snow, especially in high deserts regions that don't experience very much precipitation. I would argue that it seems very plausible that there could be significant dust emission events in temperatures below freezing in these areas. Are there any studies or other justification for this claim? How sensitive are the results to the freezing mask? Otherwise, I would consider either masking at a much lower temperature, or foregoing this mask entirely (excluding mountainous regions that likely do have precipitation).
6. Which model is used for the coarse resolution "CMIP6" data?: Lines 200-202 claim that "We analyze simulations from the low-resolution configuration of the Max Planck Institute Earth System Model (MPI-ESM-2-LR) using ICON-ESM version 1.0 as described in Jungclaus et. al. (2022)." My impression was that MPI-ESM1-2-LR and ICON-ESM are two different models, so I don't understand what this means.
7. Why ScenarioMIP?: (lines 194-207) When looking at CMIP6 model output, the authors choose to look at ScenarioMIP experiments. However, they often compare to the Zhao et. al. (2022) paper, which only uses the AMIP simulations. What was the reasoning behind choosing ScenarioMIP rather than AMIP? The varying CO_2 concentrations done by ScenarioMIP could be an additional difference between the coarse and high resolution models that make it difficult to compare them purely based on resolution. Overall, as per my previous suggestions I suggest sticking only to DYAMOND experiments and foregoing the CMIP6 output entirely. However, if this was to remain included, adding information about which scenarios were used in the final 40 ensembles is important, as well as speaking to how surface wind fields differ across various scenario realizations. Also, it would be useful to address the CO_2 concentrations used in the DYAMOND model to compare to ScenarioMIP.
8. Lengthy results section: While it is interesting to see all of the data for each region, for the sake of being concise and clear about the comparison between different regions, many figures could be combined or moved to the appendix. Because this paper is in direct conversation with the Garcia-Carreras (2021) paper, I suggest trying to keep it at a similar length to that paper, maybe 8-10 pages and 4-6 figures. Much of this could be achieved by reducing the length of the results section. Some suggestions I have for how to do this, although I leave it up to the authors:

- (a) Combine all of the regional map figures into one 4 panel figure
 - (b) Structure results by type of analysis / plot rather than by region so that comparisons across regions are clearer. For example, the discussion subsection – "Consistent patterns across regions" seems more like a results section than a discussion section to me.
 - (c) Combine all of the diurnal plots across regions into one figure, taking the average per region and only showing average lines
9. Missing comparison to observations: observations are not really mentioned in this paper. While the coarse and high resolution models are often compared with the suggestion that the high resolution is better at reproducing the wind fields, and I would expect that to be the case, there is no justification or robust comparison to observations that show that the DYAMOND results are accurately computing the dust emission events. For example, I am curious if the DYAMOND results really do accurately represented the prevailing winds mentioned in these regions, or if there are still biases even at higher resolutions.
 10. Missing discussion of future model development suggestions: The point of this analysis seems to be to speak to how convection has to be represented in global climate models to more accurately model dust emissions. Even though the results could be informative for model developers, a formal discussion of specific things to change (or not to change) in the ICON model to improve results would be very interesting.

3 Technical Corrections

1. Reference to an entire model intercomparison project rather than the individual model: There are references throughout the paper, both in descriptions and in figures, to the coarser resolution "CMIP6" models and the higher resolution "DYAMOND" models, even though I believe these results are coming only from the ICON models submitted to those projects. To prevent confusion, I suggest replacing all references of "CMIP6" to "ICON-Coarse" and "DYAMOND" to "ICON-HighRez" or something of the sort, unless if actually referring to the entire multi-model ensemble.
2. Figure color bars: The low-high subfigures, specifically those in in Figs. 2 and 7, are too unsaturated so it is hard to actually see anything. I suggest saturating at lower values.
3. Diurnal figure edit suggestions: The legends in all of the diurnal figures (Figs 3, 5, 8, 12, 17) are on top of the lines and would benefit from being outside of the plot itself. Also, similar to my suggestion above, I suggest reducing the amount of these plots in the paper to only include key results. One way to do this would be to combine data into regional means and put them together in one figure. Another option would be to single out some of the notable regions that have specific prevailing winds, and focus on only showing those diurnal results and explaining them clearly. In general I find these plots really noisy and hard to interpret, so cleaning them up and focusing on only showing the most important data would be great.
4. Spelling of "Taklamakan" on line 424.
5. Some of the regions are missing February or August data. Why?
6. Figure 14 seems like it should have come much earlier as an explanation of how the different models look. I also think it would be fine to just remove it.