

Response to Review 1

October 17, 2025

Review of “Parameterization adaptation needed to unlock the benefits of increased resolution for the ITCZ in ICON”

Manuscript authors: Kroll et al

General

This is my second review of this work, where the authors examine the double intertropical convergence zone (ITCZ) bias in ICON XPP across model resolutions. A turbulent threshold wind parameter is used as a tuning parameter. Increasing the parameter enhances evaporation, which compensates for the weak bias in moisture transport into the deep tropics, but also influences drag and convection invigoration. The paper is much improved and the authors have adequately addressed my comments. I therefore recommend accepting the paper following some minor editorial comments.

We would like to thank Reviewer 1 very much for taking the time to review our manuscript “Parameterization adaption needed to unlock the benefits of increased resolution for the ITCZ in ICON” twice. Their thoughtful and detailed comments were very helpful in improving the first version and enhancing the clarity and accuracy of the manuscript. Below we list the reviewer’s comments and respond to them individually. The reviewer’s comments are shown in black; our response is written in blue ink.

Comments by line number

9 (abstract) “low bias” is ambiguous (it can interpreted as a weak bias). I suggest “dry bias”
We adopted the improved formulation.

15 (abstract) non-discardable
We corrected the typo.

31 why “return” flow? “poleward flow” seems more appropriate
We modified - it now reads “poleward flow” instead of “poleward return flow”.

38 I would add Adam et al. 2018 who explicitly show the energetic contributions to the bias from various factors
Adam et al. 2018 are now included in the discussion: “This underlines that the double-ITCZ problem cannot be investigated as an isolated tropical phenomenon: Sub- and extratropical biases in the energy budget can also be sources of the problem (Kang et al., 2008; Hwang and Frierson, 2013; Adam et al., 2018; Kang et al., 2019), and tropical biases can likewise cause biases in the sub- and extratropics (Henderson et al., 2017; Dong et al., 2022; Feng et al., 2023).”

85 ICON or ICON XPP? Are the results generalizable to other ICON versions?
The results have been tested in different ICON configurations, but as we only investigate ICON-XPP we changed the formulation to: “What are the underlying mechanisms leading to the double-ITCZ and associated large-scale climate biases in ICON XPP and how do the chosen adjustments ameliorate it?”.

101 what do you mean by “real world atmospheric representation”? Do you mean observational data? Yes, observational data/observations are meant. We rephrased to: “For this purpose, we compare the reference observations, the ICON simulation with a double ITCZ, and the ICON simulation with tuning adjustments.”

Figure 1 walker — Walker
We corrected the notation.

145 U — U
We corrected the notation.

188 Here again “high biased” is ambiguous. Do you mean highly biased or too high? We clarified that “too high” was meant. The corresponding sentence now reads: “The ERA5 latent heat flux values are thought to have a positive bias (Martens et al., 2020; Song et al., 2021).”

Eq. 3 No need to specify 0° N and 0° S, you can simply use 0°
We removed the “S” and “N” for 0° .

243 20° N and 20° S
We changed the notation accordingly.

251-252 This explanation is speculative and not convincing. If this were true, we would see a contrast between rising and descending regions (in the lower troposphere), which I don’t see. Another possibility is that energy input into the lower atmosphere is similar in all cases, and therefore it translates to higher temperatures for the drier atmosphere. Showing zonal mean moist static energy biases would help in that regard.

We now follow the reviewer’s formulation and also state the explanation as a hypothesis: “The near-surface warm bias could result from the dry bias in the near-surface layer. If the energy input into the lower atmosphere is similar in the CTL and PTB simulations, the same amount of energy would produce a warmer atmosphere under drier conditions.”. For the sake of brevity, we chose not to include an additional figure.

291 remove comma
We removed the comma. The sentence now reads: “In a next step, we address concerns that increased U_{min} might adversely affect the velocity distribution by increasing the surface drag coefficient on the near-surface velocities and correspondingly slowing down the circulation.”.

Figure 10 caption Inconsistent slanting of variables
We corrected the inconsistent slanting of variables.

559-561 I think this is a critical point worth elaborating on. Specifically, beyond the surface heat fluxes examined in this work, changes in surface wind stress critically affect ocean dynamics and vertical structure (e.g., the depth of the mixed layer) which would then influence the climate system through multiple mechanisms.

We agree that the U_{min} fix would most likely have even more pronounced negative effects in a coupled setup. Following the reviewer’s suggestion we added the following formulation: “In a coupled climate simulation, changes in U_{min} could also critically influence ocean dynamics and vertical structure (e.g., lead to substantial cooling), potentially introducing severe large-scale biases.”

Response to Review 2

October 17, 2025

Review of “Parameterization adaption needed to unlock the benefits of increased resolution for the ITCZ in ICON”

Manuscript authors: Kroll et al

We would like to thank Reviewer 2 very much for reviewing our manuscript “Parameterization adaption needed to unlock the benefits of increased resolution for the ITCZ in ICON”. Below we list the reviewer’s comments and respond to them individually. The reviewer’s comments are shown in black; our response is written in blue ink.

This study investigated the double-ITCZ bias in the AMIP simulations of ICON at different horizontal resolutions and explored the impacts of the minimum gustiness parameter. They showed that the double-ITCZ bias still exists with 5km horizontal resolution. They found that while increasing U_{min} over ocean reduces the tropical precipitation bias to some extent, it has some adverse impacts on the large-scale circulation and evaporation and does not fix the biases in humidity and its source.

While the manuscript presents a lot of results which are worthwhile to be published, the manuscript is not clearly structured (partly because there are too many results) and the writing is sometimes too casual for publication standard. Also, I feel the manuscript lacks a clear focus. I would recommend a major revision so the authors can improve their presentation and sort out their focus.

All authors have reread the manuscript, with particular attention to shortening the text. In this process, we also removed phrasings that appeared too casual, while acknowledging that some choices of phrasing may reflect stylistic preferences. Regarding the focus of the manuscript, we worked on making the structure even clearer. A key change in this regard was to align the research questions directly with the results sections. Previously the research questions were answered over multiple section, which made it far harder for the reader to follow.

There are a lot of experiments with different values in the perturbing parameters. While I understand the workflow, I think it will be much clearer if the authors can focus on just one or two experiments. For example, they may focus on PTB-5_1 and remove the results of PTB-5. It makes sense to me to keep $U_{min} = 1\text{m/s}$ over land.

We agree that the paper contains a number of experiments and analyses. In the previous review round, this was explicitly requested (i.e., an additional 5 km simulation and analysis for Fig. 7 and 9). To accommodate these, we already discarded showing most retuning experiments after the wind speed limiter adjustments (PTB-5_1t and PTB-6_1t) in the last revision cycle. We agree that—should one decide to use the U_{min} approach—the PTB-5_1 experiment would be superior to PTB-5. However, as [SBF⁺24] used a uniform U_{min} , we think it is important to point out that using separate values over land and ocean yields better results and is physically motivated. Therefore, we see a clear motivation to keep some of the discussion about PTB-5 versus PTB-5_1. To reduce the amount of data presented, we now only show the PTB-5_1 results where they are absolutely needed to make this statement (precipitation, wind fields, and circulation), while we omit PTB-5_1 in the figures for latent heat flux and residence time.

Line by line comments

Figures: for unit in the colorbar, one may use “[]” instead of “/” which often means “divided by”. Both “[]” and “/” are commonly used in the literature. As both notations are acceptable, we would

prefer to retain our current style.

Figure 3,4: Are these biases in humidity and temperature shared by CMIP models?

Yes, similar biases exist in CMIP models. We have now added a corresponding comment in the discussion: “Again, similar temperature and humidity biases can be found in the CMIP models ([TFK⁺13, TFT24]), suggesting that the described bias in moisture export from the subtropical boundary layer is not unique to the ICON model.”

Figure 5: Since the different treatment for land and ocean, why not show the RMSE for land and ocean separately?

Following the reviewer’s comment above, we are trying to reduce the complexity and amount of data shown, specifically with respect to PTB-5.1 and PTB-5. We therefore concentrate on optimizing the global RMSE rather than two separate quantities.

Figure 5: better to move the box of RMSE and mean error out of the figure.

We tested moving the RMSE and mean error out of the figure (cf. Fig. 1), but decided to keep the former layout as it appears less busy while still showing the relevant tropical precipitation data.

For 2d spatial map, is it necessary to show the global map? Maybe focus on the tropical region since the paper is about tropical precipitation bias.

We would like to keep the global maps, as the U_{min} threshold is applied globally, and the discussed consequences also arise in the sub- and extra tropics, as seen, for example, in the newly introduced near-surface wind speed biases over extra tropical land. We however now removed more of the white space between the individual plots, which allowed saving space to display more details.

Figure 11: Is it necessary to rotate it?

We removed the rotation.

Line 204: in the origin of

We suspect that Line 304 was meant and corrected accordingly: “Section 3.2.3 explores how U_{min} leads to an unintended shift in the origin of the inner tropical moisture.”

Line 509: Here, it is more related to the double-ITCZ bias in AMIP (instead of CMIP), which exists without the bias in SST.

We now make a clear distinction between our AMIP-type setup, possible reasons for the bias therein, and then compare it to the CMIP setups: “The precipitation biases in our ICON resolution hierarchy arise from both atmospheric contributions and evaporation biases linked to a mismatch between prescribed SSTs and atmospheric conditions. Although our model setup is AMIP-type, it also expresses a double-ITCZ bias as seen across several CMIP generations ([TFT24]).”.

Line 512: It is better to avoid using two “focus on”.

We reformulated the sentence to: “To address the biases, we concentrated on the parameterizations that remain active at the km-scale, focusing on U_{min} in the bulk flux formulation of the turbulence scheme.”

Given the results presented here, what are the implications for the double-ITCZ bias in AMIP-style simulations (in the CMIP project).

We added our suggestion for ICON and AMIP-style simulations in the CMIP project in the Discussion: “The results of this study suggest that a physically motivated correction of the double-ITCZ bias in ICON (and potentially other AMIP-style CMIP simulations) should focus on reducing exaggerated vertical moisture fluxes in the subtropics.”

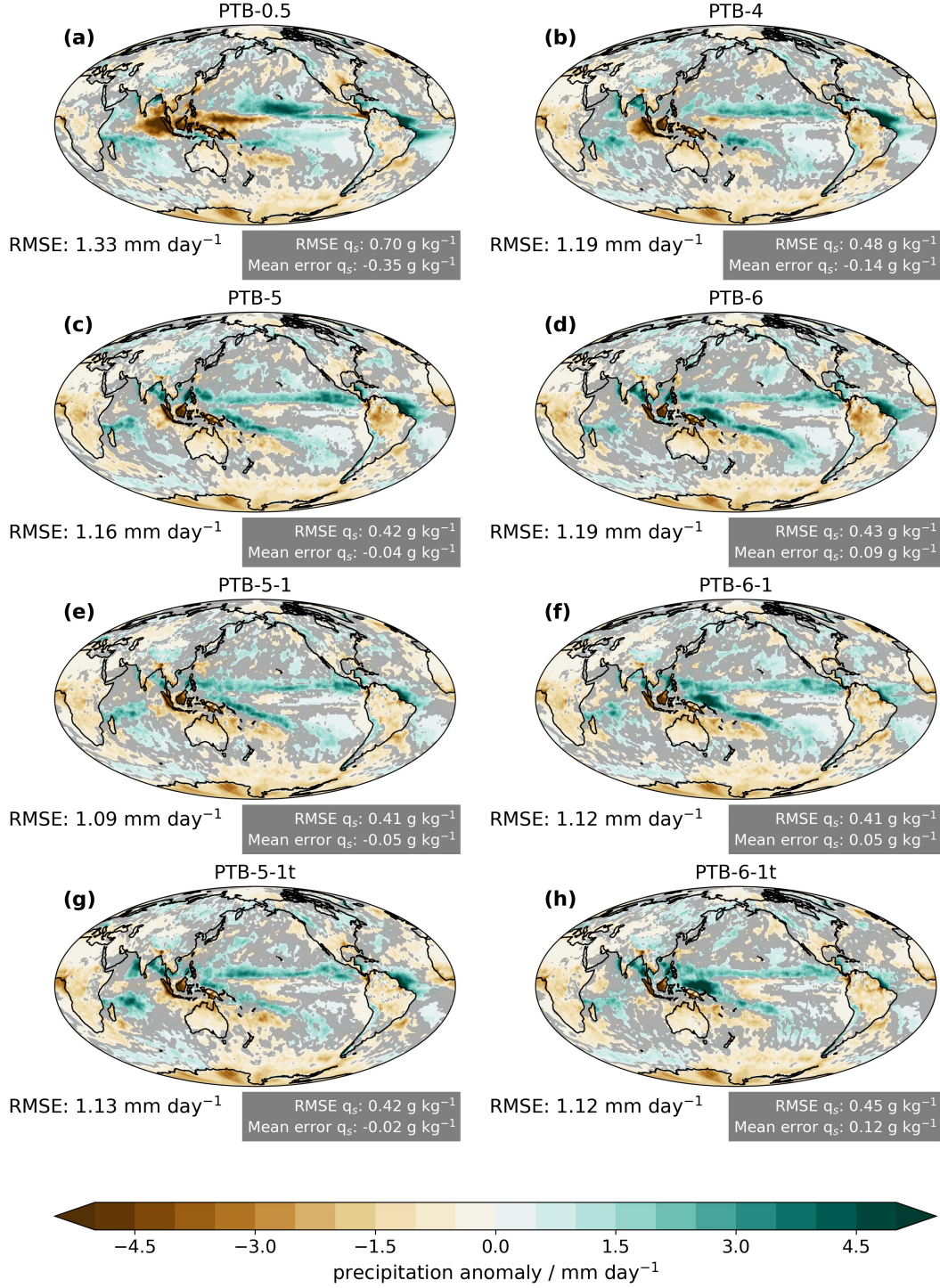


Figure 1: Two-year mean large-scale precipitation bias with respect to IMERG averaged over 2004-2010 for different settings of the surface wind at 40 km resolution: PTB-0.5 (a), PTB-4 (b), PTB-5 (c), PTB-5.1 (e), PTB-5.1t (g), PTB-6 (d), PTB-6.1 (f) and PTB-6.1t (h). The corresponding global precipitation RMSE is stated beneath the panel for each sensitivity experiment. Statistically insignificant differences between IMERG and the experiments based on a two-sided z-test at $\alpha = 0.1$ are grayed out. The global RMSE and mean error in near-surface specific humidity, calculated with respect to values derived from ERA5 reanalysis, is depicted in the inlays.

References

- [SBF⁺24] Hans Segura, Clara Bayley, Romain Fiévet, Helene Gloeckner, Moritz Guenther, Lukas Kluft, Ann Kristin Naumann, Sebastian Ortega, Divya Sri Praturi, Marius Rixen, Hauke Schmidt, Winkler Marius, Cathy Hohenegger, and Stevens Bjorn. A single tropical rainbelt in global storm-resolving models: the role of surface heat fluxes over the warm pool, 2024.
- [TFK⁺13] Baijun Tian, Eric J. Fetzer, Brian H. Kahn, Joao Teixeira, Evan Manning, and Thomas Hearty. Evaluating CMIP5 models using AIRS tropospheric air temperature and specific humidity climatology. Journal of Geophysical Research: Atmospheres, 118(1):114–134, January 2013.
- [TFT24] Baijun Tian, Eric J. Fetzer, and Joao Teixeira. Assessing the Tropospheric Temperature and Humidity Simulations in CMIP3/5/6 Models Using the AIRS Obs4MIPs V2.1 Data. Journal of Geophysical Research: Atmospheres, 129(15):e2023JD040536, August 2024.