Response to Review 1

August 8, 2025

Response to Review

General

The authors examine the double intertropical convergence zone (IITCZ) bias in the ICON model across model resolutions. They find some improvement in the bias by controlling a critical velocity criteria of the turbulent scheme, which is offset by other emergent biases. But the overall conclusion is that increasing the model resolution, which allows discarding parameterizations at resolved scales, does not alleviate the IITCZ bias. On the one hand, I support the approach of the work, given both the importance of the IITCZ bias and the increased prevalence of climate models with higher or variable grid resolutions. On the other hand, I see some critical problems with both the writing and the analysis by the authors. Overall, I would recommend publishing this work following a major and significant revision.

We would like to thank Reviewer 1 for their detailed and constructive comments to our manuscript "Parameterization adaption needed to unlock the benefits of increased resolution for the ITCZ in ICON".

We address their suggestions by supporting our findings with additional analysis of the moisture budget (residence times in boundary layer) and the general circulation, refine the methods description and reformulate sentences for clarity. 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.

General comments

1. It is well known that the IITCZ bias is generally a property of coupled climate models, though some indications for processes leading the the bias are present in atmospheric models. It is therefore not surprising that the changes in the parameterizations and resolutions do not remedy the bias.

   Yes, the study uses an atmosphere model of the ICON XPP configuration [MFK+25]. The double ITCZ bias is present in both the coupled- and atmospheric-only model version, with a reduced magnitude in AMIP experiments. In our analysis, we prescribe sea surface temperatures, which allows us to isolate and clearly identify the atmospheric contribution to the bias, independent of a biased sea surface temperature or surface fluxes of a coupled model.

   The desire to address long-standing biases in climate models has motivated several researchers to increase model resolution (e.g. more recently, [MCCLT+22, MZZW23]). The existence of differing views on the effects of increased resolution or discard of parameterizations underscores the need to systematically investigate how increased resolution or the reduction of parameterizations affects model skill. Our study therefore aims to make a contribution to the ongoing discussion of how best to capitalize on the benefits of increased resolution, focusing on the double - ITCZ bias.

   More importantly, the authors use an atmospheric model (NWP). What are the surface boundary conditions? Clearly these would be important in diagnosing the IITCZ bias in the control runs, both in terms of the data being used, and in terms of the processes controlling surface heat fluxes. But there's no mention of this.

   Necessary boundary conditions like sea surface temperature (SST) and sea ice concentration (SIC) are prescribed based on 6H data interpolated from a monthly climatology. The CMIP6 forcing dataset from 1978-2020 was used to create these climatologies. By using the monthly climatology in SST, the influence of the interannual variation of ENSO on precipitation variability is removed. This was mentioned in the previous version, but to make this clearer, we substantially refined the model and experiment section in the revised manuscript. In the course of this restructuring, we moved the description of the boundary condition to a more prominent position. It states: "In all experiments, we use prescribed 6-hourly climatological sea ice and SST fields interpolated from the monthly climatological values of the CMIP6 Forcing Datasets (input4MIPs, 1978-2020) as boundary conditions [DT18]. Prescribed climatological SSTs reduce the impact of interannual variability, such as the influence of ENSO events on precipitation. In addition, they separate the effect of model biases in the SST representation and atmospheric processes."

2. The work is riddled with inaccuracies, unclear statements, esoteric references, and Yoda-like sentences (e.g., 169–171). Lines 214 and 439 have referencing errors which should have been picked up in a reasonable proofing of the text. I urge the authors to do a better editorial job in this paper, which at its present form gives the impression of lack of attention to detail.
   The specific sentence in question has been revised to: "The ERA5 latent heat flux values are high biased [MSW+20, SND+21]. Therefore, we also compare our data to the OAFlux dataset [SDFT13, NCfARS22], which integrates satellite retrievals and three atmospheric reanalysis.". We have also corrected the references in lines 214 and 439. In order to address the general concern with respect to language and reading flow, we additionally carefully proofread the manuscript. To improve clarity and readability, we reformulated complicated passages of the manuscript. During this process, we also streamlined several sections of the text. For example, the revised manuscript now presents only the full set of results for PTB5 and shows the PTB6 in the summary tuning plot. In this way, the key results are more clearly highlighted and the readability is improved. For further examples of how we addressed language-related comments, please refer to our detailed responses under "Comments by line number" or the tracked-changed manuscript.

3. The scope of the analysis is limited.
   In order to address the reviewer's concern we integrated additional analysis focusing on the general circulation and moisture transport to support our argumentation chain. Specifically, as detailed below we investigate the Walker and Hadley circulation strength with a metric based on the velocity potential and perform a moisture budget analysis to investigate the strong vertical moisture transport out of the boundary layer based on residence times within the boundary layer.

   This (the limited scope of analysis) in itself is fine, but needs to be acknowledged. Specifically, some of the hypothesized processes are discussed with no support, and are therefore speculative. In addition, a single parameter (Umin) is used as the control parameter. The strong response of the climate system to this single parameter demonstrates how complicated the task of bias reduction is, given the numerous other potential tuning parameters. Any general discussion of the 'root' cause of the IITCZ bias (the systematic variation of the resolution not withstanding) therefore in my opinion exaggerates the scope and implications of the study.
   It is our goal is to provide a clear and focused step toward understanding and mitigating the double - ITCZ bias by isolating and analyzing the impact of one specific parameter. The decision to focus on "Umin" in this study is based on two main considerations. First, a recent study [SBF+25] proposed "Umin" as a solution to the precipitation bias in the Warm Pool. Our study complements their work by exploring the underlying global mechanism leading to an improvement in our ICON model, which employs the full suite of parametrizations. Second, a comprehensive analysis of the full spectrum of tuning parameters and their underlying mechanisms is beyond the scope of a single study. Gaining insight into why "Umin" helps reduce the double - ITCZ bias in ICON can ultimately inform more effective solutions with improved cost-benefit profiles, because, importantly, we do not advocate for the use of "Umin" due to its negative effects on atmospheric circulation, which became evident only through this parameter-specific investigation. To address the reviewer's concern regarding the discussion of root causes, we have carefully revised the manuscript to more clearly articulate the scope and limitations of our analysis. Specifically, we replaced the term "root cause" in the abstract with the phrase "a key driver".
   My two recommendations in this regard is to either temper the speculative discussion of processes

(see comments below) or provide more rigorous analysis (for example, the Seager decomposition may be helpful in the analysis of moisture transport).

We have addressed the reviewer's concerns regarding the discussion of results, as detailed in our responses to the individual comments below. In addition, we have substantially revised the manuscript. To further strengthen the analysis, we incorporated a more rigorous statistical evaluation and expanded the overall analysis. Specifically, in response to the concerns about moisture transport pathways, we now include additional diagnostics of the moisture budget (based on [PO83, PO92], but focusing on climatological residence times in the boundary layer). Additionally, we analyze the strength of the Hadley and Walker circulations with a velocity potential based metric. The entire additional analysis is shown and discussed in the manuscript (Section 2.4, 2.5, 3.2.2, 3.24). Here we show two example results. First, the velocity potential, which we used to diagnose the Walker and Hadley Circulation strength. The circulation indexes for the circulation strength introduced by Tanaka et al., 2004 [TIK04] demonstrate that in CTL the Walker Circulation is too weak but improves in PTB-5. On the other hand, the Hadley Circulation strength is reduced in PTB-5 compared to CTL (compare Table 1). This backs up the hypothesis voiced in the previous version of the manuscript. The spatial maps of the velocity potential bias with respect to ERA5 can be seen in Figure 1.

Second, we show the difference between the inverse residence time within the boundary layer up to 850 hPa compared to ERA5, which we compute based on a moisture budget analysis. Figure 2 shows that the residence time in the ICON simulations is shorter than in the ERA5 reference, i.e. due to too fast vertical export out of the boundary layer, a bias which is not corrected by the wind speed limiter fix. Further details and interpretation are given in the revised manuscript.

Table 1: Walker and Hadley circulation strengths for ERA5 as well as CTL, PTB-5 and PTB-5_1. Indices are calculated following the approach by Tanaka et al. (2004).

Experiment	**Walker circulation Strength** $\chi*_{max}$ / 10^7 m^2 s^{-1}	**Hadley circulation Strength** χ_{max} / 10^6 m^2 s^{-1}
ERA5	1.16	3.23
CTL	0.94	2.17
PTB-5	1.27	1.98
PTB-5_1	1.22	1.98

Comments by line number

1 (abstract) The double - ITCZ (IITCZ) is itself not a precipitation bias. The "IITCZ bias" is a prominent tropical precipitation bias.

The respective sentence now reads: "The double Inter-Tropical Convergence Zone (double-ITCZ) bias is a persistent tropical precipitation bias over many climate model generations."

8 The 'root' cause only in the context the atmospheric model used here. Clearly, given that the IITCZ bias is a coupled model problem, and given the numerous mechanisms proposed as the cause of the IITCZ bias (e.g., cloud albedo, trickle bias, surface wind bias, etc.) the present work does not diagnose the actual 'root' cause.

The double - ITCZ is not a feature exclusively simulated by coupled models - this work (among many others) shows that the double - ITCZ bias can also occur in uncoupled model simulations. It is of course correct that the manuscript exclusively concentrates on the ICON model in an uncoupled setup; the statement with respect to the origin of the double-ITCZ bias is therefore restricted to this setup as well. We now emphasize this even more throughout the work. For example, the title of the work specifically states that the study is focusing on the ICON model, i.e. "Parameterization adaption needed to unlock the benefits of increased resolution for the ITCZ in ICON". In the abstract, it is stated that the work investigates the double-ITCZ in the ICON model, i.e. "In this work, we study the double-ITCZ bias in an ICON XPP resolution hierarchy spanning from parameterized to explicitly described deep convection within a consistent framework.", before explaining the chain of biases leading to the expression of the double-ITCZ in this model. In order to address the concern of the

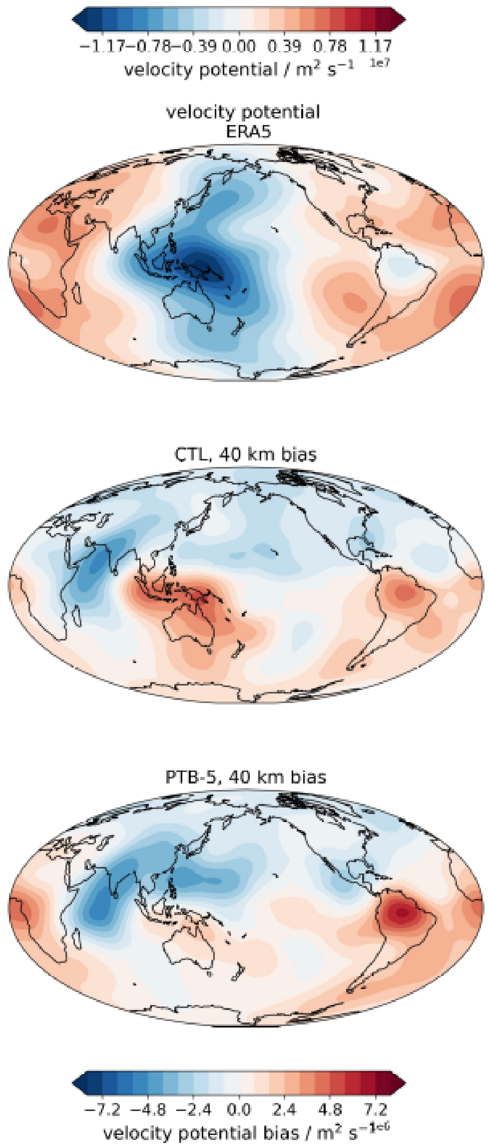

Figure 1: ERA5 multi-year average velocity potential χ at 200 hPa for the years 2004-2010. Two-year mean bias of the CTL, and PTB-5 experiment with respect to the ERA5 velocity potential. Negative values of the velocity potential are found in regions of ascent and divergent motion; positive values in region of subsidence and convergence.

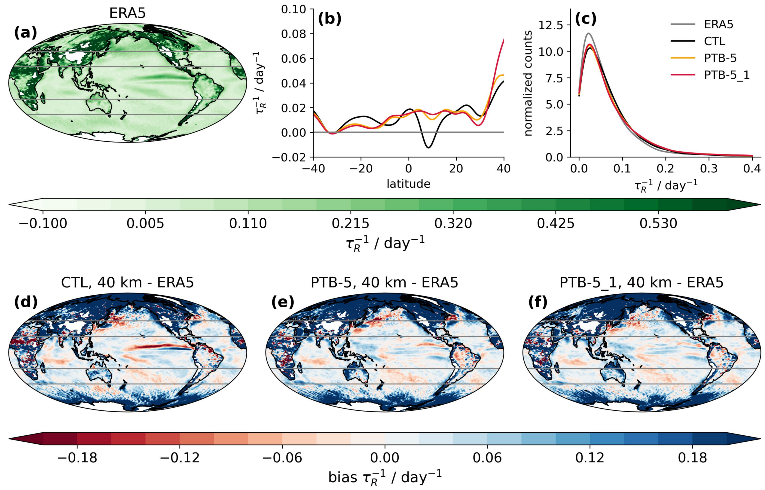

Figure 2: Inverse residence time $\tau_{R_{bl}}$ within the boundary layer up to 850 hPa: (a) ERA5 for the reference years 2004-2010, and differences of two-year average of CTL (d), PTB-5 (e) and PTB-5_1 (f) compared to ERA5. The gray lines mark the 40 and 20 latitude. (b) Difference of zonal distribution of τ_R of CTL, PTB-5 and PTB-5_1 to ERA5 within the latitude band between 40 S and 40 N, restricted to ocean only. (c) Probability density function of τ_R of ERA5, CTL, PTB-5 and PTB-5_1 within the latitude band between 40 S and 40 N, restricted to ocean only.

definitiveness in the statement in the abstract, we reformulated as: "However, we highlight that a key driver of the double-ITCZ bias in ICON seems to lie in the insufficient moisture transport from the subtropics to the inner tropics."

9 biased how? Without specifying this, the following sentence is hard to interpret.
We modified the corresponding sentences to read: "However, we highlight that a key driver of the double-ITCZ bias in ICON seems to lie in the insufficient moisture transport from the subtropics to the inner tropics. The resulting low bias in tropical near-surface moisture reduces deep convection over the Warm Pool, leading to a weakened Walker circulation. These biases ultimately culminate in the double-ITCZ feature."

11 what do you mean by 'addresses'?
We rephrased the corresponding sentence. It now states: "Increasing the near-surface wind speed limiter improves tropical near-surface moisture but exacerbates the bias in the moisture source, increasing the inner tropical contribution at the expense of the subtropics."

12 subtropical contribution to what?
We clarified that the entire sentence is focusing on "moisture" by adding "to near-surface moisture". The revised sentence now reads: "An increase in near-surface wind speed limiter resolves the low bias in near-surface moisture in the tropics, however, it exacerbates a bias in the moisture source by increasing the inner tropical over the subtropical contribution to near-surface moisture."

13 what do you mean by endanger?
We replaced the word "endanger" with the word "degrade".

21 CMIP␣(Tian and Dong, 2020) — similar missing space in many other places in the text.
We added in spaces where they were missing.

22 "tendency to overestimate precipitation over ocean in the southern tropics and underestimate it at the equator" is inaccurate, unless used to describe the zonal mean precipitation. The IITCZ bias includes positive precipitation biases south of the equator in the eastern Pacific and Atlantic, underestimated precipitation in the equatorial Pacific, and positive precipitation biases in the western tropical

Pacific.
We deleted the original formulation and followed the reviewer's suggestion. The text now reads: "Among them, the double-ITCZ bias is the most prominent problem [MRB+95, Lin07]. It describes positive precipitation biases south of the equator in the eastern Pacific and Atlantic, as well as underestimated precipitation in the equatorial Pacific."

23 please provide a citation in reference to the prominent problem.
We now provide three citations for the double-ITCZ from the time span of 1995 to 2020: "Despite its importance, biases in the representation of precipitation within the Inter-Tropical Convergence Zone (ITCZ) have been a persistent challenge throughout many model generations in the Coupled Model Intercomparison Project (CMIP) (Tian and Dong, 2020). Among them, the double-ITCZ bias is the most prominent problem (Mechoso et al., 1995; Lin, 2007)."

31 increased wind convergence where?
The corresponding sentence was deleted during the restructuring of the manuscript.

36 caused ?by? moisture
The corresponding sentence was deleted during restructuring.

42 a more relevant reference in this context would be Marshall et al. (2014, "The ocean's role in setting the mean position of the ITCZ")
We added the corresponding reference.

46 &48 Not necessarily subtropical, it could be from any region outside the tropics.
We now state "sub- and extra-tropical" instead of "subtropical" to account for this. The sentence now reads: "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 [KHFZ08, HF13, KHX+19], and tropical biases can likewise cause biases in the sub- and extratropics [HMS17, DABBW22, FDW+23]."

80 The leading questions are themselves composed of questions. 1. Is actually three questions, and 2 &3 are two questions each.
We reformulated the corresponding section. Specifically, we added "topic headings" for the research questions. The corresponding text now reads: "We focus on the following questions:

1. **Resolution and parameterization dependence of the double-ITCZ bias**: Can increased horizontal resolution and switching off deep convective and gravity wave parameterization improve the double-ITCZ bias? Are there common biases across resolutions? Where can resolution-dependent improvements be found?
(*Addressed in Section 3.1 and 3.3*)

2. **Resolution-(in)dependent bias corrections:** To the extent that there are common (double-ITCZ) biases, how can they be addressed and can the same adjustments be applied at various resolutions?
(*Addressed in Section 3.2 and 3.3*)

3. **Underlying mechanisms:** What are the underlying mechanisms leading to the double-ITCZ bias in ICON and how do the chosen adjustments ameliorate it?
(*Addressed in Section 3 and Section 4, summarized in Schematic Figure 1).*"

86 please explain what is the bulk-flux formulation.
We added an entirely new section (Section 2.1.2, subheading "Default and U_{min} adapted ICON") which explains the bulk flux formulation in the context of the U_{min} changes in detail and at one place. In the introduction, we now refer to this new section.

87 is undefined
We added a detailed description of U_{min} in a separate section, please see comment above.

104 please specify what is the schematic. Figure 13?
Yes, Figure 13 was meant. Within the restructuring process we decided to move this Figure to the introduction and reference it there.

115 Given that this is an atmospheric model, how are ocean-atmosphere interactions represented? What are the surface boundary conditions? (prescribed SST, q-fluxes, etc.)
The model and experiment section was substantially revised to account for this comment and comments by reviewer 2. Addressing this comment, the discussion of the surface boundary condition was moved to a more prominent location in the text. It can now be found in the model and not the experiment section. The revised statement reads: "In all experiments, we use prescribed 6-hourly climatological sea ice and SST fields interpolated from the monthly climatological values of the CMIP6 Forcing Datasets (input4MIPs, 1978-2020) as boundary conditions [DT18]. Prescribed climatological SSTs reduce the impact of interannual variability, such as the influence of ENSO events on precipitation. In addition, they separate the effect of model biases in the SST representation and atmospheric processes."

Section 2.3 A_p was defined by Hwang and Frierson (2013) and E_p was defined by Adam et al. (2016). Please reference the indices accordingly.
For the sake of readability, we left this sentence as is. It is already clear that these indices come collectively from these two papers.

Figure 1 $kg^{-}1$ to kg^{-1} (also in all of the other figures)
We corrected the typo in the respective figures.

Figure 1 … near-surface specific humidity, calculated with respect to values derived from ERA5 reanalysis, is …
We added the commas correspondingly.

Please refer to panel letters in the caption
We now refer to the panels in all captions.

203 ?resp.?
"respectively" is now spelled out.

Figure 2 and elsewhere, it would be better to describe units in square brackets, rather than following a divider, e.g., height / km — ¿ Height [km]
As the "[]" notation is not required by the ACP journals and the "/" notation is the more precise mathematical representation, we would like to refrain from changing the handling of the units. ACP journals require exponential writing of units, i.e. $W\ m^{-2}$, we therefore see no danger of confusion caused by multiple "/"s.

214 citation error
We added the missing citation.

216,220 and elsewhere, citet to citep
We made the corresponding changes.

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Response to Review 2

August 8, 2025

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

Manuscript authors: Kroll et al

Summary

The authors use simulations at different resolutions and with the gustiness factor perturbed in the ICON model to examine mean precip biases. They argue that the double ITCZ bias persists at all resolutions but that increasing the minimum wind speed for the evaporation improves this, which however does not fix the larger underlying humidity biases across the tropics and subtropics. There are some results of interest here, but I had a hard time following this manuscript and left it not especially convinced. In part this stems from issues with the writing, in part from the experimental design, and in part from the lack of uncertainty quantification. Addressing all this adequately would take a serious amount of work, and as such I recommend rejection for eventual resubmission by the authors.

We thank the second reviewer for all their detailed and constructive comments. In response to the reviewer's main comments, we (1) include new high-resolution simulations with the full suite of parameterizations and discuss (in part of Section 3.3) how they compare to the setup with reduced parameterizations, (2) display statistical significance of the results, and (3) substantially revise and streamline the text. In subsequent paragraphs, we respond to the reviewer's detailed comments. The reviewer's comments are listed in black, and our response is listed in blue.

Major comments

Experimental design

It would be much cleaner if the highest resolution run didn't also have the convection and gravity wave schemes disabled as well, c.f. L120-122. As it stands, going from the 40km model to the 5km model, you're both increasing resolution and changing the model formulation. And even a 5km grid is surely not fine enough to resolve the large number of convective updrafts that are smaller than 5x5km. A relevant paper here is Clark et al. (2024), and references therein. In the GFDL AM4 model in an aquaplanet context, at high resolution the model behavior still changes dramatically depending on whether the convective parameterization is enabled or not. This is especially concerning given that some fields such as the humidity and temperature biases are nonmonotonic in resolution, with the change occurring going from the 40km version in which the deep convective parameterization is activated to the 5km version in which they are disabled.
We agree with the reviewer that an additional experiment in 5 km with a deep convective and gravity wave drag parametrization would benefit the study. We ran the corresponding experiment, the evaluation of which is now integrated in the manuscript. It replaces the original 5 km run without parameterization. We dedicate part of the results section 3.3 to a comparison of the 5 km setup with and without the deep convective and gravity wave parametrization. The introduction and discussion also includes a reference of Clark et al. 2024 [CLH24]: "In this context, it is important to note that

even at a horizontal resolution of 5 km, the necessity of deep convective parameterizations is still disputed, with some studies showing improvements in atmospheric representation with the elimination of parameterizations [VTBP+20] and others showing deterioration and insufficiently resolved processes [CLH24].".
Indeed, the atmospheric representation is improved with the parameterizations; however, the double - ITCZ persists in both setups. In Figure R 1, we exemplarily show the temperature fields for the 40 km setup, the 5 km simulation including all parameterizations and the 5 km simulations with reduced parameterizations (rParam):

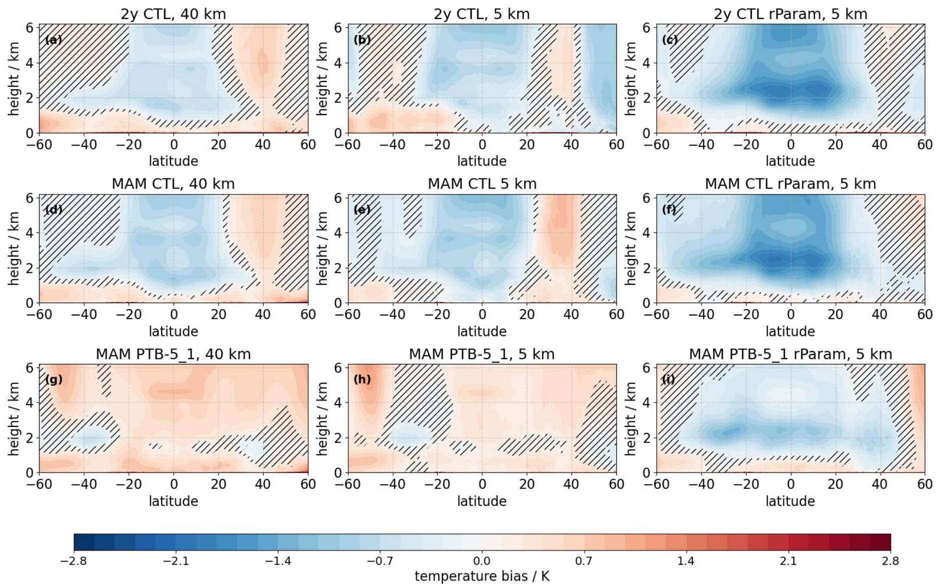

Figure 1: Zonal mean temperature bias with respect to ERA5: all CTL for a 2 year average (a-c), all CTL for an average over two MAM seasons (d-f) and all PTB-5_1 for an average over two MAM seasons (g-i) for 40 km, 5 km and 5 km, rParam. Statistically significant differences between ERA and the model data are shown, insignificant regions are hatched. Statistical significance was tested with a two-sided ztest at $\alpha = 0.1$ after autocorrelation correction.

Uncertainty quantification

C.f. L250-254, The 4, 5, and 6 experiments precip RMSE values are all within 0.03 of each other. Is that even a statistically significant separation? I worry about sampling uncertainty given the short durations of the runs. I have the same concern about other results; apart from Fig. A1 there is very little discussion of uncertainty quantification and its implications for interpreting the results. It seems plausible that you're over-interpreting differences across simulations that aren't statistically well separated.
We have expanded the uncertainty quantification and marked statistically significant differences in all precipitation, temperature, and specific humidity figures. With respect to the differences in the precipitation fields: The reference is CTL or ERA5 depending on the respective quantities shown. For our research questions, the statistically significant difference between CTL and $U_{min} \in \{4,5,6\}$ is the important quantity, not the difference between $U_{min} \in \{4,5,6\}$. We can clearly show that the difference in precipitation between CTL and $U_{min} \in 4,5,6$ is statistically significant in the revised manuscript version. Also, there is a clear trend in the various tuning experiments. This shows that the change in the respective tuning parameter leads to systematic changes in the precipitation distribution and latent heat flux. In addition, we can explain the changes in model behavior based on physical arguments, again raising confidence in the results. We have chosen $U_{min} = 5ms^{-1}$ as the basis for the 5 km experiments. After demonstrating that U_{min} can significantly alter the representation of the ITCZ compared to IMERG, we decided to invest our computation time in the requested simulation with parametrization rather than increasing the runtime of the $U_{min} \in \{4,5,6\}$.

Figure R 2 shows an example on how we are now restricting the analysis to statistically significant differences only.

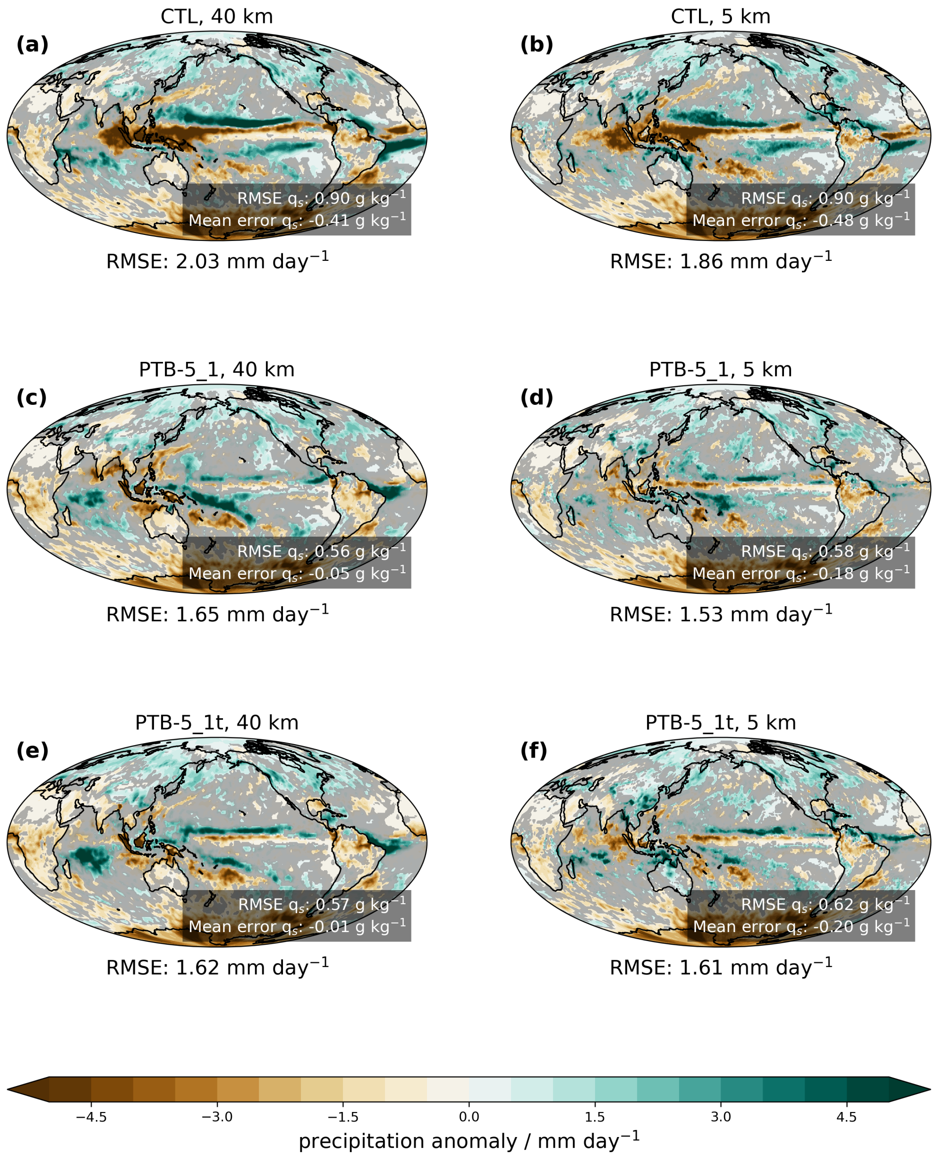

Figure 2: Two-year mean large-scale precipitation bias with respect to IMERG 2004-2010 for different settings of the surface wind at 40 km resolution: PTB-0.5 (a), PTB-4 (b), PTB-5 (c), PTB-5_1 (e), PTB-5_1t (g), PTB-6 (d), PTB-6_1 (f) and PTB-6_1t (h). The corresponding global precipitation RMSE is 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 shown, and insignificant regions 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.

Experiment names

The results would be much easier to follow if the experiments had more descriptive names. So, instead of "R2B4," call it for example "160km." The "PTBX" simulation names are similarly unintuitive.
We discarded of the ICON-specific language and only refer to the nominal resolution and experiment type, i.e. "CTL, 160 km" or "PTB-5, 160 km" to make the text easier to follow. PTB stands for "perturbed", and the numbers refer to the wind speed limiter values. As the value of the wind speed limiter is important for context, we decided to keep this part of the nomenclature.

Unclear arguments regarding model tuning

I find the discussion of tuning peppered throughout the introduction to be frustrating. It feels speculative and almost conspiratorial, seeming to imply that the modeling groups are somehow, in the case of focusing on global mean TOA radiative fluxes rather than regional circulation fidelity, doing something obviously wrong.
We want to ensure the reviewer that there is definitely no intention to accuse climate modelers of doing something wrong, and some of the authors of this manuscript are modelers themselves. We are fully aware that the TOA tuning is followed by process tuning, and we intended only to highlight the potential trade-offs between TOA tuning and process tuning, where improving one might degrade the other. We modified the corresponding sentence to make this clearer. In this context, we also added references to the studies from GFDL. It now reads: "Most model evaluation workflows focus first on global mean top-of-the-atmosphere (TOA) fluxes and potentially surface energy fluxes [MSR+12], and then proceed to tune additional atmospheric fields and processes, for example, as outlined in [HMG+17]. This is done with the knowledge that there can be trade-offs between these different optimizations, making dedicated approaches to optimize across multiple processes necessary, for example to focus on the precipitation distribution and TOA fluxes within one framework [ZGH+18a, ZGH+18b]." Adding a caveat that model tuning/process tuning is necessary should not disregard the work of modelers. In contrast, we wish to promote their work, which is unfortunately sometimes not appreciated enough. The following sentences underline that it would be very valuable for the community to allow for more publications documenting model improvements: "However, in general, most published model evaluation workflows provide little information on regional energy budgets that are mechanistically important for the large-scale circulation and precipitation distribution. This poses a problem: Sometimes improvements in global mean energy fluxes introduce compensating errors in regional energy fluxes and lead to deterioration of the large-scale circulation and precipitation distribution."

In the discussion of tuning high resolution models, L65-67, "misconception that all relevant processes are now resolved" is not justifiable, nor is it appropriate in tone. Model developers are fully aware of the physical scales of the various processes involved and how those compare to the scales resolved by their model.
We agree with the reviewer that the model developers are fully aware that not all relevant processes are resolved. However, the authors have seen cases where this is not fully acknowledged, for example in proposals for additional high-resolution modeling efforts. We softened the sentence, and it now reads: "High computational costs, in combination with the hope that the most relevant processes are now resolved, can then lead to a shortening of the model tuning process in high-resolution simulations."

The paper should incorporate Zhao et al. (2018a,b), who discuss how a tuning strategy targeting TOA fluxes was used to improve ITCZ simulation.
As stated above, the requested citations were integrated in the introduction to showcase a tuning strategy targeting the double-TCZ: "This is done with the knowledge that there can be trade-offs between these different optimizations, making dedicated approaches to optimize across multiple processes necessary, for example to focus on the precipitation distribution and TOA fluxes within one framework [ZGH+18a, ZGH+18b].".

English writing

My impression is that English is not the lead author's native language for writing. There are quite a lot of sentences where the grammar and/or word choice are difficult to follow. In aggregate, these make for a somewhat jarring reading. At least one of the coauthors is a native English speaker, and so I know it is within the authors' collective ability to, in the revision, significantly tighten up the English writing. Here is a nonexhaustive list of sentences that I struggled with:

- L36-37: The corresponding sentence was deleted during restructuring.
- L49: We reformulated to: "It is critical to recognize the coupling between the double-ITCZ problem and biases in the energy budget also in the context of model tuning, because the first step of model evaluation is in many cases the energy balance of the model [Wil20]."

- L111-114: We split this sentence in two. It now says: "ICON XPP uses parameterizations for radiation [HB18], cloud microphysics [Sei08], vertical diffusion [MSZ+07], convection [Tie89, BKJ+08], subgrid scale orographic drag [LM97] and non-orographic gravity wave drag [OBS+10]. The atmosphere is coupled to the land model JSBACH [RGG+21]."
- L132-134: The corresponding sentence was deleted during the restructuring of the manuscript.
- L134-135: This sentence was deleted.
- L138-141: The sentence was split into four for simplification: "The two U_{min} settings that perform the best in representing the large-scale annual-mean precipitation, PTB-5 and PTB-6, with $U_{min} = 5$ m s^{-1} and 6 m s^{-1}, are chosen for further optimization. First, the wind speed is adapted for land and ocean separately. In PTB-5_1 and PTB-6_1, U_{min} is set to $= 1$ m s^{-1} over land to account for the slower near-surface wind speeds. Second, in PTB-5_1t, PTB-6_1t, the model is retuned to reestablish a similar top-of-the-atmosphere (TOA) imbalance to CTL."
- L150-151: The sentence was split in two and rephrased to read: "For this purpose, a 1 horizontal resolution is chosen. This resolution is close to the tropical 1.4 horizontal resolution of the 160-km simulation."
- L235-236: The corresponding paragraph was reformulated it now says: "In this section, we investigate the efficacy of a parameter adjustment in addressing the double-ITCZ bias across resolutions in ICON. We test the U_{min} parameter adjustments (Sec. 2.1.2) to address the near-surface dry bias over the Warm Pool region with the goal of interrupting the circle of biases outlined in the previous paragraph. With an increase in U_{min}, we aim to increase evaporation and near-surface specific humidity. We expect that the increase in near-surface specific humidity will improve the Warm Pool precipitation and the double-ITCZ feature, through the mechanisms shown in Fig. 1."
- L237-238 "vicious cycle" over the top: The word "vicious" was removed.
- L238-244 (break up this sentence): We changed the sentence and the description of $Umin$. The paragraph now reads: "In this framework, evaporation E is tied to wind speed U at the lowest atmospheric level via the bulk flux formula for evaporation

$$E = \rho_a C_E U(q_s - q_a), \tag{1}$$

where ρ_a is the atmospheric density, q_s surface specific humidity, q_a near-surface atmospheric specific humidity, and the bulk transfer coefficient for latent heat C_E , which is inversely proportional to the Richardson number. The bulk flux formulation shows that the latent heat flux is directly proportional to the near-surface wind speed. The fix suggested by segura$_single_2025doesnotchange$U$directly, butadapt$ $codedlowerlimitforthenear-surfacewindspeeds.InICONXPP'sturbulenceparameterization, thesurfacewindU$ with $U = MAX(U_{min}, U)$. This lower limiter is used to account for the influence of subgrid-scale turbulence with the goal of increasing the turbulent fluxes in low-wind regimes. Increasing the default value of U_{min} from 1 m s^{-1} decreases the Richardson number in low-wind regimes, e.g., in the Warm Pool, and leads to increased evaporation. The influence of changes in U_{min} at various resolutions, including in simulations with the full set of parameterizations, has not been tested before. It is important to note that changes in U_{min} can similarly impact turbulent fluxes of sensible heat and momentum in addition to the targeted latent heat flux. Potential consequences of the resulting changes in the momentum budget were not considered in segura$_single_2025.Concretely, thismeansthatthesuggestedincreasein$U$_{min}$ will increase the drag on near-surface winds. It is our aim to investigate whether there are associated negative influences on the circulation next to the positive effect for precipitation over the Warm Pool. "

Separate from the English usage, there were far too many typos. Please carefully proofread the revision carefully as a final step before submitting.
We carefully proofread the revision before submitting it.

Line by line comments

L13 "this could endanger the representation of the global circulation, energetic balance and teleconnections" confusing, due to the "could." Does it degrade these fields in your simulations or not?
Yes, the global circulation is affected as a consequence of the increased drag on the surface winds. This is visible in the slowdown of the trade winds (Fig. 7). As intended by the fix, the latent heat flux increases. However, the latent heat flux bias shifts and increases the relative contribution of inner-tropical moisture (Fig. 8), which changes the net energy input into the atmosphere. Both changes are degrading the corresponding fields and can not be counterbalanced as they are part of the solution to the precipitation bias. The slowdown of the trade winds endangers the teleconnections related to the wind-evaporation SST feedback. The top-of-the-atmosphere imbalance also changes considerably. However, this change can be corrected via re-tuning. We the sentence to "This degrades the representation of the global circulation, energy balance, and teleconnections." to be more definite.

L15 what does "nondiscardable" mean?
"Non-discardable" refers to parametrizations which are needed at the given resolution because the corresponding processes cannot be explicitly resolved. An example would be radiation or turbulence.

L25 "bias has been central to the precipitation bias discussions" this reads funny to me; consider rephrasing
The sentence was rephrased and now reads: "In addition to its influence on regional precipitation biases, the double ITCZ can influence large-scale climate phenomena such as the El Niño-Southern Oscillation [HK14, ZDCT14]. Therefore, identifying the cause of the double ITCZ is a key step towards improving climate models".

L27-29 Correct and you should cite one or more papers that document these transient double ITCZ states, e.g. Magnusdottir and Wan 2008, https://journals.ametsoc.org/view/journals/atsc/65/7/2007jas2518.1.xml. And this preprint is particularly relevant:
https://essopenarchive. org/doi/full/10.22541/essoar.174017095.57302520
We now cite both the proposed paper and preprint.

L42 Philander et al emphasize ocean atmosphere coupling and continental geometry, making it an odd choice to cite regarding this claim about net energy imbalance; Frierson et al. (2013) would be more appropriate.
Note that the "net energy input" includes the energy flux from the ocean to the atmosphere, and thus this statement is consistent with the work of Philander et al. We now additionally cite Frierson et al. (2013).

L50-57 I don't find this discussion of the model tuning especially compelling, in large part because I'm struggling to follow it. Can you make your argument more precise and clear?
This comment was addressed in the section above titled "Unclear arguments regarding model tuning". Please refer to our previous statements and the revised manuscript.

L75-77 This sentence is meaningless to the reader, like me, who doesn't know what ICON XPP and ICON Sapphire are. I think you can omit this entirely, or if you want to keep it consider moving to the methods section or revising to provide more context
This sentence is important to include for readers who are familiar with ICON, and we believe what is relevant to take away from it is sufficiently clear even to readers not familiar with ICON. We kept this sentence as is.

L88 I would omit "the fuel for the hydrological cycle"; unneeded and too imprecise
Omitted.

L94 "their implementation should receive more attention" this feels like too much of an editorializing statement to me in the context. I don't really know what 'minorlooking treatments' are beyond your summary having not read the Kawai et al paper, but based on your summary it's not obvious to me why they should indeed receive more attention.
The corresponding statement was deleted.

L111 I would omit the footnote; just include it in the parenthetical
The footnote is removed.

L118 calling 5 km convection "resolving" is a stretch... very few convective updrafts span 5x5 square km. "convection permitting" is a widely used and I think more appropriate choice.
We switched to convection permitting throughout.

L165 Doesn't ERA5 directly output specific humidity?
Yes, ERA5 does output the atmospheric specific humidity, and we show the corresponding value in the zonal bias plots. However, the near-surface humidity values at 10 m are not available in the archive.

L214 fix the citations
Done.

L215 A lot of typos through the end of this paragraph; feels sloppy.
The two typos were corrected and formulations were adjusted for an easier reading flow.

L229 What feedback loop?
A stronger Walker circulation would lead to even more increased near-surface moisture in the Warm Pool, which constitutes a feedback. To avoid any confusion about this term, we remove the words "through a feedback loop."

L229-232 Is this proposed feedback your idea? If yes, it feels rather speculative. If not, it needs citations.
Yes, it is our idea - based on physical arguments. We test this feedback loop in Section 3.2. In order to give the reader some guidance, we outline the hypothesis we are testing at this point. In order to account for the reviewer's concern, we also expanded the analysis and give more details on the changes in circulation and moisture transport in the revised manuscript version. The additional analysis includes an evaluation of the Walker and Hadley circulations and a moisture budget analysis.

Fig. 6 What does "normalized" probability density function mean?
Normalized means that the area under probability density function integrates to the value of 1. This complies with the standard mathematical definition, we therefore did not add any further description to avoid suggesting otherwise.

Fig. 7 Is OAFlux ultimately a better product than ERA5 for the surface LH fluxes? If so, then why show the biases of the simulations against both? Why not just use OAFlux in the context of this whole discussion? Perhaps I missed something here.
OAFlux does not provide all the quantities needed for the other bias plots (that is, all the temperature and humidity plots of the atmosphere). To remain in a consistent framework that is also closed energetically, we show the difference to ERA5 and only add OAFlux as complementing information.

L342-344 I don't understand this. Why do the signs of the respective biases lead to this inference about "symptoms" vs. "root cause"?
The corresponding paragraph was deleted during the restructuring process.

Fig. 8 symbols for control run are too faint
We adjusted the chosen color map. The corresponding figure now appears as this Figure R 3.

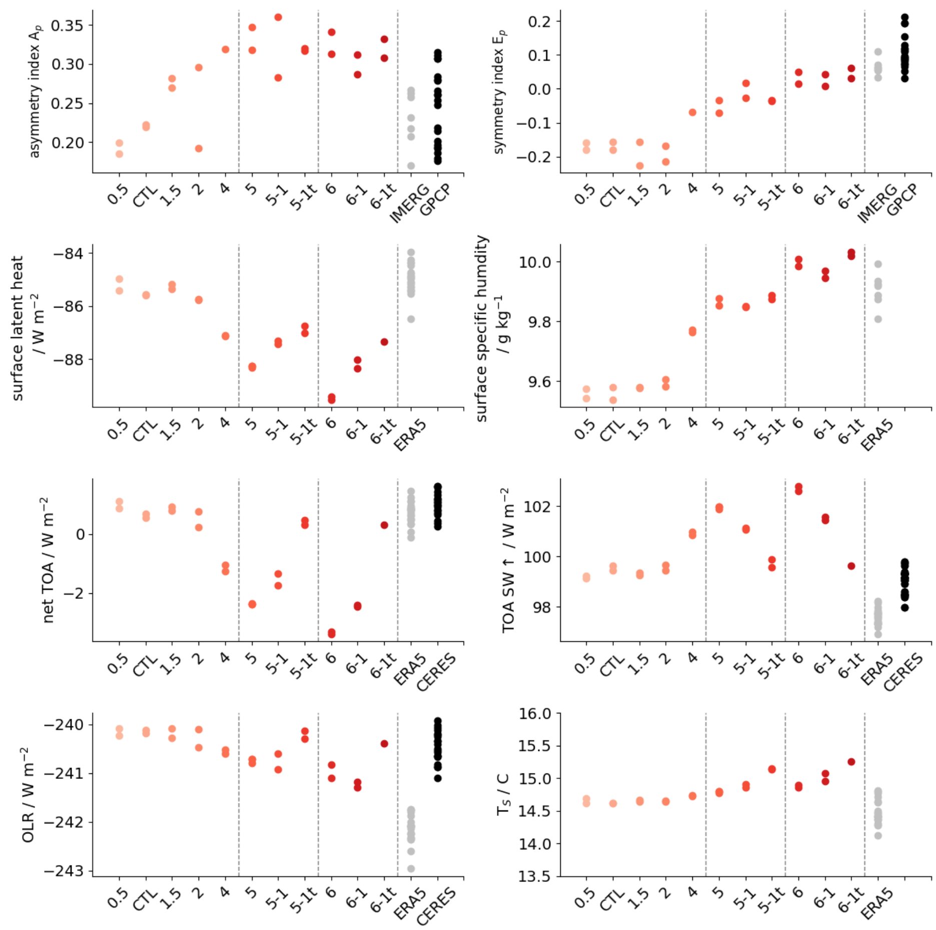

Figure 3: Tuning scores for all 40 km sensitivity experiments as a function of prescribed minimum surface wind speed. The values for a U_{min} of 1 are the CTL settings and where taken from the respective two years of the CTL simulation. Values for the asymmetric index A_p, symmetry index E_P as well as latent heat flux, near-surface specific humidity q_s, net top-of-the-atmosphere (TOA) imbalance, upwards radiative shortwave flux, outgoing long wave radiation (OLR) and global mean surface temperature T_S are shown. Each point depicts one global year average. For reference the corresponding values from IMERG, GPCP, ERA5 and CERES are depicted.

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