Response to Review 1

July 10, 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 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 climatological sea ice and sea surface temperature fields from the monthly 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 the precipitation and separate the effects of model biases on 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 biased. Therefore, we additionally compare our data to the OAFlux dataset [FBH+03, YW07, SDFT13, NCfARS22], which integrates satellite retrievals and three atmospheric reanalyses.". 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."

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 decomposition to investigate the strong vertical moisture transport at 20 degree latitude.

   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.
   By isolating and analyzing the impact of one specific parameter, our goal is to provide a clear and focused step toward understanding and mitigating the double - ITCZ bias. The decision to focus on "Umin" in this study is based on two main considerations. First, a recent study [SBF+24] 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 "important influence" in conjunction with subjunctive.
   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 (similar to those in Seager et al. 2010, but focused on the climatologies instead of climate change). 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. 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 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 moisture flux convergence above 850 hPa and "precipitation minus evaporation" (P-E) to demonstrate the bias in vertical transport out of the subtropical boundary layer that the ICON simulations exhibit. Figure 2 shows that the transport bias peaks at 20 North and 20-30 South, the regions where the specific humidity profiles show a moist bias. The wind surface limiter fix mainly improves the moisture transport in the tropical deep convective regions.

Table 1: Walker and Hadley Circulation Strengths for ERA5 as well as CTL, and PTB-5.

Experiment	**Walker Circulation Strength** $\chi*_{max}$ / $10^7 \times m^2$ s^{-1}	**Hadley Circulation Strength** χ_{max} / $10^6 \times m^2$ s^{-1}
ERA5	1.16	2.02
CTL	0.94	2.17
PTB-5	1.27	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 prominent tropical precipitation bias that has persisted over several 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 use the unique possibility offered by the new ICON XPP model configuration to study the double-ITCZ bias in a resolution hierarchy spanning from parameterized to resolved convection within a consistent modeling 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 definitiveness in the statement in the abstract, we reformulated as: "However, we highlight that an important underlying influence on the double-ITCZ

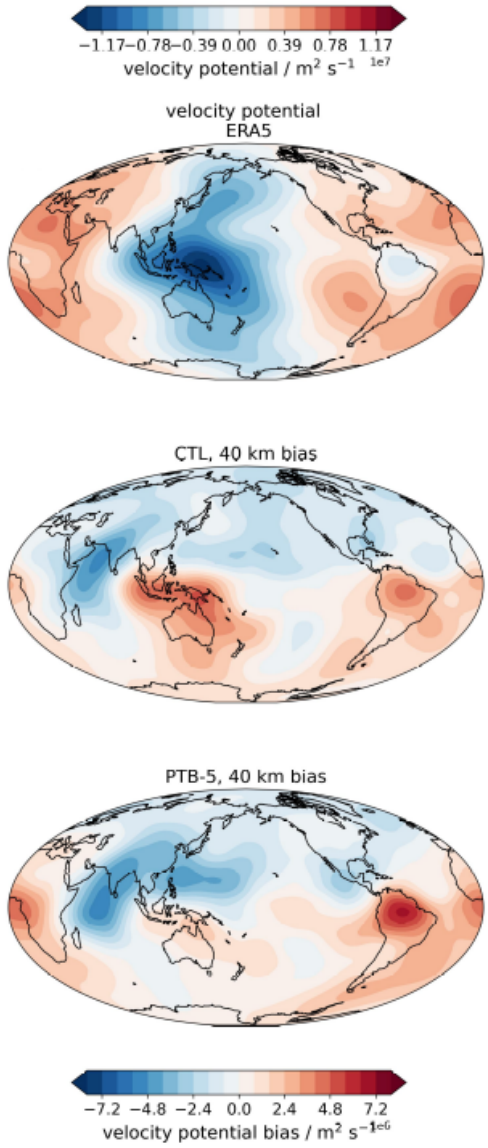

Figure 1: ERA5 multi-year average velocity potential χ at 200 hPa for the years 2004-2010. Two-year mean bias of the CTL, and PTB-5 experiment with respect to the ERA5 velocity potential. Negative values of the velocity potential are found in regions of ascent and divergent motion; positive values in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

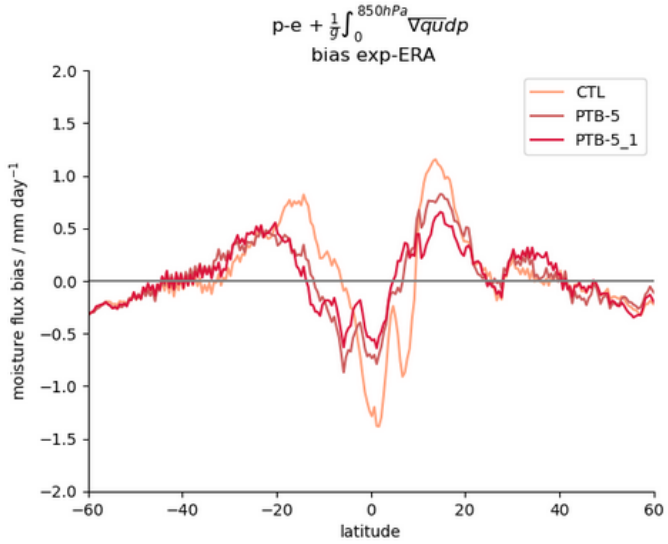

Figure 2: Biases in the moisture transport out of the boundary layer in CTL, PTB-5 and PTB-5_1 with respect to ERA5. The two-year mean difference between vertically integrated moisture flux convergence above 850 hPa and precipitation minus evaporation (P-E) is shown.

in our uncoupled ICON seems to lie in 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 an important underlying influence on the double-ITCZ in ICON seems to lie in the insufficient transport of moisture from the subtropics to the inner tropics. The resulting low bias in tropical near-surface moisture substantially reduces deep convection over the Warm Pool, leading to a weakened Walker Circulation."

11 what do you mean by 'addresses'?
We replaced the word "addresses" with the word "resolves", it now states: "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."

12 subtropical contribution to what?
We clarified that the entire sentence is focusing on "moisture" by adding "to near-surface moisture" after the word "contribution". 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 "deteriorate".

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 (double - ITCZ) is the most prominent problem [MRB+95, Lin07]. The double-IITCZ bias 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 model generations in the Coupled Model Intercomparison Project, CMIP (Tian and Dong, 2020). Among them, the double - ITCZ bias (double-ITCZ) is the most prominent problem (Mechoso et al., 1995; Lin, 2007)."

31 increased wind convergence where?
We reformulated to be more specific: "Over the Pacific, the natural double - ITCZ feature is associated with local minima in surface humidity and temperature along the equator (Zhang, 2001) and increased southeast Pacific ITCZ wind convergence (Halpern and Hung, 2001)".

36 caused ?by? moisture
We agree that the addition "caused moisture mass flux in the tropics." can lead to irritation and omitted it. We modified the sentence to read: "The net supply of moisture from higher latitudes leads to an excess of precipitation over evaporation in the tropics.".

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 extra-tropical biases in the energy budget can also be sources of the problem [KHFZ08, HF13], and tropical biases can likewise cause biases in the sub- and extra-tropical [DABBW22]. "

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: "The focus of the paper lies on:

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 and how do the chosen adjustments ameliorate it?
   (*Addressed in Section 3 and Section 4, summarized in Schematic Figure 1, 5 and 14.*)"

86 please explain what is the bulk-flux formulation.
We now added more details to the bulk flux formulation for evaporation: "To address the second group of research questions, we focus on the choice of a minimum surface wind speed threshold (U_{min}) in the turbulence scheme of ICON XPP (see Section 2.1.2 and 3.2 for details). In the used bulk-flux formulation, the surface latent heat flux is a function of near-surface moisture and wind speeds. In this context U_{min} sets the minimum wind speed for the calculation of the Richardson number, effectively

decreasing it in low wind speed regimes. Mechanistically, a higher threshold U_{min}, therefore, leads to an increased latent heat flux into the atmosphere under low wind conditions, and with it more available moisture for convection." The formula itself is also discussed when the bulk flux formulation for latent heat release is stated in section 3.2.

87 is undefined
We added a description of U_{min}, 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 slowly build up Figure 13 and show the panels (a), (b) and (c) when they are discussed within the introduction and results section. This sentence was reformulated accordingly and now states: "The identified large-scale differences in the three scenarios will be visualized building-up on the circulation schematic (Fig. 1) and summarized in the Discussion and Conclusion section."

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 climatological sea ice and sea surface temperature fields from the monthly 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 the precipitation and 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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