

Review of

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

Kroll et al.

## 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.

## 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 to 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. 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.
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.
3. The scope of the analysis is limited. This 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 ( $U_{min}$ ) 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 notwithstanding) therefore in my opinion exaggerates the scope and implications of the study. 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).

## 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.
- 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.
- 9 biased how? Without specifying this, the following sentence is hard to interpret.
- 11 what do you mean by ‘addresses’?
- 12 subtropical contribution to what?
- 13 what do you mean by endanger?
- 21 CMIP\_(Tian and Dong, 2020) — similar missing space in many other places in the text.
- 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.
- 23 please provide a citation in reference to the prominent problem.
- 31 increased wind convergence where?
- 36 caused **?by?** moisture
- 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”)
- 46&48 Not necessarily subtropical, it could be from any region outside the tropics.
- 80 The leading questions are themselves composed of questions. 1. Is actually three questions, and 2&3 are two questions each.
- 86 please explain what is the bulk-flux formulation.
- 87  $U_{min}$  is undefined
- 104 please specify what is the schematic. Figure 13?
- 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.)
- 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.
- Figure 1  $\text{kg}^{-1} \rightarrow \text{kg}^{-1}$  (also in all of the other figures)  
... near-surface specific humidity, calculated with respect to values derived from ERA5 reanalysis, is ...  
Please refer to panel letters in the caption
- 203 ?resp.?
- Figure 2 and elsewhere, it would be better to describe units in square brackets, rather than following a divider, e.g., height / km  $\rightarrow$  Height [km]
- 214 citation error
- 216,220 and elsewhere,  $\backslash\text{citet}\{\} \rightarrow \backslash\text{citep}\{\}$

Figure 3        Would be interesting to check if the biases and resolution change affect the tropical cold dome (i.e., near the tropical tropopause), which is set by the ability of deep convection to penetrate higher into the upper troposphere.

229—232        Try to break this sentence

233        schematic at the end of the text — > Figure 13

237        ‘vicious’ is ambiguous

297—305:        This paragraph is speculative. What is the support for the changes in the intensity of the Walker circulation and marine inversion? What about meridional moisture transport?

308        what is the support for the changes in the Walker circulation strength?

334        what is the support for the assumed bias in atmospheric net energy input?

347—350        try to break this sentence

371—379        Again, this is highly speculative. What is the support for increased drag, or change in the Walker circulation? In relation to changes in surface winds, cite the relevant figure.

410        you refer to ‘moisture transport’ but Figure 10 only shows specific humidity. What is the support for the assumed dynamic changes?

Figure 11        color contrast is weak

422         $A_p$ ?

439        citation error.