Response to Review 2

July 10, 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 of the atmospheric respresentation with the discard 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):

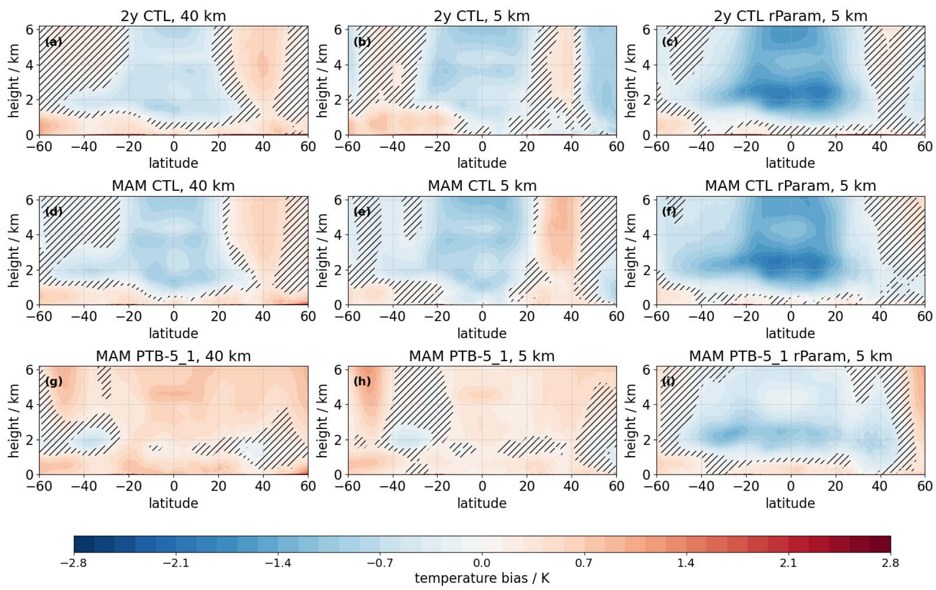

Figure 1: Zonal mean temperature bias with respect to ERA5: all CTL for a 2 year average (subfigures (a-c)), all CTL for an average over two MAM seasons (subfigures (d-f) and all PTB-5_1 for an average over two MAM seasons (subfigures (g-i)) for 40 km, 5 km and 5 km, rParam. Statistically significant 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.

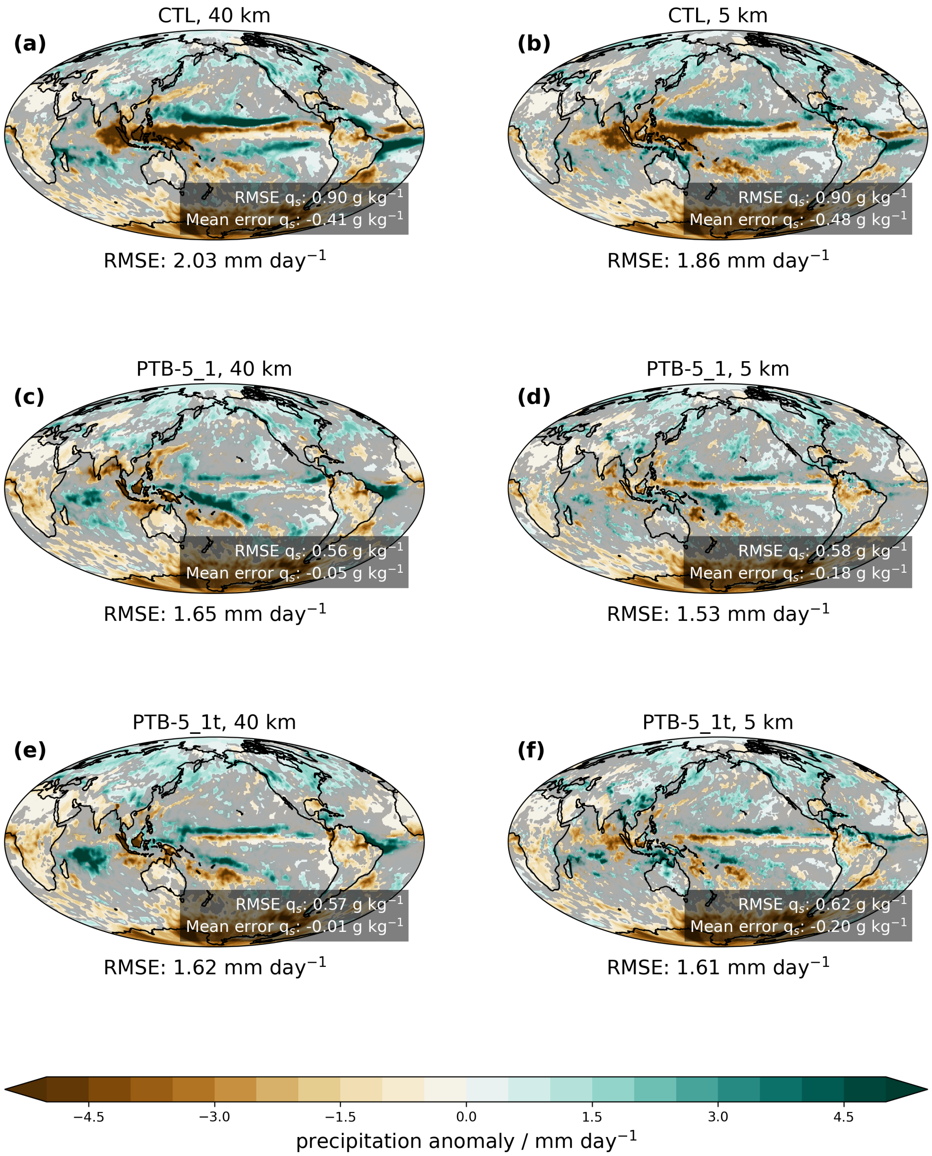

Figure 2: Two-year mean large-scale precipitation bias with respect to IMERG 2004-2010 for different settings of the surface wind at 40 km resolution: PTB-0.5 (subfigure (a)), PTB-4 (subfigure (b)), PTB-5 (subfigure (c)), PTB-5_1 (subfigure (e)), PTB-5_1t (subfigure (g)), PTB-6 (subfigure (d)), PTB-6_1 (subfigure (f)) and PTB-6_1t (subfigure (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 Hourdin et al. 2017 [HMG+17]. There can be trade-offs between these different optimizations, although there are existing approaches to optimize the precipitation based on TOA flux [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 highresolution 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 many relevant processes are now resolved, can 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: "There can be trade-offs between these different optimizations, although there are existing approaches to optimize the precipitation distribution based on TOA flux [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 sentence was reformulated as follows: "The net supply of moisture from higher latitudes leads to an excess of precipitation over evaporation in the tropics. "
- L49: We reformulated to: "It is critical to recognize the coupling between the double - ITCZ problem and biases in the energy budget 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 is based on the parametrizations for radiation ecRad [HB18], cloud microphysics [Sei08], vertical diffusion (TTE) [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 simplified to: "The limiter U_{min} is used to compute the wind-shear term of the near-surface moist Richardson number if the horizontal velocities meet the criterion $\boldsymbol{U}^2_{horizontal} < U^2_{min}$. "

- L134-135: This sentence was deleted.

- L138-141: The sentence was split into four for simplification: "The two U_{min} settings that perform best in representing the large-scale annual-mean ITCZ, PTB-5 and PTB-6, with U_{min} $= 5\ ms^{-1}$ and $6\ ms^{-1}$, are further optimized. First, the wind speed is adapted for land and ocean separately. In PTB-5_1 and PTB-6_1, U_{min} is set to $= 1\ ms^{-1}$ 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 the CTL top-of-the-atmosphere (TOA) imbalance."

- L150-151: The sentence was split in two and rephrased to read: "For this purpose, a 1 degree horizontal resolution is chosen. This resolution is close to the tropical 1.4 degree horizontal resolution of the 160 km simulation."

- L235-236: The sentence was split apart and now reads: "We now understand the biases common to all resolutions and their potential role in the ITCZ formation. In a next step, we focus on the second group of research questions addressing parameter adjustments to counterbalance the identified issues in the atmospheric representation."

- 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: "Near-surface humidity is strongly influenced by evaporation. Evaporation E can be 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 near-surface specific humidity, q_a atmospheric specific humidity and C_E a proportionally constant. The bulk flux formulation shows that the latent heat flux is directly proportional to the near-surface wind speed. We do not change U directly however, but adapt a hard-coded minor-looking treatment. In ICON-XPP's turbulence parameterisation, the surface wind used for the computation of the Richardson number has a lower prescribed threshold U_{min} with $U = MAX(U_{min}, U)$. Increasing the default value of U_{min} from 1 decreases the Richardson number in low-wind regimes, enhancing the proportionally constant C_E. This increases turbulence and, consequently, the turbulent fluxes. In total, the bulk flux formula for latent heat release thus suggests an increase in horizontal wind speed low-limiter, U_{min}, as a potential lever to address the dry bias over the Warm Pool and the related double - ITCZ biases. It is important to note that changes in U_{min} can similarly impact the turbulent fluxes of sensible heat and momentum next to the targeted latent heat flux. "

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. 5). 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. 7), 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 changed the wording to "can" to be more definite.

L15 what does "nondiscardable" mean?

"Nondiscardable" 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: "The simulated double - ITCZ results in strong local precipitation biases and influences the El Niño–Southern Oscillation and other large-scale climate phenomena [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.

Unlike "conventional" tuning parameters, "minor locking treatments" are hardcoded in the code and act as limiters in physical quantities. They are much less regularly revised, i.e. when the model resolution is increased. We now choose a softer formulation: "As "minor-looking treatments" can have impacts comparable to the exchange of parameterization schemes [KYKY22], their implementation merits more attention."

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"?

We reformulated giving more details: "The high bias in latent heat flux suggests that enough moisture is supplied to the atmosphere without the wind speed limiter fix. The increase in positive latent heat flux bias in the inner tropics with the wind speed limiter fix shows that the inner-tropical moisture source is increasingly overrepresented at the cost of higher-latitude moisture sources within the trade wind regions. The coincidental increase in the higher-latitude moist bias in the free latitudes demonstrates that the moisture is transported vertically instead of horizontally into the inner tropics, which is likely to lead to the dry bias in the inner tropics. These three findings point to an underlying problem in moisture transport for which U_{min} only combats the symptoms rather than the root cause.".

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.

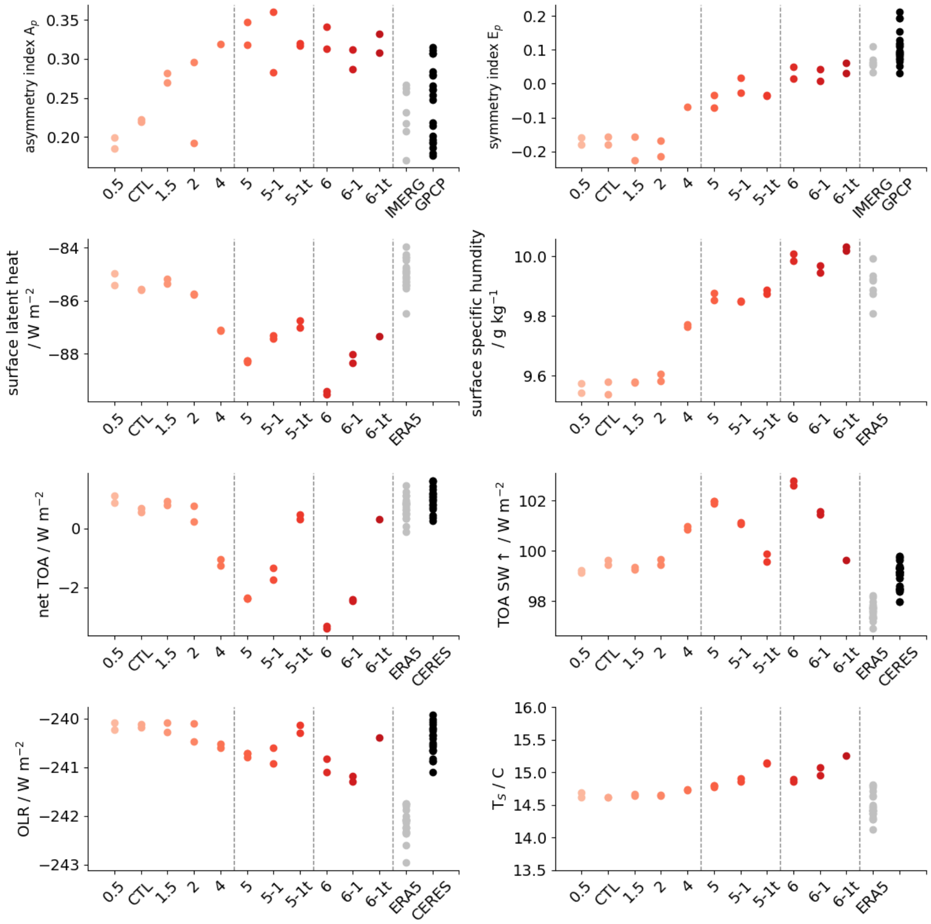

Figure 3: Tuning scores for all 40 km sensitivity experiments as a function of prescribed minimum surface wind speed. The values for a U_{min} of 1 are the CTL settings and where taken from the respective two years of the CTL simulation. Values for the asymmetric index A_p, symmetry index E_P as well 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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