Response to Reviewers: Atmospheric Blocking Representation in Storm-Resolving Climate Models under Historical and Future Forcing

We again thank the reviewers for their thorough review and comments on our revised manuscript. We address each reviewer's concerns below. As in the previous response document, we have highlighted in blue the specific changes made in response to each comment. Unless otherwise noted, figure and line numbers refer to the original manuscript.

Reviewer 1

I thank the authors for responding carefully to my previous comments. The revised manuscript has been substantially improved, so I can recommend publication.

I only noted a small inconsistency between 28 years and the period 1993-2019 (that equals 27 years) for ICON hist in Table 1.

We thank the reviewer for the positive assessment of the revised manuscript and for recommending it for publication. We also appreciate the careful identification of the inconsistency in the simulation length for ICON historical in Table 1. The period 1993–2019 corresponds to 27 years, and we have corrected the table accordingly.

Reviewer 2

Assessment

I appreciate the authors' efforts in addressing my previous comments in the first round of the review process and revising the manuscript accordingly. The paper is now in much improved shape and clearer in several important aspects including a more concise title. However, some shortcomings remain. Some of them have only recently become apparent, while others persist from the earlier version. The study demands further improvement in the discussion of the results as well as a more convincing justification for including the blocking trend estimates. The latter represents the only remaining major concern. Given the progress made and the issues that still require improvements, I recommend a **minor** revision. If the concerns outlined below are adequately resolved, the manuscript would be in a good position for acceptance in *Weather and Climate Dynamics*.

We thank the reviewer for the positive assessment of the revised manuscript and for acknowledging the improvements made. Regarding the remaining concern on the inclusion of blocking trend estimates, we have further clarified their role in the manuscript. In particular, we emphasize that the climate change analysis is not intended as a standalone result, but rather as a complementary component that provides context for the response of atmospheric blocking to future forcing in storm-resolving models. We trust that these revisions address the reviewer's concern and clarify the scientific motivation for including the trend analysis.

General comments

1.) The authors consider the evaluation of future blocking trends valuable, particularly when the projected signals are consistent with those from CMIP-based studies. This raises concerns about the reliability of the results in cases where km-scale models and CMIP models disagree. Such a selective agreement does not provide a sufficiently robust basis for confidence in the projected trends. Also, if the analysis emphasizes only trends that align with CMIP-based studies, it is unclear what the novel contribution is beyond showing that km-scale models seem to behave similarly to CMIP models.

In light of this, I encourage the authors to reconsider the relevance and robustness of these results. A meaningful evaluation of blocking trends requires a more careful and comprehensive treatment, accounting for both mean state biases and trend signals across the key contributing factors (background flow, SSTs, and storm tracks). If the section is to be retained, the magnitude of the trend signal needs to be critically assessed in a quantitative manner to ensure it can be distinguished from the influence of model biases. In particular, an estimation of SST biases would be highly beneficial, given that the AMIP simulation perform considerably better and the manuscript emphasizes the strong sensitivity of blocking to underlying SSTs in both seasons.

Additional minor comments are provided in the line-by-line comments below.

We thank the reviewer for this thoughtful and constructive comment. We agree that agreement with CMIP-based studies alone does not constitute a sufficient basis for confidence in projected blocking trends, and we clarify that our intention is not to emphasize selective agreement. In the revised manuscript, we explicitly discuss both similarities and differences between storm-resolving models and CMIP6 in the historical simulations, and interpret these results as part of a broader assessment of the consistency of blocking representation across model hierarchies. We further clarify that the projected changes are derived from a single storm-resolving model (IFS) and should therefore not be interpreted as robust projections. Instead, the trend analysis is included to provide context for the response of blocking to future forcing within this modelling framework, and is interpreted in light of the biases identified in the historical simulations using CMIP6, IFS, AMIP-IFS, and ICON. In this context, the added value of the present study lies not only in identifying similarities with CMIP6, but also in linking the simulated future response to the underlying representation of blocking and associated processes in storm-resolving models. This allows us to assess whether higher-resolution models yield qualitatively consistent or distinct signals, while highlighting the limitations associated with their mean-state biases. We also acknowledge that a comprehensive evaluation of blocking

trends would require a detailed analysis of the role of contributing factors such as background flow, SSTs, and storm tracks. While such an analysis is beyond the scope of the present study, we have strengthened the Discussion to more clearly articulate these limitations, including the potential influence of SST biases suggested by the differences between AMIP and coupled simulations, and to avoid overinterpretation of the trend signals. We trust that these revisions address the reviewer's concerns and improve the clarity and robustness of the manuscript.

Line-by-line comments

Line 40-43: 1) Horizontal resolution and 5) Parameterisations are technical model-related aspects, whereas 2), 3) and 4) are physical processes of the climate system. The authors should add this information and emphasize the interdependence.

We thank the reviewer for this helpful suggestion. Following this comment, we have revised the text to explicitly distinguish between model-related aspects (horizontal resolution and parameterizations) and physical processes of the climate system (storm-track activity, jet structure, and SSTs). We also emphasize their interdependence, clarifying that model formulation influences the representation of these physical processes, which in turn affects the simulation of atmospheric blocking. We suggest the following sentence:
"Several interacting factors are thought to contribute to these deficiencies, including both model-related aspects and physical processes of the climate system. Model-related aspects include (1) coarse horizontal resolution and (2) simplified parameterizations of moist diabatic processes, which influence the representation of key physical processes such as (3) storm-track (transient eddy) activity, (4) the large-scale jet waveguide (mean flow), and (5) sea surface temperatures (SSTs) and air–sea coupling. These factors are strongly interdependent and jointly affect the simulation of atmospheric blocking (Woollings et al., 2018; Dolores-Tesillos et al., 2025; Woollings et al., 2025)."

Line 63-64: I would suggest to remove the second part of the sentence, as the AMV is not further addressed in the study.

We thank the reviewer for pointing this out. We have removed the second part in the revised manuscript.

Line 76: Add space between "realistically" and reference.

We thank you the reviewer for noticing the missing space. We have modified accordingly in the revised manuscript.

Line 80-81: "...where biases of SST, storm tracks and moist processes..."

We thank the reviewer for helping us to polishing the wording. We have taken the suggestion and implemented it in the revised manuscript.

Line 82: What is meant by "expose new limitations"? Either expand or remove this phrase.

We thank the reviewer for this comment. We have clarified this phrasing in the revised manuscript. Specifically, we now explain that "exposing new limitations" refers to the possibility that, while storm-resolving models may reduce some biases in blocking representation, they may also reveal sensitivities or biases related to the representation of key processes, such as the mean state, large-scale circulation, or air–sea coupling, which may not be as apparent in coarser-resolution models. The sentence has been revised accordingly to improve clarity. Suggested sentence:
"Understanding whether storm-resolving models meaningfully reduce blocking biases, or instead reveal additional sensitivities related to the representation of the mean state, large-scale circulation, or coupled processes, is therefore a critical step for advancing blocking theory and climate projection credibility."

Line 100 & 105: The reference to nextGEMS is given twice here. I'd suggest to remove the second part of the sentence in line 105.

We thank the reviewer for pointing this out. We have removed: ", which are primarily derived from nextGEMS efforts."

Line 118-124: This is the main motivation for assessing km-scale models. I'd recommend moving it to the introduction.

We thank the reviewer for this suggestion. We agree that this paragraph provides important motivation for assessing kilometre-scale models. In response, we have strengthened the Introduction to more clearly highlight the role of storm-resolving models in improving the representation of processes relevant to atmospheric blocking. We have retained the paragraph in the Data section, as it provides more specific context on the capabilities of the particular kilometre-scale simulations used in this study (e.g., representation of mesoscale precipitation and orographic forcing), thereby complementing the more general motivation given in the Introduction. We believe that this structure avoids redundancy while maintaining a clear distinction between the general motivation and the model-specific context. See our modified paragraph in the introduction:
"Recent studies have shown that increasing horizontal resolution has the potential to improve storm-track and blocking representation by better capturing orography, jet structure, warm conveyor belts, and transient eddies (Berckmans et al., 2013; Willison et al., 2013; Schemm, 2023; De Luca et al., 2024; Takasuka et al., 2024; Gao et al., 2025). Atmospheric blocking is strongly influenced by the interaction between large-scale flow, transient eddies, and diabatic processes, all of which depend sensitively on the representation of these features. Storm-resolving climate models therefore provide a unique framework to explicitly test these processes. By operating at kilometre-scale resolution, they substantially reduce the reliance on deep convection parameterizations, resolve mesoscale SST gradients and frontal structures, and represent transient eddies, jet curvature, and diabatic processes associated with warm conveyor belts more realistically (e.g., Vivant et al., 2025). These improvements directly target key mechanisms underlying blocking onset, maintenance, and decay, which are only partially captured in conventional CMIP-class models. As such, storm-resolving simulations offer an opportunity not only to assess whether increased resolution improves blocking statistics, but also to diagnose which processes remain limiting factors when resolution-related constraints are relaxed. "

Line 212: Why do you use a southern border of the ATL domain at 50°N and for the PAC domain at 40°N? Please clarify.

We thank the reviewer for this question. The domain definitions follow Schiemann et al. (2020), where different southern boundaries are used for the Atlantic and Pacific sectors to better capture the respective blocking regimes. In particular, blocking in the North Pacific tends to extend further south compared to the North Atlantic, which motivates the choice of 40°N for the Pacific domain and 50°N for the Atlantic domain. This becomes evident when using the anomaly index (see Figure S1 of Schiemann et al. (2020)). These domains are not intended to strictly represent ocean basins, but rather to encompass the full spatial extent of the dominant blocking activity, including adjacent continental regions. This approach ensures consistency with previous studies and allows for a more complete representation of blocking variability, including regimes such as Ural blocking. The later is described in Section 3.2

Line 221: Remove "in multidecadal simulations" in the 4.1 title. Same for 4.2 title in line 348.

We thank the reviewer for this suggestion. We have removed the phrase "in multidecadal simulations" from the titles of Sections 4.1 and 4.2 accordingly.

Line 235: Consider adding the bold-written part: "suggesting that biases **of blocking frequency** in the coupled version"

We thank the reviewer for the suggestion. We have added the bold-written part.

Line 253, 258, 331, 401-402, 433, 452: These lines include overly too descriptive parts of what is shown in the respective figures. As I already mentioned in the first review, the authors should consider moving the figure reference to the end of the sentence and removing these overly too descriptive information, which

distracts the reader from being fluently guided through the results.

We thank the reviewer for this helpful suggestion. Following this comment, we have revised the text to improve readability by moving figure references to the end of the corresponding sentences and by reducing overly detailed descriptions where possible. At the same time, we have retained key information necessary to identify the relevant panels and metrics, in order to address comments from the first review regarding the need for clarity and completeness in the figure descriptions. We believe that this revised approach provides a better balance between readability and sufficient detail to guide the reader through the results.

Line 304-307: Same here: This paragraph is too long in terms of accompanying information, describing how the short following paragraph is structured. I'd suggest to remove this part.

We thank the reviewer for this comment. We have removed this paragraph.

Line 310: This conclusion about the baroclinicity can be *suggested*, but not drawn from any figure shown in the manuscript. I'd suggest rephrasing this sentence accordingly.

We thank the reviewer for this comment. We agree that baroclinicity is not directly diagnosed in the figures shown. We have therefore rephrased the sentence to clarify that the inferred increase in baroclinicity is based on the SST gradient and should be interpreted as a suggested mechanism rather than a directly demonstrated result. Rephrased sentence:
"This suggests an enhancement of lower-tropospheric baroclinicity, which would support a stronger eddy-driven jet near 50–55°N."

Line 436: The result about ICON hist and CMIP6 ensemble should be compared against ERA5 and not against IFS hist.

We thank the reviewer for this important clarification. We agree that comparisons should be consistently made relative to ERA5 rather than between individual models. Accordingly, we have revised the text to ensure that SST biases and their relationship to blocking are described with respect to ERA5. In particular, the discussion has been rephrased to avoid direct comparison with IFS hist and instead emphasize how the magnitude and sign of SST biases relative to ERA5 relate to differences in blocking behaviour across models. We have also revised the paragraph for clarity and consistency, including the placement of figure references and reduction of overly descriptive elements, as noted in previous comments.

Line 440: Again insert the figure reference at the end of the sentence. The authors may consider adding labels to subplots (e.g. a), b), c) ...)

We thank the reviewer for this suggestion. We have revised the sentence accordingly by placing the figure reference at the end to improve readability. Regarding the subplot labeling, we have added the label referenced in the text. We also note that the panels are labeled consistently across figures to facilitate comparison between different variables (e.g., blocking, storm-track, and zonal wind biases). In the case of SST biases, the IFS AMIP configuration is not shown, as it is constrained by observed SSTs and therefore does not exhibit a bias. For consistency with the labeling used in the other figures, we have retained the current panel labels. We hope this clarification addresses the reviewer's comment.

Line 478: Remove "nextGEMS"

We thank the reviewer for this suggestion. We have modified accordingly.

Figure 11: Does the trend patterns still hold significance, when the threshold is increased from 80%? The authors should rethink the value, upon which significance is given. As mentioned in the general comment 1, the authors could think about a way of indicating a trend significance by accounting for the uncertainties arising from the underlying mean state biases. Here, I would recommend to focus on the strength of the SST bias as the AMIP simulations indicates the importance of correct SSTs.

We thank the reviewer for this important comment. The 80% threshold was chosen to highlight regions with pronounced relative changes in blocking frequency, rather than to provide a formal measure of statistical significance. We agree that the interpretation of these patterns may depend on the chosen threshold, and we have clarified this in the revised manuscript. To address this point, we have revised the corresponding discussion to emphasize that these highlighted regions should be interpreted as indicative of strong changes, rather than as robust or statistically significant signals. We also explicitly note that the projected changes are derived from a single model and may be influenced by underlying mean-state biases. In addition, we have strengthened the discussion of the role of SST biases, particularly in light of the AMIP simulations, to highlight how uncertainties in the mean state can affect the interpretation of projected blocking changes. We believe that these revisions provide a more cautious and physically grounded interpretation of the trend patterns without introducing additional assumptions related to threshold selection.

Line 531: You are writing "previous studies", however, there are no studies cited. Please add the reference to the studies that you are addressing.

We thank the reviewer for pointing this out. We have added the appropriate reference to support this statement in the revised manuscript.

Line 576: "?" - Reference list probably includes a mistake in the compilation of one reference.

We thank the reviewer for noticing this issue. We have removed the question mark.

Line 590-600: The subsection 5.5 provides a summary again rather than a discussion in the context of state-of-the-art research. Particularly, this section dedicated to future changes requires a thorough discussion due to the lack of reliability emerging from the mean state biases.

We thank the reviewer for this important comment. We agree that the previous version of this subsection was too focused on summarizing the results and did not sufficiently place them in the context of existing literature. We have therefore revised this section to provide a more comprehensive discussion of the projected changes in atmospheric blocking. In particular, we now explicitly relate our findings to previous studies based on CMIP models, highlighting that projected changes in blocking are generally modest, regionally dependent, and subject to substantial uncertainty. We also expand the process-based interpretation by discussing the role of jet and storm-track changes, as well as the sensitivity of blocking to SST gradients and air–sea coupling. Furthermore, we have strengthened the discussion of the limitations arising from mean-state biases, emphasizing that the projected changes are derived from a single storm-resolving model and should not be interpreted as robust projections. Instead, they are framed as an indication of the sensitivity of blocking to future forcing within this modelling framework. We believe that these revisions provide a more balanced and state-of-the-art discussion of future blocking changes and address the reviewer's concern.

Line 659: Where is the reference to the "studies with similar results"?

We thank the reviewer for this comment. We have added the relevant references.

Line 667: Why are these types of simulations considered particularly promising for a more process-based analysis? The model output ultimately determines which processes can be examined, largely independent of resolution. In general, the reader would also expect a combined assessment of the three processes considered. Specifically, which of the three factors (jet stream, SSTs, storm tracks) has the strongest impact on improving the representation of blocking, and which one requires the most improvement?

We thank the reviewer for this insightful comment. We agree that the previous formulation was too general and did not sufficiently justify why storm-resolving simulations are particularly suited for a process-based analysis. We have therefore revised the paragraph to clarify that their added value lies in the improved representation of interacting processes, such as transient eddies, jet structure, and air–sea coupling, rather than resolution alone. In addition, we have incorporated a more explicit synthesis of the processes considered, highlighting the role of SST biases (particularly as indicated by the differences between AMIP and coupled simulations) while also emphasizing the close coupling between jet structure, storm-track activity, and block-

ing. As well as the feedbacks between the SST biases and stormtracks. Since the model topography is better resolved by the high-resolution simulations, we can assume that the biases in the background flow are driven to a substantial degree by the SST biases and the misrepresentation of the moist diabatic processes. Given that this paragraph serves as the final summary of the manuscript, we have kept the revision concise while ensuring that the key process-based interpretation is clearly conveyed.

References

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