Authors' Response to Reviewer 1

General Comments. Review of manuscript egusphere-2025-2212 by Tahvonen et al.

The authors investigate the effect of reduced sea ice extent and higher SSTs separately or together using global climate simulations from two models on the change in zonal wind and Rossby wave breakings. They find that the impact of warmer SSTs is stronger than the impact of reduced sea ice especially on wave breaking frequencies.

The study is interesting and the manuscript well written. Therefore, I recommend minor revisions. My comments follow in the order of the manuscript.

Response: We thank the reviewer for their detailed comments that have helped to improve the manuscript and are pleased that they found the manuscript interesting. We have listed the comments made by the reviewer below and address each of them individually. Changes to the manuscript text are indicated in the boxes shaded gray.

Minor comments:

Comment 1

Lines 69-72: The authors could also mention that absolute vorticity fields on isobars have also been used to detect Rossby wave breaking (e.g., Rivière, 2009, and Barnes and Hartmann, 2012) as it is easier to get from climate models outputs.

Response:

Thank you for the suggestion. The manuscript has been edited as follows to acknowledge that absolute vorticity fields have also been used for RWB detection:

Absolute vorticity on pressure levels, a model product more commonly available than PV and θ, has also been used by e.g. Rivière (2009) and Barnes and Hartmann (2012). Approaches based on PV are generally favoured since potential vorticity and potential temperature are conserved in adiabatic and frictionless flow.

Comment 2

Lines 130-131: Concerning the simulations set-up: For the SIC_{SSP585} simulation, what are the values of the historical SSTs where sea ice has disappeared? For the SST_{SSP585}, are the SST along the sea ice edge larger (due to future ocean warming) than in the historical simulation? In other words, is the surface temperature gradient much larger at the sea ice edge and if yes, what is the expected impact of this "artificial strong gradient"? Please add a bit more explanation on these points in the manuscript.

Response:

For the SIC_{SSP585} simulation, the historical SSTs values which are used depend on the sea ice concentration in the Baseline simulation. If sea ice concentration values are lower than 1, then historical SST values provided by the ACCESS-ESM1.5 model are used. If the historical sea ice concentration is 1, then the SST values in the SIC_{SSP585} simulation are set to the melting point of sea water (approx. -1.8 °C). This results in skin temperatures which are slightly lower than the melting point of freshwater, where sea ice is removed.

For the SST_{SSP585} simulation, the SSTs are increased according to the projected warming in ACCESS-ESM1.5 including in the sea ice boundary zone (sea ice concentration values between 0 and 1). In the sea ice boundary zone, the changes in SST gradients from ACCESS-ESM1.5 are the major driver of the near-surface baroclinicity. Overall, the SST gradient changes lead to both reductions and increases in near-surface baroclinicity.

However, these changes are small in magnitude above the boundary layer. The following text has been added to the revised manuscript:

> For the SIC_{SSP585} simulation, the historical SSTs values which are used depend on the sea ice concentration in the Baseline simulation. If sea ice concentration values are lower than 1, then historical SST values provided by the ACCESS-ESM1.5 model are used. If the historical sea ice concentration is 1, then the SST values in the SIC_{SSP585} simulation are set to the melting point of sea water (approx. -1.8 °C). This results in skin temperatures which are slightly lower than the melting point of freshwater, where sea ice is removed. In the SST_{SSP585} simulations, SSTs are increased also in areas where sea ice concentration values range between 0 and 1. However, this has only a minimal impact on the surface temperature gradient (Naakka et al., 2024) and baroclinicity above the boundary layer.

Comment 3

Lines 174-178: What is the point of the DBSCAN step and how can it implies a discrimination with fixed strength and spatial extent ? Can't simple criteria of strength and spatial extent be used instead?

Response:

When looking for RWB on all available 5 K isentropes, using DBSCAN to cluster overturning contours connects nearby contours into single instances of Rossby wave breaking. If this was not done, an RWB instance of e.g. 20 K overturning gradient would be counted as multiple, partially overlapping, RWB instances. We could increase the isentrope spacing to 20 K, but this would result in pointlessly ruling out smaller-magnitude RWB and in still potentially counting large-magnitude RWB multiple times. Therefore using DBSCAN allows us to seek for RWB without defining any specific strength for RWB (except for a lower limit) and without specifying which isentropes must be overturned for RWB. We have added the following motivation for the use of DBSCAN to the manuscript:

> Clustering the contours instead of counting each individual 5 K or 10 K contour as an instance of RWB ensures that each RWB at a given time step is counted only once, regardless of its magnitude.

Comment 4

> Lines 227-229: Could the authors explain in a few words the "false discovery rate control method" in the context of their study?

Response:

Repeating statistical tests, as we do over grid cells, eventually results in the null hypothesis being falsely rejected in a number of grid cells. Setting a false discovery rate of 5% means that false null hypothesis rejections are expected to occur in at most 5% of significant grid cells. In practice, this is done by calculating the t-test p-values, sorting these in ascending order and selecting a maximum significant p-value based on the desired false discovery rate and the distribution of the p-values. The motivation for applying false discovery rate control, and a more detailed description of this method, have been added to the manuscript.

> As repeating statistical tests in this manner will result in the null hypothesis being falsely rejected in a number of grid points, the false discovery rate control method (Benjamini and Hochberg, 1995; Wilks, 2016) is applied. We use a false discovery control level of 5 %: this means that the null hypothesis is expected to be falsely rejected in less than 5 % of the significant grid cells, and prevents overinterpretation. In practice, a maximum threshold p-value for rejecting the null hypothesis is selected based on the desired control level and the distribution of the

sorted p-values calculated for each grid cell.

Comment 5

Line 275: It is not clear to me that "over the central and East Pacific, the maximum in zonal wind appears to weaken and shift east". Weakens, fine, but the maximum shifting east, how can you know?

Response:

Thank you for pointing this out. There is an error in Figs. 4-9 of the preprint: instead of plotting zonal wind and its changes at 250 hPa, they are plotted at 350 hPa. This means that the Baseline feature referenced in this sentence (a subtropical jet between about 180-120°W) was not visible. The magnitudes of the changes also increase slightly at 250 hPa compared to 350 hPa. Otherwise we do not find major differences. These figures have now been corrected to show changes at 250 hPa, and the colourmaps used have been changed to a cool-warm colour scheme (per the request of reviewer 3). We have carefully gone over the associated text to ensure that everything is consistent. Additionally, for clarity, the Baseline wind contour spacing in Figs. 4-9 is now the same as in Fig. 3. The captions of the figures have been edited to reflect this.

Comment 6

Lines 285-286: "the eastward shift of the local jet is slightly weaker": What do the authors mean with "local jet"? And the shift is weaker than what? Please precise.

Response:

The "local jet" references the Pacific jet in DJF in the SST_{SSP585} simulations. The eastward shift of this jet is weaker than what is observed in the SSP585 simulations. We have changed the wording and replaced the “local” jet with the more specific “Pacific” jet, and added that the comparison is with the SSP585 Pacific jet.

> Over the Pacific, Fig. 6e-f shows that the eastward shift of the Pacific jet is slightly weaker than that of the Pacific jet in SSP585, reflecting the smaller decrease in AWB frequencies there.

Comment 7

Have the authors considered using the zonal wind at a lower pressure level than 250 hPa in order to see how the eddy-driven jet changes are linked with wave breaking changes? The zonal wind at 250 hPa sometimes mixes the sub-tropical jet with the eddy-driven jet such as the North Pacific and Western North Atlantic. It seems that previous studies found a better agreement between low-level (eddy-driven) jet changes and wave breaking changes. See, e.g., Rivière (2009), Fig. 10 in Barnes and Polvani (2013), who used a pressure-weighted average of the 850 and 700-hPa zonal wind, and the last supplementary figure in Michel et al. (2021).

Response:

Thank you for the suggestion. We have added an appendix with figures showing zonal wind changes at 700 hPa in OpenIFS and EC-Earth. This level should represent changes to the eddy-driven jet better than the 250 hPa level. The following text has been added to discuss these figures:

In reference to SSP585 zonal wind changes in DJF:

> A similar eastward acceleration, albeit smaller in magnitude, is apparent at 700 hPa (Fig. A1), a height more representative of the Pacific eddy-driven jet.

In reference to zonal wind changes in SIC_{SSP585} in DJF:

At 700 hPa, the changes in zonal wind (Figs. A1-A2) are also insignificant but appear to largely oppose the significant effects of SST in SST_{SSP585}.

In reference to JJA zonal wind changes in SSP585, in Discussion:

Additionally, Fig. A2a-b show that over the Indian Ocean, the low-level Somali jet shifts significantly eastward in both the SSP585 and SST_{SSP585} simulations. Bhatla et al. (2022) have also found a significant weakening in lower tropospheric zonal winds over the Arabian Sea and Bay of Bengal, where parts of the Somali jet are located. This change has general implications for the Asian monsoon, but a direct interpretation of the effects on RWB is that the eastward acceleration of the Somali jet also moves an area of low-level cyclonic vorticity towards the longitudes of the Baseline Pacific AWB surf zone. The effect that this may have on AWB is difficult to quantify: a similar shift in zonal wind is at least not visible at 250 hPa (e.g. Fig. 5e-f).

Comment 8

Lines 302-303: I find that the CWB frequencies changes in SIC_{SSP585} look closer to the changes in SSP585 than in SST_{SSP585}. Don't you think so?

Response:

Thank you for pointing this out, the labels were mixed up. This has been corrected as follows:

The SIC_{SSP585} CWB frequencies (Fig. 8c-d) increase by a similar magnitude and over similar areas as in SSP585 (Fig. 4c-d), although the changes in SIC_{SSP585} are not statistically significant.

Comment 9

Lines 316-317: Could the authors rephrase this sentence? I do not understand it.

Response:

Upon further reflection, we have decided to remove this discussion point from the manuscript. The intent was to emphasise the point that the binary detection method does not separate between small and large changes. However, when applying the method on large amounts of data such as our simulations, the RWB number should be sensitive to small changes in the atmosphere only if the distribution of RWB intensity is very narrow and has a mode close to the threshold value (10 K). As this is unlikely to be the case over large areas, we think that addressing this issue in the manuscript is not relevant.

Comment 10

Lines 380-382: Please precise the season in which there is "a reduction in the frequency of blocking".

Response:

Blocking frequencies have been shown to decrease in both DJF and JJA. In addition to the references already mentioned in the text, this is supported by e.g. Matsueda and Endo (2017). This reference has been be added to the revised manuscript along with the following edits to the text:

> To the extent that RWB can be considered analogous to atmospheric blocking, it supports our findings that a reduction in the frequency of blocking has been discovered consistently during both DJF and JJA (Matsueda and Endo, 2017; Woollings et al., 2018). This has also been found in CMIP6 model simulations of the SSP5-8.5 scenario (Lohmann et al., 2024).

Comment 11

Line 422: “by about .”: Please finish the sentence.

Response:

Thank you for pointing this out, this has been fixed in the manuscript.

> The SST_{SSP585} experiments show that in winter, North Pacific AWB is reduced by about 50%.

Technicalities:

Comment 12

- Line 11: the primary change → the primary changes
- Line 39: is → are
- Line 139: conditions to which → conditions which
- Line 153: modify → modified
- Line 281: there is are no . . . differences → there are no . . . differences
- Line 370: $SSP585_{SST}$ → SST_{SSP585}
- Line 394: of → on
- Line 425: SSP should not be in italic.

Response:

We have revised the manuscript as suggested in these comments.

References

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