Authors' Response to Reviewer 3

General Comments. Overview: This manuscript explores both how well two atmosphere-only GCMs capture Rossby wave breaking (RWB) in the historical period and how RWB surf zone will change in response to SSP5-8.5 forcing. They further break down the future change into changes broadly due to sea surface temperature (SST) changes and sea ice cover (SIC). They use a dynamic tropopause-based algorithm, which is the perfect application for a future changes paper given the projected varying changes in the height/pressure level of the tropopause in response to anthropogenic forcing. Their results show that the model reasonably replicates RWB with fidelity, and that future changes in RWB occurrence are far more sensitive to SST changes rather than SIC changes. The paper is well written and the figures are generally clear. There are some minor changes I'd suggest to the authors prior to final publication, but I commend them on a concise and clear study and manuscript.

Response: We thank the reviewer for their detailed comments that have helped improve the quality of the manuscript. 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.

General comments:

Comment 1

Discussion and differentiation of North Pacific and North Atlantic jet: I thought the authors generally did a good job of focusing on changes in the jet and RWB across the two dominant surf zones/ocean basins (North Atlantic and North Pacific). I felt that both the Introduction (eg. around lines 87-98) and Discussion would benefit from a bit more nuanced discussion of the differences between the jet mechanisms and interpretations across the two basins. For example, they made clear that the future changes for the two basins have different levels of confidence (eg. more confidence in jet shifts in the N. Pacific rather than the N. Atlantic) but didn't get into much detail about why this is the case. Given that, particularly in the winter, the North Pacific jet often acts as a superposed subtropical and eddy-driven jet (that is more zonal in nature), while the North Atlantic is generally an eddy-driven jet (that tilts with latitude), I felt this discussion warranted a bit more careful detail on differentiating the two (and how proposed mechanisms may be impacted by the differences).

Response:

Thank you for the comment. We have added nuance to the discussion by adding an appendix with two figures showing zonal wind changes in DJF and JJA at 700 hPa. Interpretations of these figures have been added to the manuscript as follows:

DJF, for SSP585:

> A clear eastward shift in the North Pacific jet stream can be observed in the 250-hPa zonal wind, as shown in Fig. 4e-f. The zonal wind strengthens by up to 9 m s^{-1} in both models, although over a larger area in OpenIFS. 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.

DJF and JJA for SIC_{SSP585}:

> 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}.

We are not certain which part of the text the reviewer is referencing with regards to the point that changes over the Pacific are more certain than changes over the Atlantic. We state that the changes over the Pacific are larger in magnitude particularly in DJF, and hence use the DJF Pacific often as an example.

As for more comprehensive discussion on the jet mechanisms and changes, we have edited and rearranged the paragraphs on lines 87-106 as follows:

> As the previously listed references suggest, RWB and the weather events associated with it are very sensitive to future changes in the jet streams, which on the other hand are also affected by RWB. On a zonally averaged level, it is estimated that the mid-latitude jet streams will experience a poleward shift by the end of the century (Woollings and Blackburn, 2012; Barnes and Polvani, 2013; Simpson et al., 2014). This finding is however disputed particularly in the Northern Hemisphere, where substantial spatial variability in the response of the zonal circulation to climate change has been found (Simpson et al., 2014; Grise and Polvani, 2014; Matsumura et al., 2019; Harvey et al., 2020). This variability has been attributed to e.g. SST gradients associated with ocean currents changing in ways that differ between oceanic basins (Matsumura et al., 2019), competition between the effects of tropical, Arctic and mid-latitude warming as well as the North Atlantic warming hole (Oudar et al., 2020), and differential warming on the eastern and western sides of the tropical Pacific (Oudar et al., 2020). These effects are further complicated by feedbacks resulting from jet position (Zhou et al., 2022). Future changes to the Northern Hemisphere jet stream are therefore uncertain and diverse. In reanalyses, the winter Atlantic eddy-driven jet has been discovered to have already accelerated

in the recent decades in a way not replicated by climate models (Blackport and Fyfe, 2022), and is projected to also become narrower with further acceleration (Harvey et al., 2020; Oudar et al., 2020). In the boreal summer, a slight poleward shift is observed over the North Atlantic in CMIP6 simulations (Harvey et al., 2020). Over the Pacific, the eddy-driven and subtropical jet are often merged at upper levels. The lower levels, where only the barotropic eddy-driven jet is observed, have been found to exhibit a slight poleward shift with no clear changes in magnitude (Ossó et al., 2024), while on the upper levels, the jet shifts poleward on the West Pacific and equatorward and eastward on the East Pacific (Harvey et al., 2020).

The effects of sea surface warming have been found to be more influential to the jet streams than direct radiative forcing (Grise and Polvani, 2014; Matsumura et al., 2019). On the other hand, the effects of the rapid warming of the Arctic (Arctic Amplification) have been studied extensively without a clear consensus on whether or how it may affect weather in the mid-latitudes (Overland et al., 2015; Blackport and Screen, 2020; Yin et al., 2025). One manifestation of Arctic Amplification is the reduction of sea ice cover (SIC), which CMIP6 models estimate to result in ice-free conditions in September being reached before 2050 (Notz and Community, 2020).

Barnes and Hartmann (2012) find that changing the latitude of the jet streams poleward eventually results in reduced frequencies for both AWB and CWB. Rivière (2011) examines the interactions between RWB and jet latitude in idealised simulations and finds that enhanced tropical warming causes a poleward jet shift associated with AWB becoming more common. Takemura et al. (2021) study the Pacific and also find reduced RWB frequencies, which they attribute to shifts and acceleration of the local Asian jet due to sea surface temperature (SST) warming inducing changes in the Asian monsoon circulation. The spatial variability of the jet response implies that the response of RWB will also be basin-dependent, but to the authors' knowledge, this has not been previously studied at a hemispheric scale. Studying the effects of SST and SIC changes on the tropospheric circulation separately from other factors allows quantifying the response of RWB to these consequences of global warming.

The jet stream changes are also mentioned in the discussion, where we have clarified the following sentence to not imply that the changes in the Pacific and Atlantic basins are similar to one another:

> In CMIP6 models, the Pacific and Atlantic jet streams have been found to respond to warming with a similar spatial pattern as shown in our results, although the magnitude of the changes differs (Harvey et al., 2020).

Additionally, for mechanisms of the changes, the following sentence has been added:

> Mechanisms for this are e.g. upper tropospheric tropical warming shifting the area of maximum baroclinicity equatorward as well as influencing teleconnections with higher latitudes (Oudar et al., 2020), and effects on oceanic currents influencing SST gradients at midlatitudes (Matsumura et al., 2019).

In reference to zonal wind changes over the Pacific and Indian Ocean in JJA:

> Zhou et al. (2022) also note that a weak jet is more likely to experience an equatorward shift caused by tropical warming, while a strong jet tends to be pushed poleward by synoptic eddies: this suggests that in summer, the Asian jet is more susceptible to tropical warming and it may move equatorward also for this reason. 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 2

Discussion of model experiments: I completely understand (and support) the author's decision to reference the Naakka et al. 2024 paper for details on the experiments. This said, I think it would benefit the readers to have a bit more detail here on one particular part of the experimental design: How the SSTs were handled in the SICSSP585 experiment. Though one could dig into the provided reference for detail, I think it would be helpful to throw a sentence or two into your manuscript about how the SSTs evolved/were prescribed from the historical period (presumably under ice cover) when the model was in a reduced/removed ice scenario (seasonally dependent). I think this would help because it's generally easy to visualize how future SSTs project (because it's provided in figure 1) but much harder to visualize what the baroclinic zones look like with future SIC but historical SSTs.

Response:

We agree with the reviewer, and multiple sentences on the handling of SST values near sea ice in the SICSSP585 experiment have been added in Section 2.1.

In particular, historical SSTs values used in the SICSSP585 experiment depend on the sea ice concentration in the Baseline simulation. If sea ice concentration values are lower than 1, then historical SST values are provided by the ACCESS-ESM1.5 model. If the historical sea ice concentration is 1, then the SST values in the SICSSP585 experiment are set to the melting point of seawater (approx. -1.8 °C), where there is reduced sea ice. This results in skin temperatures which are slightly lower than the melting point of freshwater, where sea ice is removed.

In areas where sea ice is removed in the SIC_{SSP585} simulations, the SSTs are kept at the historical values, as in the Baseline experiment. This results in skin temperatures that are slightly lower than the melting point of freshwater. 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

Possible supplemental figures: It might be beneficial to try and create a set of maps that show the difference between the experiments and the full future model as a supplemental to help clarify some of the discussion. For example, a 6-panel that is essentially fig. 6 subtracted from fig. 4 (or fig. 7 – fig. 5), etc. I'm don't think it's necessary in the main body of the paper, but I think it would help clarify some of the differences you've discussed.

Response:

As stated in the beginning of Section 3.3, the differences between the SSP585 and SST_{SSP585} experiments are not different at a level that is statistically significant. The SIC_{SSP585} experiment on the other hand is not significantly different from the Baseline, so differences between SIC_{SSP585} and SSP585 would only reflect changes that we already show. The differences between SSP585 and SST_{SSP585} are discussed since they help demonstrate that the magnitudes of changes in zonal winds and RWB frequencies appear to be connected at least in the case of the changes over the Pacific in DJF: changes in both RWB frequencies and zonal wind are less intense than in the SSP585 experiment. Beyond this, we do not see use for these figures.

Specific comments:

Comment 1

Lines 29-39: It may be helpful here to cite the recent work by Tamarin-Brodsky and Harnik (DOI: 10.5194/wcd-5-87-2024) in this section. It's new (and understandable you didn't have it here), but it's a nice extension of the discussion you have here.

Response:

Thank you for the suggestion, we have added discussion of this reference to the manuscript as follows:

> Studying reanalysis data, Tamarin-Brodsky and Harnik (2024) found that over 60% of surface weather systems over the North Atlantic are at some point associated with RWB. From this weather system point of view, RWB can result from interactions between troughs and ridges. A cyclone can be associated with AWB when a ridge is building upstream of the upper-level trough, while cyclonic wave breaking happens when the ridge is building downstream of the trough. With an anticyclone as the primary weather system during wave breaking, AWB occurs on the equatorward side of the jet when a trough intensifies downstream, and CWB on the poleward side of the jet when a trough intensifies upstream relative to the ridge associated with the anticyclone. The barotropic conversion of eddy kinetic energy to the kinetic energy of the mean flow associated with RWB can result in acceleration and shifts in the latitude of the jet stream, poleward (equatorward) for AWB (CWB) (Thorncroft et al., 1993; Rivière, 2009; Bowley et al., 2019b).

Comment 2

Lines 48 (and elsewhere): In general, best practice for citations is to list in chronological order.

Response:

We have changed the order in which the citations are listed to chronological.

Comment 3

Lines 64-66: I found the start of this sentence a bit hard to interpret – you may want to rework.

Response:

Thank you for the suggestion, we have edited the sentence as follows:

> RWB is usually defined as the reversal of a particular upper-troposphere gradient compared to climatology, but the variable considered as well as the threshold value for the gradient strength and the methods for calculating the gradient reversal vary.

Comment 4

Line 140: When you state that 'this improves the detection ...', it's unclear if the 'this' refers to the 4.4 K or 2 K. Also, a brief clarifier on how this improves signal detection would be helpful.

Response:

We have clarified that the stronger forcing compared to previous studies improves detecting an atmospheric response against internal variability.

However, the key difference is that the CRiceS simulations provide boundary conditions which correspond to a +4.4 K global warming, while the PAMIP forcing is equivalent to a +2 K global warming. The larger forcing in the CRiceS simulations compared to previous studies improves the signal detection against internal variability by increasing the magnitude of the atmospheric response.

Comment 5

Figure 1: Given that the focus of this paper is the Northern Hemisphere, it might be helpful to cut this figure to only the hemisphere of focus.

Response:

Our focus is indeed on the Northern Hemisphere, but tropical warming has been found to be a very important factor to changes in the midlatitudes (e.g. O'Gorman, 2010; Butler et al., 2011; Zhou et al., 2022). Keeping the figure as is allows us to show the distribution and magnitude of tropical warming applied in our experiments; this is speculated on in the discussion (lines 340-345) as a cause for some of the changes we observe.

Comment 6

Section 3.1: There were a few 1-2 sentence paragraphs here. I would consider trying to collapse these short 'paragraphs' into other ones. For example, lines 190-191 could be combined with the next paragraph, and the paragraph ending on line 221 could be combined with the paragraph starting at line 222.

Response:

We have merged the paragraphs as the reviewer suggested.

Comment 7

Lines 203-205: Does this result match to the climatologies of other studies?

Response:

This references our result of DJF cyclonic wave breaking being more common over the North Pacific than over the North Atlantic. E.g. Bowley et al. (2019a) show a similar result (their Fig. 6a), and Strong and Magnusdottir (2008) also have a similar result. We have added a mention of this to the manuscript:

> Similar results have been found by e.g. Strong and Magnusdottir (2008) and Bowley et al. (2019a).

Comment 8

Line 231: I understand using the 250 hPa wind (a lot of studies do) – but if you have the dynamic tropopause wind in your dataset, why not use that instead to more perfectly match your RWB identification level? This in particular could be beneficial in the JJA analysis given the elevated warm season tropopause.

Response:

Unfortunately we do not have access to wind components on the dynamical tropopause, so 250 hPa zonal wind is used as it is a commonly considered variable when studying changes to the upper atmosphere and as the 250 hPa level is often located near the tropopause in the midlatitudes.

Comment 9

Lines 237-239: I may have missed this later in the manuscript, but if you haven't discussed a bit why this difference occurs between the two models, it would be helpful to do.

Response:

There are extensive differences between OpenIFS and EC-Earth as models. As stated in the Methods section of the preprint, the atmospheric components of the models are based on different cycles of IFS (43r3 for OpenIFS, 36r4 for EC-Earth), meaning that the models are differentiated by seven years of development. Another notable difference is that OpenIFS only uses climatologically averaged aerosols while EC-Earth incorporates interactive aerosols and atmospheric chemistry. Therefore ascertaining the causes for differences between the models would require either speculation or analysis beyond the scope of this paper.

Comment 10

Lines 242-243: It appears the zonal wind also has an equatorward shift (in addition to the eastward shift) for the North Pacific. This has ramifications for the occurrence of AWB (equatorward shift → less AWB). It might also be linked to the enhanced CWB (either more CWB due to the equatorward shift, or an equatorward shift due to nudging via momentum flux by the CWBs).

Response:

Thank you for the comment, there is indeed an equatorward shift in zonal wind over the DJF Pacific. We have added a mention of the shift and its possible implications to the manuscript as follows:

The equatorward shift of the jet over the East Pacific could be interpreted as either the cause of increased (decreased) CWB (AWB) or the effect of the RWB frequency changes in momentum fluxes.

Comment 11

Line 272: I'm not entirely sure I see the increase in speed over East Asia – in particular in EC-Earth – but I do see the strong signal over the western North Pacific. I might suggest focusing more on that.

Response:

Thank you for the comment. The changes in EC-Earth are shifted east compared to OpenIFS over this area. This wording has been clarified to emphasise that the locations of the changes differ between models:

Overall, the Asian jet shifts southward and increases in speed over East Asia and the Pacific in OpenIFS and mainly over the Pacific in EC-Earth. The western flanks of these changes correspond in location with the largest relative decreases in AWB frequencies in the respective models.

Comment 12

Lines 273-274: It's unclear to me whether this is actually an eastward shift in the exit region (the differences on the southern flank of the jet extend just as far east as the jet), but instead may be more due to a shift in the entrance region (eastward and southward) coupled with a more equatorward jet (which would reduce AWB). I think the eastward (and southward) shift in exit region is more confined just to the winter months.

Response:

Firstly, we want to note that 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. The magnitudes of the changes increase slightly at 250 hPa compared to 350 hPa, and in JJA, a subtropical jet is visible in the Baseline contours over the eastern Pacific. 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 (as requested in Comment 14). 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.

To address the comment, in OpenIFS, the 20 ms^{-1} isotach is confined over the Asian continent, but in EC-Earth it extends across 180°E. The equatorward shift is more obvious in both models and is also visible on lower pressure levels (see the new 700 hPa zonal wind figures in the appendix). We have edited the manuscript to discuss the changes more on this basis rather than an eastward extension:

> Overall, the Asian jet shifts southward and increases in speed over East Asia and the Pacific in OpenIFS and mainly over the Pacific in EC-Earth. The western flanks of these changes correspond in location with the largest relative decreases in AWB frequencies in the respective models. An eastward shift in the location of

the jet exit in OpenIFS may be related to the increase in AWB frequencies over the Central Pacific, downstream from the new location of the jet exit.

Comment 13

Lines 298-301: It may be beneficial to shift the discussion to changes in RWB in the peripheral Arctic seas, where we see the greatest shifts in RWB (ie. a localized changes). This also plays right into the discussion point on the extremely localized impacts of SIC changes on lower tropospheric temperatures, which may be impacting local baroclinicity, vertical wave propagation, etc. in just these regions.

Response:

The RWB frequency changes that agree between models are not confined particularly far north, even if they are located poleward of 20°N (AWB) and 40°N (CWB). As these changes are also statistically insignificant, we would prefer to not add this to the manuscript, although it is entirely possible that highly localised effects are at play here.

Comment 14

Figures 4-9: I had a hard time with the color bar for panels e and f. It might be beneficial to use a cold to warm color bar rather than a cold to cold bar. At times it was hard to identify increases or decreases.

Response:

We have changed the colourmap to use a cool-to-warm range.

Comment 15

Lines 304-306: More an observation than something that needs changing (in particular given the lack of statistical significance) – it almost looks like a standing wave response in the summer CWB for the SIC experiment. Interesting given the importance of some of these seas for generating standing wave patterns that could be interpreted as RWB!

Response:

Yes, we also consider that to be an interesting detail. However, as the change is not statistically significant, we prefer to refrain from commenting on it in the manuscript.

Comment 16

Lines 325-328: I would lean more into the discussion on changes in the jet and causality here (and possibly in the introduction). There is an extensive body of literature exploring changes to the jet in a variety of models and experimental designs that would be beneficial to lean into here.

Response:

We have added discussion of the mechanisms influencing the jet streams to the introduction, as detailed in Comment 1.

Comment 17

Lines 372-373: Consider looking into Woollings and Hoskins 2008 (DOI: 10.1002/qj.310) here to link the weakened flow over the North Atlantic to benefiting CWB. Their study was for winter rather than summer, but there may be helpful information there.

Response:

Thank you for the suggestion. Woollings and Hoskins (2008) discuss high-latitude blocking as a result of CWB occurring simultaneously over the Pacific and Greenland/North Canada as a result of the deformation of a polar trough over Canada. However, the largest signal we observe in JJA is located directly over the North American continent. We do not see a clear link between this result and Woollings and Hoskins (2008), so think it best not to speculate based on this.

Comment 18

Lines 379 and 388: I found the starts of both of these paragraphs to be a bit informal – consider reworking.

Response:

Thank you for the suggestion. The sentence at the start of the first paragraph has been reformatted as follows:

> Changes to e.g. blocking, a phenomenon closely related to RWB (Pelly and Hoskins, 2003), are most commonly studied from climate model ensembles (e.g. de Vries et al., 2013; Woollings et al., 2018; Trevisiol et al., 2022).

The second sentence has been reformulated based on this comment and comment 20 (see that comment for the changes).

Comment 19

Lines 384-385: There are a few more things that play a role in blocking representation in models (eg. orography) – you may want to consider adding a bit more to the discussion here.

Response:

Thank you for the suggestion. We have added error sources for blocking studies to the text and clarified that the definition of blocking is not the only issue:

> The underestimation of blocking has been attributed to many causes, e.g. insufficient model resolution resulting in poorly represented orography and errors in the atmospheric mean state (Berckmans et al., 2013), and issues with the parametrisation of diabatic processes such as convection and warm conveyor belts (Hinton et al., 2009; Maddison et al., 2020; Dolores-Tesillos et al., 2025).

Comment 20

Line 388: ‘contested’ is fine here, though I always find it make it sound a bit more negative in nature. Consider ‘the past and future trends of which are an area of diverging perspectives’.

Response:

Thank you for the comment. Based on this and comment 18, the sentence has been reformulated as follows:

> In addition to atmospheric blocking, trends in the measure of jet stream waviness is another research topic closely related to AWB (Martineau et al., 2017) on which no clear consensus has yet been reached, in part due to results depending on the chosen methodology (Barnes, 2013; Martin, 2021; Yamamoto and Martineau, 2024).

Comment 21

Lines 421: I'm not sure you can entirely say this first sentence with the experimental design. I think you've shown that SST changes, relative to SIC changes, are the dominant part of the total signal, but I'm not sure you've shown that the changes in boreal winter can be exclusively attributed to SST (and nothing else in the system).

Response:

Thank you for pointing this out, we agree completely. The wording of this sentence has been changed to reflect that although our results show significant effects only from SST, we cannot conclude that SIC would under no circumstances have any impacts:

> In our experiments with future SSTs and SIC, only SST changes produce statistically significant effects on RWB frequencies and zonal wind at 250 hPa. We however cannot rule out the effects of SIC, as a longer simulation may be required to establish a significant signal (Peings et al., 2021).

Technical corrections:

Comment 1

Handling of spaces after certain values – this might just be the format for this journal (every journal is different), but I found the gaps between degree symbols and directions (eg. 120° W) as well as the gaps between values and percentage signs (eg. 20 %) to be too wide/awkward in places given the text formatting. I would consider removing the spaces.

Response:

These are to our understanding in accordance with the journal's guidelines, so we think it best to leave correcting this for the typesetting phase.

Comment 2

Line 422: You're missing the end of your sentence here.

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 %.

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