Response to Reviewer 1

We sincerely thank Irina Rudeva for her constructive critique and detailed feedback of our manuscript. She addressed important and valid points that helped us to improve the manuscript. We provide detailed responses with our comments and changes in blue.

MAJOR COMMENTS

1. The finding that cyclones play a secondary role in forming strong sensible and latent heat fluxes is only true for the low-pressure systems which have their centres in the selected areas, i.e. right over the oceanic currents. However, anomalous turbulent fluxes may not be associated with the cyclones with centres in the selected boxes. Tilinina et al. (2018; https://doi.org/10.1175/MWR-D-17-0291.1) showed that extreme fluxes over the Gulf Stream are linked to regions of cyclone-anticyclone interaction (usually associated with strong winds and large air temperature anomalies) when they are located above the ocean current. The finding by Tilinina et al. implies that the atmospheric circulation plays an important role in creating strong turbulent fluxes. While systems that have their centres within the selected boxes may, indeed, not be associated with strong air-sea fluxes, I believe that the paper should focus on a different subset of cyclones, i.e., those that are located ~ 1000-3000 km to the north or north-east of the Gulf Stream or Kuroshio). This is in agreement with the area of the largest changes of cyclone activity shown in Fig.4.

We thank the reviewer for this comment and agree with her that the attribution to only cyclones in the box was indeed restrictive. We thus complemented our previous analysis with one in which we considered the pertinent variables occurring within a radius of 750 km (consistent with a maximum cyclone circumference in Wernli and Schwierz (2006) and the analysis of Rudeva and Gulev (2011)) of any cyclone over its entire lifetime in the ocean basin, irrespective of its propagation and its location of maximum intensification. We adjusted our manuscript and present the new results in the following Sections: Introduction (lines: 96-99), Data & Methods (lines: 165-171), in subsections 3.4.1 (lines: 316-319), 3.4.2 (lines: 372-375), 3.4.3 (lines: 386-401), 3.4.4 (lines: 417-423), and in the Conclusions (lines: 456-470). We also changed figures 7-10 (c-d, g-h) and their associated captions, respectively. The results arising from this new analysis are in line with our previous results and support our previous conclusions. The only exception is Figure 10 (g-h), in which we now find the wind speed composite at 300 hPa that is not associated with cyclones to better resemble the climatological differences in the North Pacific, which is now in line with the results for the North Atlantic. In addition, we conducted our analysis within different radii around the cyclone centers (500 and 1000km) and present the results for the surface heat fluxes and the different types of precipitation in Appendix B (Figures B1-B4).

2. I do not feel comfortable with the idea of using SST fronts from CNTRL for analysis of the smoothed runs. I believe that fronts should be objectively identified in all runs. This is particularly important in the Pacific, where SST fronts can hardly be seen in the SMTHK experiments. As the classification of cyclones in this paper is based on the location relative to the SST front, in those cases when an SST front in SMTH cannot be detected, the type of cyclones cannot be defined. That said, given my first comment on the subset of cyclones that may be particularly affected by the SST gradient, I am not sure that a classification based on the location of cyclone centres relative to the ocean current is particularly important. Fig. 5 shows that there are hardly any statistically significant differences between those types, not to mention the differences between SMTH and CNTRL runs.

We thank the reviewer for her comment that our rationale for this choice has not become apparent in the previous version of the manuscript. We use this classification only to be able to compare cyclones with a geographically similar genesis location and track across the experiments. We adapted the manuscript accordingly to point this out more clearly (please refer to subsection 2.4 (lines: 158-163)). Having said this, we did attempt to objectively identify the SST fronts for the SMTH experiments, but this fails because the SST gradient is very homogeneous over a large region such that the detection of the SST front location is mostly determined by numerical noise rather than a defined maximum in the SST gradient.

3. Finally, it will be interesting to assess how much changes in heat fluxes, precipitation, etc. scale with a change in the SST gradient. It is mentioned multiple times that smaller reduction in the SST gradient in the Kuroshio region led to smaller corresponding changes in the atmospheric circulation. The question I want to raise here is if similar reductions in the SST gradient in those basins lead to comparable response. Or, alternatively, how stronger/weaker smoothing affects the same region.

We thank the reviewer for raising this issue. As mentioned in the "Data and Methods" section, we use data from model experiments that were provided by JAMSTEC. For this reason, we unfortunately do not have the capability to run additional experiments to estimate how stronger/weaker smoothing affects the same region (the Gulf Stream or the Kuroshio region). Nonetheless, we thank the reviewer for sharing with us this interesting idea, which is an interesting potential analysis in the future, when we will be capable to execute more/different experiments.

OTHER COMMENTS

We thank the reviewer for all the "other/minor comments" she raised, which we considered when editing the manuscript. Our responses to each comment are following:

Title: In the current version of the manuscript, it is, indeed, shown that the atmospheric conditions change stronger in the absence of cyclones over the ocean currents. However, as I said in the above, if more analysis is done on cyclones in the storm track areas, the title may need to be changed.

With the new analysis considering the entire ocean basins, we confirmed our previous findings and thus the choice of the title. We just changed the order of some words in the title, between the previously submitted and revised version of the manuscript. The current title is: "The Gulf Stream and Kuroshio SST fronts affect the winter climatology primarily in the absence of cyclones".

l.90: Instead of just saying 'to the questions raised above', it is worth repeating the main questions here.

We thank the reviewer for this comment and rephrased the respective sentence (please refer to lines 99-100).

Data: I could not find the time resolution of the data used.

We thank the reviewer for highlighting this missing information. We analyse the AFES output on a 0.5° horizontal grid at 6 hourly intervals. We added this information in subsection 2.1 (line: 109).

l.97: Why the period of integration ends in 2001? Considering the coarse SST resolution in ERA-Interim prior to 2002, selection of the time period needs to be justified.

As indicated previously (please see our response to your Major comment 3), the model experiments we use were conducted for the period 1982-2001 and this is why we are not able to extend this time step after 2002. But this is not a caveat, as the AFES simulations do not suffer from the same limitation as ERA-Interim, but use high-resolution SSTs as boundary conditions for the entire time period.

l.104: Explain what is meant by "1-2-1 running mean"

It is a three-point filter with the weights 0.25, 0.5 and 0.25. The 1-2-1 filter has a sharp cutoff, so that unwanted frequency components are effectively removed. This filter was applied to the data prior to JAMSTEC providing it to us. For further information please see Kuwano-Yoshida and Minobe, 2017 (https://journals.ametsoc.org/view/journals/clim/30/3/jcli-d-16-0331.1.xml?tab_body=fulltext-display). We added the additional information in the revised version of the manuscript (please refer to subsection 2.1, lines: 115-117)

l.114: Give a more detailed description of how the jet was detected.

We thank the reviewer for letting us clarify this issue. To assess the potential impact of the SST smoothing on the upper levels, we detect the position of jet following the algorithm of Spensberger et al. (2017). The algorithm detects jets by their axis, the line of maximum winds that separates cyclonic from anticyclonic wind shear. The algorithm requires the wind maximum to be well-defined, but does not impose a strict minimum wind speed (Spensberger et al., 2017). We added this information in subsection 2.2 (lines: 131-134).

l. 123: following a comment above, 3 consecutive time steps for the minimum life time of 12 hr comes from nowhere. I'd mention here that 12 hours is less than more often used threshold of 24 hours (i.e., five 6-hour time steps), applied in Neu et al. (2013; https://doi.org/10.1175/BAMS-D-11-00154.1) and adopted in many recent studies on extratropical cyclones.

We thank the reviewer for letting us clarify this issue. Following Neu et al. (2013), we indeed followed the same and often used technique, considering cyclone tracks with at least five 6-hour time steps, but in addition we required cyclones to have three 6-hour time steps in the Gulf Stream or the Kuroshio region in order to classify the cyclones into different categories (C1-5). Asking for five 6-hour time steps only in the "box"/area of interest would significantly reduce the total number of tracks, as we are particularly interested (and applied the respective criterion in the detection algorithm) in "strong" cyclones, including rapidly developing and propagating systems. Given the geographically restricted region and considering that the specific areas are regions of cyclogenesis, a further restriction would downgrade the significance of our findings. We added this missing information in the manuscript, in subsection 2.3 (lines: 143-144) and in subsection 2.4 (lines: 152-154).

l.124, 144: I presume that you require that the cyclone centre, not just any point of the cyclone area, passes over the Gulf Stream or Kuroshio regions?

The reviewer suspected correctly. However, this method is not valid anymore, due to the new analysis we conducted (as described in detail to our response in Major Comment 1).

l.159: Also refer to Fig. 2a,b for the SST gradient.

We thank the reviewer and added this missing information in subsection 3.1 (lines: 186-187)

Fig. 3: what are the units used to show the distribution of the SST fronts and jet axis? The caption says 'km of line/ 100km2'. It suggests that the SST front is represented by a line, not**

by an area where the SST is above a 2 or 1.25K/100km (this indicates again, that the method used to define the SST front needs to be better described in Section 2.2, see my earlier comment). Explain how you got km of line per a unit area. Why not showing the frequency of the SST values above a threshold instead? As I do not quite understand the units in Fig.3, I am not sure how to interpret higher jet distribution values over the Pacific.

The reviewer is right. We do detect SST front lines rather than regions of strong SST gradients. We require front lines in order to be able to define when cyclones cross the front. We pointed this out more clearly in subsection 2.2 (lines: 126-127).

For the climatologies and composites we normalise the occurrence of both SST front lines and jet axis lines to account for the varying area covered by grid cells. We achieve this by showing the average length of SST front line/jet axis line per unit area, hence the resulting unit of length per area. For details on the normalisation, we refer to Spensberger and Spengler (2020). To compile a climatology, we use for both the SST front and the jet axis distributions a unit of length per area, due to its independence of the grid resolution. We added this information in subsection 2.2 (lines: 135-138).

"The text says that the jet is 'stronger', does that mean that it is present more often or that it is wavier?"

If the density is more widespread, the jet is wavier or shifts more in latitude. If the density is focused, the jet is usually mostly at this location. Woollings et al. (2018) found that a more focused jet is usually also associated with the jet being stronger. Hence, we use more focused synonymously with stronger.

l. 172: How do you know that it is the jet that affects the cyclones, not the other way round? More importantly, how does this section answers the question raised at the start of the paragraph (on the impact of the upper-level circulation on cyclones)? I only see a comparison of statistics at the upper and low levels.

We thank the reviewer for this comment. The importance of the jet for intensifying cyclones has been confirmed in numerous studies (e.g., Evans et al., 1994; Schultz et al., 1998; Riviere and Joly, 2006; Tsopouridis et al., 2020b). Please refer to the Introduction Section (lines:42-48). Nevertheless, we acknowledge that the topic sentence of this paragraph was confusing and rephrased accordingly to better express the content of the paragraph, that is the climatological position of the jet stream over the two ocean basin, with respect to the position of the respective SST fronts in the two regions. Please refer to subsection 3.2 (lines: 199-200) for our change.

l. 179-187: As the Pacific jet is located further south compared to the Atlantic jet, it is hard to shift it more equatorward.

We agree with the reviewer and only document the observed differences (lines: 208-209).

l. 188-191: Again, I do not see larger changes in the cyclone activity in the Atlantic. The patterns in the Atlantic and Pacific are slightly different, but this may be a result of the different location of the SST front/jet axis, as well as difference in the shape of continents. Moreover, I see a shifted storm track activity in the Atlantic or Pacific, however, basin-wide it does not seem to be weaker as opposed to the Southern Ocean mentioned in the text.

We explicitly refer here to the Gulf Stream and Kuroshio regions (as these are indicated by a black box, respectively) and not for the Atlantic and Pacific basin as a whole. In the Gulf Stream region there is indeed a larger reduction in storm track activity ($2*10^{-6}$ km^{-2}), compared to the Kuroshio region (maximum of $1*10^{-6}$ km^{-2}). Given the fact that the SST front (despite significantly weaker in the SMTH experiments) is located at the same regions after the SST smoothing (please refer to Figure 2) and the shape of the continents is unchanged between the experiments, we relate this difference to the sharper SST smoothing, and thus a considerably weaker SST gradient in the Gulf Stream region (please compare Figure 2c with Figure 2f).

l.192: The differences in the Atlantic are also shifted between the upper and lower levels, less than in the Pacific, but in the same direction. It will be interesting to add cyclone frequencies in the middle of the troposphere (e.g., 500hPa).

We thank the reviewer for her comment. We agree with the reviewer that the differences in the Atlantic, between the upper and lower levels, are shifted in the same direction and refer to this on lines: 205-208. An additional analysis of cyclone frequencies in the mid-troposphere is beyond the scope of our study.

Fig. 5: If you keep this plot in the revised manuscript, could you estimate if the difference between lines (i.e., between mean values for the different cyclone types at every time step) are statistically significant and mark the time steps when the mean value for a given type is significantly different from the other two types?

We thank the reviewer for this comment. As the reviewer can see in Figure 5a,b, the results are statistically significant around maximum intensification (from -6h to 6h) for C2, where the 50th percentile of C2 coincides with the 75th percentile of C1 and with the 75-100th percentile (not shown in colors) of C3. Moreover, we mark that the median (50th percentile) of C2 is always above the ones for C1,3 during the 48h period in both experiments for the Gulf Stream region, indicating a clear tendency for higher intensification in C1,3, compared to C2. In the Kuroshio region (please refer to Figure 5d,e) there is a lot of overlap in the Interquartile Range (IQR) for the 3 categories and a larger variability. Nevertheless, again the median of C2 is above the ones for C1,3 before the time of maximum intensification, indicating a clear tendency for weaker intensification in C2, compared to C1,3. We thank once more the reviewer for pointing out this missing information and we added a discussion in subsection 3.3 (lines: 240-244).

l.250, Fig.6: I am not sure if Fig 6 is worth showing. As the cyclone classification in this paper is based on the location relative to the SST front, the mean SST in the cyclone area is somewhat prescribed.

As the reviewer correctly indicates, the cyclone categorisation is based on the location of cyclones relative to the SST front. However, as we explicitly refer to in lines 190-191, it is not only the SST gradient that changes between the experiments, but also the absolute SST. Moreover, the absolute SSTs are important independently, or in conjunction with the SST gradient, for the intensification of cyclones in the two regions, which we discuss in more detail in the Introduction Section (please refer to lines: 24-41). We thus decided to keep Figure 6, in which the absolute SST changes between the experiments for the different cases and regions are captured in detail and can be related with the intensification of cyclones (Figure 5).

l.256: In line with my earlier comments, the statement that weaker SST gradients in the Pacific are not strong enough to affect cyclone development, points to the fact that, perhaps, the SST threshold for the Pacific regions should be increased. Ideally, I think, they should be identical (even if the Atlantic threshold is decreased).

We thoroughly tested different thresholds to obtain reasonable SST fronts in both basins. Given the different SST gradient in the two basins (e.g., Nakamura et al., 2004; Tsopouridis et al., 2020b), if using the Atlantic threshold, we would capture almost no SST fronts in the Kuroshio region. Vice versa, with the Pacific threshold in the Gulf Stream region we would obtain many weak SST fronts that would make it difficult to identify the main SST front. We thus believe that a different threshold is necessary to accommodate the different natures of the boundary currents and SST fronts. To clarify the use of different thresholds in the two ocean basins, we adapted the text accordingly in subsection 2.2 (lines: 127-130).

l.303: As I said in the major comments, cyclones that have centres over the oceanic currents are not expected to cause strong heat fluxes.

Following the concern expressed from the reviewer in her Major Comment 1, we conducted a new analysis and found that the result is valid, not only for cyclones that have centres over the oceanic currents.

l. 307, Section 3.4.2: In line with my Major comment 1, it will be interesting to show how much precipitation changes with cyclones most affected by the SST change. (however, the increased precipitation over the ocean currents may be associated with another subset of cyclones, not those that are related to the increased heat fluxes).

Following the concern of the reviewer expressed in Major comment 1, we now include all cyclones, not only the ones with a maximum intensification in the GS/Kuroshio region and can estimate the overall contribution of cyclones after the SST smoothing in the two basins. For more details, please refer to our response in Major comment 1.

l.313: Does smoothed SST in the Atlantic lead to a reduced separation distance between the maximum in large-scale and convective precipitation? If that does not happen, then explain why smaller SST gradients in the Pacific are responsible for more collocated precipitation maxima in the Pacific.

We thank the reviewer for her suggestion, which helped us clarify this issue. Smoothed SST in the Atlantic does not lead to a reduced distance between the maximum in large-scale and convective precipitation. We thus deleted the hypothesis raised in the previous version of the manuscript. Please refer to lines 339-340 in the revised version of the manuscript.

l.323: the largest shift in cyclones occurs downstream of the selected regions, however, the precipitation changes most significantly within the black boxes. I think it should be discussed in the text.

We thank the reviewer for her comment. In the revised Figure 8 c,d, g,h, we show that a considerable fraction of the precipitation differences after the SST smoothing is not associated with cyclones. Our results are thus consistent with what the reviewer indicates, that is: the more significant precipitation change occurs in the western parts of the two ocean basins, where the largest changes in SST are observed. We discuss this in subsection 3.4.2 (lines: 357-362).

l.349: specific humidity can be affected by cyclones over the central part of the North Atlantic. Besides local change in evaporation over the ocean currents, changes in characteristics of the extratropical cyclones downstream of the Gulf Stream and Kuroshio may modify the intensity of warm conveyor belts. There is also an increase in the relative humidity in the south-east corner of the Pacific basin, most likely associated with a southward shift of the storm track.

We thank the reviewer for this comment. Overall, we found the specific humidity, which is not associated with cyclones (outside of a 750 km-radius) to better resemble the climatological differences after the SST smoothing. Nonetheless, when considering the specific humidity that is associated with cyclones (within a 750 km-radius), we indeed observe an increase in the central Atlantic and relate this with a shift to the southeast of the storm track in the SMTHG experiment. Please refer to subsection 3.4.3 (lines: 389-391).

l.395: The pattern in the Atlantic (jet, cyclone frequency) suggests negative NAO in the smoothed runs

We want to the thank the reviewer for sharing her thoughts with us. Regarding the NAO phase, we avoided such an association, as this would need a further analysis to be confirmed, and this is beyond the scope of this study.

l.400: I like the hypothesis here. However, in my opinion, the jet response in the Pacific is mainly associated with a strong increase in cyclone frequency in the Bay of Alaska (Fig.4c). A well developed cyclonic circulation will increase (decrease) the wind speed to the south(north) of its centre. This low pressure system may also be responsible for the moisture advection from the central Pacific to north-east (Fig. 9f). Interestingly, cyclones over the Kuroshio are associated with an intensified subtropical jet.

We thank the reviewer for her comment. Our results based on the new analysis indicate that the wind speed difference that is not-associated with cyclones (outside of the 750km-radius, Fig. 10d,f) resembles more the difference of the wind speed climatology (Fig.10 b,f) in both regions, compared to the upper-level wind speed difference that is associated with cyclones (Fig.10 c,g). We relate our previous findings to the restriction of the cyclones' subset used before and are confident that our new analysis (based on the reviewer's major comment 1) more completely interprets the upper-level wind speed field.

Fig. 7-10: Indicated statistical significance of the differences.

We thank the reviewer for raising this issue. In the revised version of the manuscript, we now provide the statistically significant results (>95%, based on a t-test) and present the new results in Figures 7-10 (b,f), as the reviewer suggested.

MINOR COMMENTS

l.6: I'd replace the 'intensification' with 'intensity', unless you really want to stress the process of deepening, rather than the maximum strength of the systems.

As the reviewer suspected we are particularly interested in the intensification of cyclones, rather than their intensity. We thus kept the word "intensification" in this sentence as it reflects what we want to stress.

l.14: change 'states' to 'state' &
p.1, l17: and throughout the paper: change to 'regions' as the Gulf Stream and Kuroshio are two independent regions (as opposed to, e.g., the Kara and Barents Sea) &
l376: change to '40%'

We thank the reviewer for pointing out this spelling errors. We corrected them in the revised version of the manuscript.

l.102: Introducing SMTHG and SMTHK, it is worthwhile mentioning the Gulf Stream and Kuroshio, respectively, instead of the North Atlantic and Pacific. Otherwise, it is not immediately obvious what K and G stand for.

We agree with the reviewer, but replaced it with "the greater area around the Gulf Stream" and "the greater area around the Kuroshio Extension" to avoid confusion with the terms "Gulf Stream region" and "Kuroshio region", as these are used in the manuscript. Please, refer to lines: 113-114 for the respective change.

l.145: replace "intensification" with "intensity".

The word "intensification" has been correctly used and is thus kept in the revised version. We made this choice to highlight the difference with the set of cyclones used for the categorisation/classification (as described in the subsection 2.4). For the different categories (C1-5) we indeed considered cyclones with a maximum "intensification" (and not "intensity") in the Gulf Stream and Kuroshio regions.

l.199: Perhaps, 'classification' sounds better.

We agree with the reviewer's suggestion and replaced "categorization" with "classification".

Fig. 1, caption: Remove the first '(a)' from the caption. In c (and the following figures), make clear that it is SMTHG minus CNTL. The caption should mention black solid lines.

We thank the reviewer for the comments and proceeded with the respective correction and edits in the captions.

Fig. A1, panels b and e: The range of values in the colour scale is too big and yellow shading is hard to see against the white background. The colour scale does not match either Fig. 2a or 2d, so I do not see why you chose such large range.

We understand the concern of the reviewer (that the range in Figure A1 is large), however as we want to compare the respective results between the model (AFES) and Reanalysis (Era-Interim) we believe that using the same range and colour scale is useful as it will be more straightforward for the reader to capture the differences between the two datasets by using the same range.

Fig.2, b, e: Why did you choose to show the SST gradient in 0.5K/100km instead of K/100km?

We thank the reviewer for letting us clarify this issue. As the reviewer realizes herself in the previous comment ("yellow shading is hard to see against the white background"), using K/100km instead would result in an almost white background, given the significant smoothing of the SST. We decided thus instead to show the SST gradient in 0.5K/100km and highlighted the different units used between panels a,d and b,e in both the figure and the caption.

Fig.7-10: I do not think I understand the last sentence in the captions. What kind of scaling was applied? In this case please describe in the Methods.

We thank the reviewer for the comment. After applying a different analysis, the sentence in the caption is removed.

Fig. 3: On my screen the 'orange' colour scale looks nowhere near to orange, I'd call it pale red. Perhaps, change the scale a little bit.

Following the reviewer's suggestion, we replaced the word "orange" with "pale red" in the caption.

References

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Response to Reviewer 2

We sincerely thank the anonymous reviewer for his/her feedback and comments, which helped us to improve the manuscript. We provide detailed responses with our comments and changes in blue.

(M): The introduction provides a good comprehensive overview of many aspects of GS/KE - atmosphere interaction, but fails to discuss a large body of work directly relevant to the main focus of this paper i.e. SST influence on time-mean as affected by changes in synoptic storms. The following studies have already considered contributions of storms to the mean atmospheric state in the Gulf Stream and Kuroshio regions (Parfitt and Czaja, O'Neill et al, Parfitt and Seo, Masunaga et al, a,b). The first two of these studies show that the mean state in the tropospheric wind fields and precipitation are set by extreme synoptic situations, with the latter four studies specifically highlighting that it is the atmospheric fronts (and not really the cyclones). Indeed, when you remove the atmospheric fronts from the climatology, you remove the time-mean convergence signal above the Gulf Stream and Kuroshio altogether (see Figure 2 in Parfitt and Seo) as the remaining baroclinic components mostly cancel out (see Figure 3 and Supplementary Figure 2 in Parfitt and Seo). Furthermore, atmospheric fronts contribute ~90% of the rainfall in these regions (Catto et al., 2016).
One of your main conclusions (that changes in individual cyclones resulting from your SST changes aren't important for changes in the mean state) is therefore not surprising given the results of these aforementioned studies (that show the mean state in both winds and precipitation is set by atmospheric fronts). I think the authors should discuss these studies, as well as the consistency of their results with them. Furthermore, I think it is fair (and scientifically interesting) to request that the authors make some attempt to use these studies together with their results to make some additional hypothesis regarding the relationship between SST front, cyclone and atmospheric front (e.g. SST frontal influence on mean-state mostly through weak atmospheric fronts/weak cyclones, as discussed in Masunaga et al, 2020b, or some other idea).

We thank the reviewer for this comment, and we included these papers in the Introduction Section (lines: 73-82) of the revised manuscript. In particular the findings of Masunaga et al., 2020a,b are very much in line with our findings as well as the findings in a related submitted study focusing on the interaction of atmospheric fronts with SST gradients (Reeder et al., resubmitted to JAS). We would, however, caution whether what Masunaga et al., 2020a,b refer to as "weak systems" is representative of cyclones. We thus complement the discussion presented in Masunaga et al. 2020a,b with our cyclone-centred viewpoint.
As fronts are specifically discussed in a related submission (Reeder et al., resubmitted to JAS), we decided to not duplicate this analysis and discussion here.
Following the concern from the reviewer, we however complemented our previous analysis with one in which we considered climatologies of the pertinent variables occurring within a radius of 750 km (consistent with a maximum cyclone circumference in Wernli and Schwierz (2006) and the analysis of Rudeva and Gulev (2011)) of any cyclone over its entire lifetime in the ocean basin, irrespective of its propagation and its location of maximum intensification. We adjusted our text and present the new results on the following Sections Introduction (lines: 96-99), Data & Methods (lines: 165-171), in subsections 3.4.1 (lines: 316-319), 3.4.2 (lines: 372-375), 3.4.3 (lines: 386-401), 3.4.4 (lines: 417-423), and in the Conclusions (lines: 456-470). We also changed figures 7-10 (c-d, g-h) and their associated captions, respectively. The results arising from this new analysis are in line with our previous results and support our previous conclusions. The only exception is Figure 10 (g-h), in which we now find the wind speed composite at 300 hPa that is not associated with cyclones to better resemble the climatological differences in the North Pacific, which is in line with the results for the North Atlantic. In addition, we conducted our analysis within different radii (500 and 1000km) and present the results for the surface heat fluxes and the different types of precipitation in Appendix B

(Figures B1-B4). With the larger radius, we also included the contribution of nearly all fronts in the analysis and thereby implicitly addressed the reviewers concerns about the role of atmospheric fronts.

Line 80: Following on from the above, the strong surface fluxes primarily occur behind cold fronts that bring cold dry air off the continent, not necessarily with cyclones as a whole (especially when the cyclones are positioned directly over the GS), and are modulated thus. Also, this paragraph should clearly reference Bishop et al. (2017) which discusses the scale dependence of SSTs and heat fluxes in the western boundary current regions in terms of atmosphere-driven vs. ocean-driven.
Bishop, S. P., Small, R. J., Bryan, F. O., & Tomas, R. A. (2017). Scale dependence of midlatitude air–sea interaction. Journal of Climate, 30(20), 8207-8221

Following our response to the previous comment, we believe that a large enough radius would include a significant part of the cold sector associated with the parent cyclone. However, cold air outbreaks must not necessarily occur in the cold sector of a cyclone, but can also be associated with an anticyclone (e.g., Dalavalle and Bosart 1975; Colucci and Davenport 1987; Nakamura et al., 2016). Hence, climatological features associated with these type of cold air outbreaks should not be accounted for in a cyclone specific climatology.
We thank the reviewer for pointing us to the paper by Bishop et al. (2017). We acknowledge this debate on whether the SST variability in the WBCs is atmospheric or ocean-driven on different timescales. However, with our analysis based on simulations with prescribed SSTs we feel that we cannot contribute to this debate and thus decided to not discuss our results in the context of Bishop et al. (2017) in the revised version of our manuscript.

(M): I have a few serious concerns about the data used for this study. Firstly, it has been shown many times how important model resolution is for accurately resolving air-sea interaction in western boundary current regions. Some examples: Smirnov et al. (2015) show the necessity for both ocean and atmosphere at 0.25deg in order for SST-induced heating to be balanced by transients rather than cold-air advection. At 50km, you are not there, and barely resolving the cross-frontal scale in the atmosphere (~100km). It is also well known that you need eddy-resolving (~1/10deg) resolution in the ocean to actually resolve many of the oceanic eddy processes crucial to the GS/KE air-sea interaction (e.g. Figure 2 from Hewitt et al., 2017). These eddies are known to be crucial for the interaction (Ma et al., 2015) and its influence on general atmospheric and oceanic variability. Your resolution falls under these values and I don't see how definitive conclusions can be made given we already know the interaction cannot be fully resolved. The importance of resolution and resolving transients for the hemispheric flow is also shown in Lee et al. (2018). I also can't understand why ERA-Interim is used instead of ERA-5 which is available, which has a much better resolution atmosphere and ocean. You even note this yourself in Line 157. Please also mention in Section 2.1 what temporal resolution you use. How often do you calculate SST fronts etc?

Regarding the first point of the reviewer on the model resolution, we would like to make three remarks. First, the AFES simulations on which we based our analysis have been used for numerous studies on the effect of SST gradients and fronts on the atmosphere (e.g., Nakamura et al., 2008; Kuwano-Yoshida et al., 2010; Kuwano Yoshida and Minobe 2017). We thus regard the AFES data as a valid dataset in the context of previous studies on this topic.
Second, we appreciate and are aware that smaller-scale ocean features can affect the climatological air-sea exchange to some extent. However, as we only focus on 1st-order effects on synoptic-scale systems, it seems implausible that ocean eddies will fundamentally change these attributions and thus our conclusions.
Finally, our analysis based on this coarser model data is highly relevant to the climate modelling community.

Regarding ERA-Interim, we included this comparison to the AFES climatology only to make plausible that the AFES simulations, which are comparable in resolution to ERA-Interim, provide a reasonable depiction of the air-sea exchange and storm track over our target region. Moreover, we have tested the sensitivity of our results to either just using 1979-2001 or 2002-2016, with no significant changes between the two periods despite the resolution change in SST for ERA-Interim (e.g., Masunaga et al. 2015). Overall, ERA-Interim has been extensively and fruitfully used in numerous climatological analyses of air-sea interactions. We thus argue that it remains a relevant data set.

Finally, regarding the SST fronts, we perform the detection using SST data filtered with a spectral truncation to T84 resolution and detect the SST fronts in the instantaneous SST field every 6-hours. We added the missing information in the revised version of the manuscript (please refer to subsection 2.2, lines: 125-126).

Line 110 - What is the sensitivity of your results to choice of threshold on the SST gradient? Also, how can you confidently make comparisons between the Atlantic and the Pacific when you have a different threshold for both?

We thoroughly tested different thresholds to obtain reasonable SST fronts in both basins. Given the different SST gradient in the two basins (e.g., Nakamura et al., 2004; Tsopouridis et al., 2020b), using the Atlantic threshold, we would capture almost no SST fronts in the Kuroshio region. Vice versa, with the Pacific threshold in the Gulf Stream region, we would have obtained many weak SST fronts that would make it difficult to identify the main SST front. We thus believe that a different threshold is necessary to accommodate the different natures of the boundary currents and SST fronts. We adapted the text accordingly (subsection 2.2 (lines: 127-130)), to clarify the use of different thresholds in the two ocean basins.

Line 120 - What about the sensitivity to cyclone detection? Neu et al. clear show vastly different climatologies depending on the specific method. Neu, U., Akperov, M. G., Bellenbaum, N., Benestad, R., Blender, R., Caballero, R., ... & Grieger, J. (2013). IMILAST: A community effort to intercompare extratropical cyclone detection and tracking algorithms. Bulletin of the American Meteorological Society, 94(4), 529-547

We thank the reviewer for raising this issue and letting us discuss it in more detail this issue. We conducted several sensitivity experiments where we varied parameters in the detection and tracking namelists used by the algorithm. As the reviewer mentioned, there are qualitative differences between previous studies, but unfortunately no sufficient information is provided regarding the parameters used in each study. Motivated by this gap, we decided to publish our detection and tracking namelist in Tsopouridis et al., 2020a. The cyclone density pattern is in good spatial agreement with previous studies (e.g., Hanley and Caballero, 2012; Neu et al., 2013), successfully capturing the major regions of cyclone activity in both basins. We observe small quantitative differences compared to the density climatology presented by both Neu et al. (2013) or Murray and Simmonds (1991a), who also used the Melbourne University algorithm. These small deviations are most likely due to the neglect of shallow and weak systems in our database. We thus included the relevant discussion in the subsection 2.3 (lines: 145-149).

Line 123-125 - This seems like a strange and unnecessary choice to me, given the importance of quasi-stationary systems in the region (Masunaga et al., 2020)

We only consider actively developing cyclones in the region and further investigate their evolution and the associated mechanisms. Moreover, the steep orography around Greenland's coast would result in artefacts in the detection and therefore we do not include quasi-stationary systems.

(M): Section 2.4. I see this as a serious concern. Firstly, I have a problem with using the SST front definition from the CNTL experiment to define the location in the SMTH. The fact that there are no "fronts" in the SMTH points to a shortcoming in the methodology being used to identify them. Also, the definition of SST front in the CNTL experiment is also concerning, as it is simply based on an SST gradient. The SST front is not a straight line (I suggest you look at a figure like Figure 1 from Andres (2016)) how do you deal with Gulf Stream rings for example? This also makes me rather skeptical of the definitions of C1, C2 and C3, despite their use in previous manuscripts. Additionally, if there is no defined SST front in the SMTH experiment, then there is minimal relevance to the definitions of C1, C2, and C3 anyway. Andres, M. (2016). On the recent destabilization of the Gulf Stream path downstream of Cape Hatteras. Geophysical Research Letters, 43(18), 9836-9842.

We thank the reviewer for this comment and for letting us clarify this issue. We did attempt to objectively identify the SST fronts for the SMTH experiments, but this fails because the SST gradient is very weak and homogeneous over a large region such that the detection of the SST front location is mostly determined by numerical noise rather than a defined maximum in the SST gradient. We therefore used the classification based on the CNTL experiments only to be able to compare cyclones within a geographically similar genesis location and track across the experiments. To point this out more clearly, we adjusted the text accordingly (please refer to section 2.4 (lines: 158-163)).

Regarding the second point of the reviewer, our detection scheme is indeed able to detect all lines, including rings, and not only straight lines as the reviewer seemed to suspect.

Figure 3: It is not clear what I am looking at here-also, how do you arrive at the units? Also, please provide further information on the jet detection method

We thank the reviewer for letting us provide more information on this. To assess the potential impact of the SST smoothing on the upper levels, we detect the position of jet following the algorithm of Spensberger et al. (2017). The algorithm detects jets by their axis, the line of maximum winds that separates cyclonic from anticyclonic wind shear. The algorithm requires the wind maximum to be well-defined, but does not impose a strict minimum wind speed (Spensberger et al., 2017). We added this information in subsection 2.2 (lines: 131-144).
For the climatologies (Figure 3) and composites we normalise the occurrence of both SST front lines and jet axis lines to account for the varying area covered by grid cells. We achieve this by showing the average length of SST front line/jet axis line per unit area, hence the resulting unit of length per area. For details on the normalisation, we refer to Spensberger and Spengler (2020). To compile a climatology, we use for both the SST front and the jet axis distributions a unit of length per area, due to its independence of the grid resolution. We added this information in subsection 2.2 (lines: 135-138).

Figure 4: I find the wording of this caption extremely confusing.

Following the reviewer's concern, we rephrased the caption of Figure 4.

Line 185- In my opinion, this hypothesis should be removed as it implicitly assumes an SST influence on the jet, which is meant to be a topic of exploration here. Similar comments apply to Line 188 and Line 288.

We thank the reviewer for this comment. Considering the reviewers opinion, we removed the hypothesis in line 185.

However, we kept the hypotheses for lines 188 and 288, as previous studies confirmed both the significance of a strong SST gradient for anchoring the storm track (e.g., Nakamura et al., 2004,2008; Sampe et al., 2010), as well as the relationship between moisture and temperature (here between humidity and SST) through the Clausius-Clapeyron relation.

(M): Line 200 - I cannot understand the logic behind not just looking at all cyclones.
Line 223 - you make the bold claim that cyclone intensification is only weakly modified by SST gradient, but what about all those that aren't undergoing maximum intensification right there?

The Gulf Stream and Kuroshio regions are the areas where the major SST fronts are located and where the maximum SST differences due to smoothing occur. We wanted to relate the intensification of cyclones with the changes in absolute SSTs and/or SST gradient and therefore did not include cyclones with a maximum intensification further away towards the central and eastern parts of the two basins, as the intensification of these cyclones could not be directly associated with these changes in SST.

(M): Line 258 - This is a main point of the paper and one I don't feel comfortable with. Your claim is that the SST gradient is not particularly important for the intensification of individual cyclones. Whilst I may agree with this general sentiment (see my first major comment), I do not think the analysis presented here can make that conclusion.
1) As mentioned previously, you are looking at a subset only.

We thank the reviewer for sharing these thoughts with us. First of all, please let us clarify that we only used a subset of cyclones (C1,2,3) to investigate the possible changes on cyclone intensification for cyclones belonging to the different categories, for the reasons mentioned in the previous response. For the remainder and main part of the manuscript, however, we use all cyclones. For our previous climatological analysis, we used all cyclones in the respective regions (irrespective of track and location of maximum intensification). For the revised manuscript, we actually use all cyclones in our database. We pointed out this difference between the parts of the paper more clearly (e.g. lines: 95-99, 166-171, 439-440).

2) There are many differences between your two experiments, not just the SST gradient. The absolute SST changes also, and I strongly suspect that the variability in the Gulf Stream path length/separation from coast changes too, each having been shown to significantly affect the interaction. I don't see anything up to this point that allows you to definitively link it to the SST gradient. You mention in Line 40 that these factors need to be teased apart, but I am not convinced that has been done here simply by defining C1, C2 and C3 - these classifications are also ill-defined in the SMTH experiment.

We fully agree with the reviewer regarding the change of both the absolute SSTs and the SST gradient after the SST smoothing, as well as to their proven importance on cyclone intensification. Thus, this issue is frequently discussed in our paper (e.g., lines: 24-41, 186-191, 267-274, 357-362, 381-383, 435-438, 464-466) as well as in our ERA-Interim-based analyses for the same regions (Tsopouridis et al. 2020a,b). One of our conclusions is that both the absolute SST and the SST gradient play a role for the evolution of cyclone characteristics. For instance, the increase (decrease) of the absolute SST results in an increase (decrease) of the upward surface heat fluxes and specific humidity, whereas a weak SST gradient in the SMTHG experiment is found to be associated with a considerable reduction in precipitation (in line with Kuwano-Yoshida et al., 2010b), compared to the pronounced SST gradient in the CNTL experiment. Moreover, we document the changes in SST prior and after the

SST smoothing (Figure 6) and how these changes can affect the intensification of the different cyclone categories, that is C1,2,3 (Figure 5), as discussed in subsection 3.3. Nevertheless, we acknowledge the reviewer's concern (based on Line 40) that the two factors have not been separated as thoroughly as in Tsopouridis et al. (2020a,b), and thus rephrased our sentence in the Introduction (please refer to Section Introduction, lines: 40-41).
Regarding the "variability in the Gulf Stream path length/separation from coast", the heavy smoothing in the SMTHG experiment implies that there is actually not even a "Gulf Stream" anymore, not only that the length has changed.

Line 266- as I alluded to earlier, it does not seem appropriate to use two different subsets of cyclones for each of these sections (individual vs. mean state) if you are going to draw comparisons between them. Additionally, given that the typical timescale for thermal air-sea interaction is ~ a day or so, one would expect the GS/KE to impact some storms further downstream from the region. This again raises questions regarding the conclusion that the SST front does not affect individual cyclones, just because the ones reaching maximum intensity in that specific regions are not that affected.

Regarding the subsets we refer to our response to L258/point 1.

Regarding the debate on local versus remote effects, we appreciate that our previous climatological analysis was not ideal to comment on this issue, but the revised analysis allows us to comment on this issue. Counting all air-sea exchange within different radii of a cyclone centre as "local", we now show that "non-local" air-sea exchange dominates the climatology. This finding is in line with previous studies that tried to clarify the influence of surface heat fluxes on midlatitude cyclone development and untie their local and remote effects (e.g., Nuss and Anthes 1987; Kuo and Reed 1988; Haualand and Spengler 2020; Bui and Spengler 2021). We thus believe that SSTs and SST fronts are mainly important for climatologically setting the environment in which cyclones evolve, though each individual cyclone is not significantly affected directly by changes in the SST.

(M): Section 3.4. Line 294 -Parfitt and Czaja (2016) have shown that the top 30% of climatological latent heat fluxes in the Gulf Stream region are associated with cyclones that have just passed over the region. It is continuously said here "when no cyclones are present in the respective region", however that simply means the cyclone center is not directly above the region - this does not mean a cyclone's passing isn't responsible for the trailing cold air that maximises the fluxes. This is relevant for consistency with Section 3.4.2 later, where it is shown cyclone precipitation does change significantly. A lot of moisture availability, that is ultimately taken up in say the warm conveyor belt of any given cyclone, in these regions can come from the strong latent heat fluxes
behind the cold front of a system that passed ahead of it. This comment is also relevant for your statement "for specific humidity, cyclones account only for a small part of the climatological differences" in Line 359, and for your statement from Line 379-383. In particular, I do not agree with Line 381-383. This comment feeds back to the previous one, where I do not think conclusions about the SST influence on the atmosphere through cyclones can be drawn simply by comparing whether a cyclone is in that specific region or not.

We acknowledge the reviewer's concerns and agree that the attribution to only cyclone centres in the box might have been too restrictive. Please refer to our response in the first comment on the additional analysis we now present in the revised version of the manuscript where we include all cyclones in the respective ocean basin.

Figure 8: The colors on Figure 8 (b-h) I would recommend changing as it is hard to see any difference.

We adjusted the colours, so that the differences are more evident in the revised version of the manuscript.

Section 3.4.4 - I would recommend mentioning Lee et al. (2018) here also.

We thank the reviewer for his suggestion and making us aware of this study. Nevertheless, we decided instead to mention the study of Lee and Kim 2003, as it better reflects our findings and discussion in subsection 3.4.4.

Lastly, I noticed a number of spelling/grammatical errors, I suggest a thorough check.

We thank the reviewer for making us aware of these errors/typos, which we adjusted accordingly in the revised version of the manuscript.

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