Response to Reviewer 1

We sincerely thank reviewer 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 agree with the reviewer that the attribution to only cyclones in the box might have been too restrictive and we will complement our previous analysis with one in which we will consider the pertinent variables occurring within different radii of any cyclone over their entire lifetime in the ocean basin irrespective of its location. A preliminary analysis indicates that this additional analysis will support our previous conclusions, but we will adapt the manuscript accordingly should they not.

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 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 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, showing that our rationale for this choice has not become apparent in the current state 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 will adapt the manuscript to point this out more clearly. 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 conducted at and 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/MINOR COMMENTS

We thank the reviewer for all the "other/minor comments" she raised and we will consider them when editing the manuscript. Please let me answer/clarify some of these questions.

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 the storm track areas, the title may need to be changed.

In this version of our manuscript, we strictly associate our results to the Gulf Stream and Kuroshio regions, as these are defined in the manuscript. As the reviewer underlined, for these regions "it is indeed shown that the atmospheric conditions change stronger in the absence of cyclones over the ocean currents". Based on a preliminary analysis, we believe that our additional analysis (see our response to Major comment 1) will confirm our previous findings and will allow us to extend this argument to the wider area of the North Atlantic and North Pacific, respectively. Of course, if needed we will edit/change the title to correspond to our new findings.

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).

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. 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 will add this missing information in the manuscript.

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?

Exactly. Please see our response on Major comment 1 to see how we will proceed with the new analysis.

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

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 will point this out more clearly in the Methods section.

We acknowledge that the unit of length per area is somewhat unintuitive. We nevertheless use this unit when compiling a climatology, because this is independent of the grid resolution, while for example a naive masking of all grid points that contain a front line would yield lower detection frequencies the finer the analysis grid becomes.

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), using the Atlantic threshold, almost no SST fronts would have been captured in the Kuroshio region. Vice versus, with the Pacific threshold in the Gulf Stream region we would have obtained many weak SST fronts that would have made 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 will adapt the text accordingly, to clarify the use of different thresholds in the two ocean basins.

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 will provide the statistically significant results, as the reviewer suggested.