# 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 will include these papers in the introduction and discussion sections 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 will thus complement the discussion presented in Masunaga et al. 2020a,b with our cyclone-specific viewpoint.
As fronts are specifically discussed in a related submission, we will not duplicate this analysis and discussion here. Following concerns from both reviewers, we will however complement our previous analysis with one in which we will consider climatologies of the pertinent variables within different radii of all cyclones in our database, irrespective of the location of the centre and their state in the life cycle. We vary radii between 500 and 1000 km, consistent with a maximum cyclone circumference in Wernli and Schwierz (2006) and the analysis of Rudeva and Gulev (2011). With the larger radii, we will thus also implicitly include the contribution of nearly all fronts in the analysis. Preliminary analysis supports our previous conclusions.

**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 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 will not be able to contribute to this debate and would thus find it misleading to discuss our results in the context of Bishop et al. (2017) in 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 this as valid data in the context of previous studies on this topic.
Second, we appreciate and are aware of that smaller-scale ocean features can affect the climatological air-sea exchange to some extent. We here, however, aim to contribute to a discussion of a much more fundamental question, namely which processes are typically associated with the atmosphere air-sea exchange. It seems implausible that ocean eddies will fundamentally change these attributions and thus our conclusions.
Finally, our analysis based on this type of coarser model data is highly relevant to the climate modelling community, which typically uses even coarser resolutions. We will adapt the discussion and conclusions to make this focus of our analysis clearer.

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. We have used ERA-Interim at 0.5° resolution, interpolated from the original 0.7°. In our department, we would only have access to ERA5 data at 0.5° resolution data due to constraints on data storage. 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. Overall, ERA-Interim has been extensively and fruitfully used in numerous climatological analyses of air-sea interactions, so 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 will add the missing information in the revised version of the manuscript.

**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, 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.

**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 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.

**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 use 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. We will adapt the manuscript to point this out more clearly.
Regarding the second point of the reviewer, let us underline that our detection scheme is indeed able detect all lines, including rings, and not only straight lines as the reviewer seems to suspect.

**(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 the maximum SST differences after the smoothing are observed. 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/eastern parts of the two basins, as the intensification of these cyclones could not be directly associated with these changes, as likely other reasons/mechanisms can evolve.

**(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 in the different categories, for the reasons mentioned in the previous response. For the remainder and main part of the manuscript we used all cyclones. For the climatological analysis we previously used all cyclones in the respective regions (irrespective of track and location of maximum intensification) and in the revised manuscripts we use all cyclones in our database. We will make sure to point out this difference between the parts of the paper more clearly.

**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.**

On this point we fully agree with the reviewer, and brought up this issue frequently in the discussion of this paper (L25, 37-41, 160-170, 185-187, 244, 250-251, 285-286, 331-334, 350-358, 411-414, 435-437) and our ERA-Interim-based analyses for the same regions (Tsopouridis et al. 2020a,b). Our main conclusion here is that both the absolute SSTs and the SST gradient play roles for the evolution of cyclone characteristics. For instance, the increase (decrease) of the absolute SST values 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.

**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 will allow us to comment on this issue. Counting all air-sea exchange within different radii of a cyclone centre as "local", we will 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). We thus believe that SSTs and SST fronts are mainly important for climatologically setting the environment in which cyclones evolve, even though each individual cyclone is not significantly affected directly by changes in the SST.

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.

**(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 will include in the revised version of the manuscript.