Response to Referee 1 (Irina Rudeva)

My major concern is that the paper does not offer explanation as to why SST fronts seem to have little influence on cyclone characteristics. I would like to see if not a detailed analysis on this, but at least discussion.

We understand the reviewers wish and added several discussion items as further outlined below.

Despite the absence of an SST front in the Atlantic, the SST gradient over the black box remains strong compared to the rest of the ocean. Thus, even in the absence of a sharp SST front, this gradient may remain efficient in creating baroclinic instability. Jacobs et al. (2005) offered a very simple prestorm baroclinic index PSBI = (TG - Tland)/d, where d is the distance to the Gulf Stream from the coastline. The results shown in the paper make me think that land-sea contrasts might be significantly larger than the SST difference between CNTRL and SMTH experiments. I suggest calculating a similar index for cyclones in C1-C3 categories to check this (along which line this gradient should be calculated is an open question, e.g., along the same latitude as the cyclone centre at max intensity).

We thank the reviewer for raising this issue. Tsopouridis et al., 2020a,b (https://doi.org/10.1002/qj.3924 and https://doi.org/10.1002/qj.3929) related the intensification of individual cyclones to the land-sea contrast (in the Gulf Stream region) and to the upper-level forcing (particularly in the Kuroshio region), while they found the SST front to only play a secondary role for the intensification of cyclones in the two regions. We included this discussion in the revised version of the manuscript (lines: 256-258, 300-301). Moreover, to estimate the relative role of the land-sea contrast and the SST front, we conducted a composite analysis for cyclones in the three categories (C1-C3) for the North Atlantic, where both the land-sea contrast and the SST gradient are more pronounced, but only the latter was considerably changed in the SMTH experiment. We included this additional analysis in the supplement (Figures S11, S12) and discuss the results in lines: 258-266. Our results indicate that despite the significantly reduced SST gradient in the SMTHG experiment, the low-level baroclinicity (based on the temperature gradient at 850 hPa) remains largely unchanged, indicating the dominant role of the land-sea contrast to enhance low-level baroclinicity and hence cyclone intensification compared to the SST front, which is fully in line with the reviewer's thoughts.

The other important question is why SST fronts play a larger role in the absence of cyclones. While land-sea contrasts, discussed above, can be particularly important in case of already formed cyclones, sharp SST fronts may play a large role during less perturbed flows, creating thermal wind and affecting the circulation downstream.

We are not sure what the reviewer refers to as "less perturbed flows". If one would argue that the flow is less perturbed in the absence of cyclones, the argument of the reviewer actually aligns with our results and interpretation, i.e., that the non-cyclone time steps and thus less perturbed situations play a more crucial role in terms of how the SST front imprints itself on the mean state.

Title: climatology of what? I'd replace 'climatology' with 'atmospheric flow'. The title still reads like a media headline rather than a name of a scientific paper, but I feel that this is the impression the authors wanted to make.

We thank the author for this comment. With this title, we wanted to highlight the main finding of our study and did not intend to make a "media headline". Explicitly stating "atmospheric flow" instead of climatology would not be correct, as we also address variables, such as precipitation and surface fluxes, that do not describe the atmospheric flow.

Am I right that cyclones selected for the analysis spent 3 6-hr steps and reached their max intensity within the black boxes? All other cyclones fell into category 'absence of cyclones'? l 537-8, 539: This is only true for cyclones that reach their max intensity over the Gulf Stream/Kuroshio, if I got it right. Cyclones downstream of the currents may still be modified by changing SST gradients (and baroclinicity) and play a role in forming those anomalies.

As indicated in the first revised version of the manuscript, we consider cyclone intensification not intensity (e.g., lines: 160, 228-229, 243-246, 247-255, 258-266, 2659-272, 275, 287, 300-301, 452-455, 491-493). Further, for the decomposition of cyclones, we outline in the manuscript that we consider all cyclones propagating in the North Atlantic and the North Pacific "irrespective of the direction of cyclone propagation and location of maximum intensification" (see lines: 165-171, 439-440 in the previous version of the manuscript). The only limitation is that we consider cyclone tracks with at least five 6-hour time steps, following a commonly used technique (Neu et al. 2013). We thus feel that the data selection is sufficiently clear in the current version of the manuscript.

Why max wind speed is significantly displaced equatorward in the absence of cyclones but not when they are present? Is it because the jet is already displaced southward in both CNTL and SMTH simulations when a cyclone is present?

In the Atlantic, the displacement is present for both when cyclones are present and absent. The different intensity of the displacement pattern mainly reflects the fact that the region features more time steps with no cyclone present. See also the respective climatology in the supplement (Fig. S13). For the Pacific, the story is similar, though with some differences between the cyclone/no-cyclone patterns in the westernmost part of the Pacific, which we do not have a good hypothesis for. We included a brief discussion about this in the manuscript, though cannot provide a conclusive answer explaining these discrepancies (see lines 434-440).

l. 245; Could slowing of the jet over the Gulf of Alaska along with increasing cyclone density be related to higher cyclolyse in that region?

We agree with the reviewer that a slowing of the jet over the Gulf of Alaska along with increasing cyclone density can indeed be related to higher cyclolysis in that region. However, a further analysis of this geographic region would be beyond the scope of this paper and we thus decided to not include this discussion in the revised version of the manuscript.

Minor comments:

l.536: decrease in SST contrast

We thank the reviewer for pointing this out. We filled the missing word in the revised version.

l. 537: Did you mean SST gradient, not just SST?

Yes, we meant SST gradient and adjusted the text accordingly.

Fig. 4: I would suggest to show statistically significant differences. It is also interesting to see the trajectories of cyclones in supplements.

Following the reviewer's request, we revised Figure 4 to show only statistically significant differences. We also present the trajectories of all cyclones for the two regions in the supplement for all the experiments as well as the trajectories of cyclones belonging in the three sub-categories (C1,2,3), as suggested by the reviewer (Figure S9,S10). Finally, following the editor's suggestion we included the frequencies for the "within/outside" cyclone area analysis (Figure S13).

Fig.6: Is the SST averaged over 750km radius?

No, the SSTs are averaged over a 400km radius. Sensitivity tests using different radii (e.g., 200km) were conducted and yielded similar results. The 750km radius was used only for the second part of our analysis (“presence/absence of cyclones”) following the reviewers’ comments during the first round of reviews (see lines 174-180, 333-336, 476-478).

Fig.7-10: It is said that differences are statistically significant at 95% level. Is it true for all grid points even with differences close to zero? There are white areas in panels b and f, but not in c and g.

We acknowledge that the caption was confusing, giving the impression that a statistical test was also conducted for the “within/outside” analysis (panels c,d and g.h) for figures 7-10, which was not the case. However, instead of editing the caption, we decided to perform statistical tests for panels c,d & g,h and present the respective results in figures 7-10.

Response to anonymous referee 2

- Please describe these papers correctly. The Parfitt and Seo (2018) paper indicates the importance of atmospheric fronts and the baroclinic waveguide (i.e. the combination of both cyclones and anti-cyclones), not extratropical cyclones specifically. This distinction is important. Also, many frontal detection algorithms detect atmospheric fronts in the Gulf Stream / Kuroshio regions without an "associated" extra tropical cyclone, especially with weaker fronts, as in Masunaga et al. (2020a, b). Simply including a larger cyclone radius does not address the issue of fronts discussed in previous work.

We thank the reviewer for this comment and agree that the citation should have been more on point and we thus rephrased this sentence (see lines: 80-81). Further, while we agree with Parfitt and Seo (2018) about the role of the baroclinic waveguide for the propagation of Rossby waves and thus cyclones and anticyclones, such a wave guide is usually constituted by neither cyclones nor anticyclones, but by the jet (c.f. review in Martius et al. 2010, https://doi.org/10.1175/2009JAS2995.1). As we discuss the impact on the position and intensity of the jet, we thereby also address the waveguide (e.g., see lines: 43-49, 138-141, 208-219, 273-275, 425-428, 441-443), similar to TSS20a,b.

Regarding the role of fronts, we would like to refer the reviewer to a recent publication (Reeder et al. 2021, https://doi.org/10.1175/JAS-D-20-0118.1) which complements the analysis in the present manuscript by focussing on fronts rather than cyclones. Given that Reeder et al. (2021) document that the SST front imprints itself mainly in the absence of atmospheric fronts, it is a timely and pertinent question to ask whether the same might be true for cyclones. We now explicitly refer to this study and put our work in context (see lines: 84-86).

Regarding the association of variables to cyclones by defining a cut-off radius, we follow a standard analysis method (c.f. Rudeva and Gulev 2011; and Catto and Pfahl 2013, https://doi.org/10.1002/jgrd.50852). Catto and Pfahl (2013) associate precipitation with fronts by the same technique. With a maximum radius of 1000 km we make sure to include all fronts associated with the cyclone into our analysis, such that our analysis appears well-suited to address the questions we set out to answer.

The Bishop et al. (2017) paper is relevant as it shows a clear shift to ocean-driven variability. Numerous other papers show you need eddy-resolving SSTs to fully understand this variability and its influence on the atmosphere. e.g. Liu et al. (2021) - regardless of prescribed SSTs or not. Liu, X., Ma, X., Chang, P., Jia, Y., Fu, D., Xu, G., ... & Patricola, C. M. (2021). Ocean fronts and eddies force atmospheric rivers and heavy precipitation in western North America. Nature communications, 12(1), 1-10.

-The above comment leads to the authors response regarding the lack of ocean eddy-resolving resolution in their simulations. I have two issues with their response. 1) It is not a valid argument to say that because the simulations and datasets have been used for studies previously, that they are suitable to use now. One would no longer solely use the CMIP2 models or ERA-40 to model climate variability, despite them being used extensively in the past. As mentioned above, many papers show lack of eddy-resolution in the ocean results in incorrect atmospheric responses. 2) In my opinion, papers like the one mentioned above clearly demonstrate the authors assertion that "it seems implausible that ocean eddies will fundamentally change... 1st order effect on synoptic-scale systems" is incorrect.

We acknowledge the reviewers point about the potential impact of mesoscale ocean eddies on air-sea heat fluxes and are aware that this has been documented in previous studies, including the ones the reviewer mentions. While some recent studies focus on the impact of mesoscale eddies, there is a large body of literature on how larger scale SST gradients along the two of the major western boundary currents impact the atmosphere. We aim to investigate this impact and ask how and if the SST gradient *directly affects* the evolution of individual cyclones. Or, alternatively, if the SST gradient predominantly affects the environment in which the synoptic systems occur, and thus *indirectly affects* individual cyclones. As argued in our manuscript, our findings indicate that the *indirect impact* on cyclone development is dominating the *direct impact*, because, as shown in our results, the SST gradient imprints itself on the atmosphere mainly in the absence of cyclones. Given the insensitivity of individual cyclones to such drastic changes in the SST gradient as considered here, we find it very plausible that also the *direct impact* of mesoscale eddies in the ocean is small. Nevertheless, similar to the large-scale SST gradient, we believe that ocean eddies can play a role in shaping the atmospheric mean state, potentially also mainly through interactions in the absence of cyclones. However, this investigation is beyond the scope of our study. We provided more context for our studies vis-à-vis the role of ocean eddies in our revised introduction (see lines: 60-62).

We hence believe that the usage of AFES data is justified, as we only focus on 1st-order effects on synoptic scale systems and on the potential direct effect of the SST fronts on the evolution of cyclones. The same dataset has been extensively used in the past in numerous respective analyses, as explicitly described in the manuscript (e.g., lines: 109,115,308). We acknowledge the reviewer's concern on the coarser resolution of the AFES model, but after comparing the AFES climatology with the ERA-Interim dataset we found the air-sea heat exchange, as well as the storm track to be reasonably represented. Apart from this, we previously tested the results for the two time periods of the ERA-Interim reanalysis (1979-2001 and 2002-2016) to test the possible impact of the resolution change in SST, with overall no significant change. We believe that just using a more recent dataset would not constitute a more novel approach to the problem that we are tackling in our manuscript.

We argue consistently within our findings and our arguments are backed up by the analysis that we present. Thus, we disagree with the reviewer that we make incorrect statements. Our statements are based on our analysis, which we believe is sound. If the reviewer disagrees with our analysis, the reviewer should clearly outline flaws in our methodology. We regard stating an opinion that our claims are incorrect, despite them being firmly based on our analysis and presented results, insufficient as a criticism. Time will show if future analyses and methodology will support our findings or refute them.

- I still do not believe that you can draw strong comparisons between the Gulf Stream and Kuroshio if the definitions used in each basin are different. I understand the magnitude of the SST gradients in the two basins are different, but I cannot accept the argument that this means it is impossible to come up with an SST front metric that fits both. This is also related to a comment I have on the SST gradient definition between CNTL and SMTHG/K (see below).

In general, it would be best to use a metric that "fits both" basins, rather than conducting sensitivity tests for each region to identify suitable thresholds. However, the Gulf Stream and Kuroshio regions feature certain differences, such as a distinct upper-level wave field, a different structure and intensity of the upper-level jet, and a significantly greater SST gradient in the Atlantic compared to the Pacific (e.g., Nakamura et al., 2004; Tsopouridis et al., 2020b). Thus, choosing the same threshold to detect the SST front in the two regions, would indicate that we set aside one of the most important differences between the two basins, which we argue would lead to questionable results in both regions (underestimation/overestimation of SST fronts, respectively). We thus strongly believe that a different threshold is necessary to accommodate for the different natures of the boundary currents and SST fronts together with the overall different characteristics in the two regions. These arguments are also clearly presented in our manuscript (Lines: 131-137).

- Related to my comment earlier, I still have a serious concern with using the SST front definition in the CNTL experiment to define the location in the SMTHG/K. The fact that no SST front is identified in the SMTHG/K with the current SST front definition simply tells me the definition needs to be altered. I suggest the authors review the numerous papers that have defined the Gulf Stream / Kuroshio location or the associated SST gradient in data as coarse as that used here. I have to say that this issue makes it extremely difficult for me to be confident in any of the CNTL vs. SMTHG/K comparisons. Especially as this makes the definitions for C1, C2, and C3 have little meaning.

We thank the reviewer for this comment. "Reviewing the numerous papers that have defined the Gulf Stream/ Kuroshio location or the associated SST gradient in data as coarse as that used here" was our first priority and we indeed referred to the respective studies several times, particularly to the ones from Kuwano-Yoshida and Minobe (2017) and Kuwano-Yoshida et al. (2010), in which the same data were used (e.g., in lines: 21-22, 29-47, 52-54, 64-72, 111-117, 285-294, 335-337, 343-344 in the previous version of the manuscript). The fact that no SST front is identifiable in SMTHG/SMTHK is not a result of "the current SST front definition", but arises from the extent to which the SSTs are smoothed. In a region where the SST gradient is constant over several hundred kilometres, the concept of SST front simply does not longer apply, irrespective of the method chosen to identify fronts. We would understand the reviewer's concerns if a considerably weaker SST gradient would have resulted in a significantly reduced number of cyclones or generally different climatology of cyclones in the region. However, as presented in Figures S1,S2 in the revised version of the manuscript, an almost equal number of cyclones of C1,2,3 propagate roughly in the same region in the experiments with the smoothed SST. While the approach for our C1,2,3 analysis comparing CNTL and the smooth experiments might not be fully optimal, we argue that it is a valuable compromise for the comparison that we present.

- The authors did not make an attempt to actually show how sensitive their results (ocean influence on atmosphere) in this paper are to cyclone detection. I would be surprised if the results were not sensitive to this, especially if the differences in "shallow and weak systems" are noticeable between algorithms as the authors mention.

We thank the reviewer for letting us share our thoughts on this. The different algorithms used for cyclone detection provide different results, first and foremost due to the different atmospheric fields used for defining a cyclone, with mean sea level pressure (MSLP) or lower tropospheric vorticity being the basic tracking metrics (e.g., Sinclair 1994; Hodges et al. 2003; Rudeva and Gulev 2007; Ulbrich et al. 2009). As stated in Neu et al., 2013 "there is no accepted universal definition of what a cyclone is or where its exact position is". However, for the analysis in Tsopouridis et al., 2020a, we thoroughly tested the sensitivity of the results and found that even when using the same metric to define a cyclone, the results are sensitive to the choice of the several parameters, which is also evident from Figure 1 in Neu et al., 2013. We added this information in the manuscript (lines: 150-151). Unfortunately, the great majority of the studies do not provide a detailed namelist of the chosen parameters. In Tsopouridis et al. (2020a), we publish the values of the parameters for the detection and tracking namelists. Overall, we want to highlight, what is underlined in Neu et al., 2013 (to our knowledge the most complete study on cyclone detection and tracking algorithms) and with which we fully agree: "since there is no universal agreement upon cyclone definition, we cannot "judge" the algorithms or say that a specific one delivers "incorrect" results. They are all "right" in some sense."

- Regarding the logic behind only looking at cyclones undergoing maximum intensification right there, I understand the authors point that the maximum intensification of cyclones away from the SST front could not be directly associated with changes in SST. But that doesn't at all mean that they can't be. And in fact, it is fairly well understood that many ocean induced impacts are related to maximum intensification elsewhere (in which case you are not considering the whole set of scenarios). For example, changing the absolute SST will alter the magnitude and location of the evaporation input into the atmosphere. One can expect scenarios where the maximum impact of anomalous latent heating within the cyclone will occur downstream once moisture has risen within the system. This is also relevant for the authors response to a later comment (“ we thus believe that SSTs … are mainly important for climatologically setting the environment in which cyclones evolve, though each individual cyclone is not significantly affected directly “). What about an SST change that moistens the atmosphere prior to the cyclone arrival, but then the cyclone intensifies maximally downstream?

Our results do not contradict the arguments of the reviewer, in fact, we feel that the reviewer paraphrases our findings. Our argument is that the direct impact of the SST front on cyclone development is rather negligible, while the SST changes certainly have an imprint on the climatological setting that will influence cyclone development. In particular, we actually present that there are significant changes in evaporation when SSTs are smoothed. However, we also show that the direct impact on cyclones appears to be small, whereas the indirect effect can contribute to alterations in cyclone development, which is consistent with other recent findings (Bui and Spengler, 2021; Haualand and Spengler, 2020) (see lines: 89, 302-303). We further clarified this line of argument in the manuscript (see lines 491-494).

- For many of the reasons stated above, I still do not think the results in this manuscript can be used to claim that ’ SST fronts only have a minor impact on the characteristics and intensification of individual cyclones’. I would like to also point out that just because differences between CNTL and SMTHG/K are greater in some variables outside a cyclone radius, does not mean that SST fronts have a minor impact on individual cyclones.

We would like to refer to our response to the previous comment. We agree that changes in the SST can have an impact on cyclones, though argue that this influence is indirect, where the changed SST leads to a different baroclinicity and moisture availability that will then influence cyclone development. These arguments are in line with other recent findings clarifying the direct and indirect effects of surface fluxes on cyclone development (Haualand and Spengler, 2020; Bui and Spengler, 2021, lines: 89, 302-303, 491-494).