Response to Referee #1 (Irina Rudeva)

We thank Irina Rudeva for another round of constructive feedback on our manuscript and are pleased to read that we were able to address most of her concerns.

Regarding the reviewer's concern about the "provoking title", we agree that the wording is probably too strict. However, our study is not only about the "cyclone response", as we explicitly analyze how the climatological fields are composed conditioned on times with cyclones present and absent. It is correct that we cannot provide a conclusive argument for the observed differences, though climatological studies often have a more descriptive character. We agree that the verb "affect" might be too strong as it implies a causality that we did not prove. Furthermore, it is not strictly the climatology that is not affected by cyclones, but the response to changes in the intensity of the SST gradient that is not affected by the presence of cyclone. We thus thank the reviewer for her continued criticism of the title that now made us reconsider its formulation. We changed the title to "Smoother versus sharper Gulf Stream and Kuroshio SST fronts: Effects on cyclones and climatology" and hope that the new title addresses the reviewer's concerns. It now consistently reflects the presented analysis in our manuscript.

Response to minor comments:

l.138: in line

We thank the reviewer for indicating this typo.

l.221: perhaps, 'elongated' is better than 'distributed''

We agree with the reviewer and replaced "distributed" with "elongated".

l.437: In my opinion, fig. S13 shows only marginal increase in the number of cyclones. Even if the difference is significant, it is not clear to me how it explains the equatorward shift of the jet.

We agree that the increase in number might be small and reformulated the argument to reflect that the difference can be only partially attributed to the differences in cyclone presence. We have already previously indicated that we do not have a causal explanation for the jet shift and the paragraph in question also does not attempt to causally explain this shift, but to merely attribute those parts of the signal that might be interpretable based on the data at hand. To properly attribute the jet shift additional numerical simulations and a more extended analysis would be necessary, which is beyond the study at hand.

L.514-515: I find this statement on the direct influence of the SST front to be very strong. As said in Reeder at al. 'strong localised diabatic frontogenesis, which is amplified by adiabatic frontogenesis, can result in a front, which is consistent with atmospheric fronts in the region being most frequently located along the SST front' - I fully agree with this statement and new plots (S12-13) make me think that a very weak effect on cyclones my be due to characteristics of the model.

We, of course, agree with Reeder et al. (2021), though struggle to contextualize the reviewer's comment about the "weak effect on cyclones [being] due to [the] characteristics of the model”, as we do not understand what “characteristics” the reviewer refers to. In fact, all the lines the reviewer refers to in the comment above only directly relate to the study by Reeder et al. (2021) only dealing with fronts without a direct reference to cyclones. We thus propose to leave the paragraph as it is.

Also, in the phrase 'when no atmospheric fronts are present' replace 'fronts' with 'cyclones', as this paper does not explore atmospheric fronts

We refer to the study by Reeder et al. (2021) in this statement, which explicitly analyzes fronts. Therefore, it is correct to keep ’fronts’ in the sentence, as Reeder et al. (2021) did not analyze cyclones. We however rephrased the sentence to avoid the impression that we also considered fronts in the present manuscript.

Fig. S12: It is hard to compare S11 and S12 by eye. Would be good if fig. S12 showed SMTH-CNTRL.

We used these figures to argue that despite the change in the SST gradient between the experiments (CNTL & SMTHG) the low-level baroclinicity (T850, with purple contours) remains relatively unchanged. We explored the suggested possibility and concluded that a difference plot is not so well suited in this case, as the differences at T850 is affected by both changes in location as well as amplitude. Instead of a difference plot, we now include the delta in temperature across the domain, similar to Tsopouridis et al. (2021b), where we studied the ERA-Interim in the Kuroshio region. The presented arguments hold, except for C2, though these are the cyclones propagating away from both the SST gradient and the continent.

Response to Referee #2

We thank the reviewer for the constructive comments on our manuscript.

Response to the referee's general comments:

It is not clear to me what the motivation for this study is. Why perform the smoothed SST simulations in the first place? Are we expecting the SST gradients in the Gulf Stream and Kuroshio current to change in the future? Are the authors trying to say something about the response of the climate in coarse resolution models with low ocean resolution?

The main motivation is to understand the impact of the intensity of the SST gradient along these western boundary currents on the cyclones developing in these regions as well as how changes in cyclone behavior feeds back on the detected climatological differences. This motivation was clearly stated at the end of the first paragraph in our introduction. We nevertheless rephrased this sentence for further clarity and added a recapitulation of this motivation in the revised last paragraph of our introduction.

We do not claim that the investigated changes in SST are realistic. Similar to previous work with a similar approach, we aim to attain a more mechanistic understanding of cyclone response and their feedback on the climate in dependence on the underlying SST.

The conclusion from the second aim is ambiguous. The results show that the influence of cyclones on environmental changes due to smoothing the SST gradients is small. Does this mean that cyclones do not influence the environment in either simulation, or that their influence is large in both simulations but does not depend on the underlying SST gradients?

Thank you for raising this question. We also analyzed the atmospheric fields conditioned on cyclone presence or absence for different variables. For surface fluxes, cyclone and no-cyclone time steps contribute more or less equally to the climatological fluxes, whereas for precipitation, time steps with cyclones present tend to contribute more than when no cyclones are present. However, the main question the manuscript tries to address is how changes in the SST gradient imprint themselves on the climatology and how much these changes can be attributed to changes in cyclone characteristics. For this question, the contribution attributable to the presence of cyclones is small, also for precipitation, despite most of the large-scale precipitation usually occurring in the presence of cyclones. Given that the motivation for this study is to understand the impact of the differences in SSTs on cyclones and the climatological fields, we feel that including a discussion on the general influence of cyclones on the climatology in the Pacific and Atlantic would be beyond this study.

Furthermore, the authors conclude that 'cyclones play only a secondary role in explaining the mean state differences between the smoothed and realistic SST simulations'. To what extent are the mean state differences because there are fewer cyclones, i.e., it is the absence of cyclones in the smoothed SST gradient simulations that results in the large differences. If this is the case, then you could say that changes in the storm track position and a reduction in the number of cyclones play the dominant role in explaining the mean state differences. Perhaps this perspective is what the authors are referring to with their 'direct' and 'indirect' terminology? If so, this needs to be clarified.

The total occurrence of cyclones is not altered significantly enough (shown in the supplementary material) to explain the differences. The reviewer's argument was already considered in a previous round of review and we hence already included the cyclone densities in the previous version. We did not find evidence for the change in cyclone densities explaining significant parts of the signal, which is also what we argue for in our manuscript. Haualand and Spengler (2020) coined the direct and indirect influence of surface fluxes on cyclone development. We now refer to their definition and further clarified this in the manuscript.

I did not understand the title. What climatology are they referring to?

In response to concerns from both reviewers we changed the title to “Smoother versus sharper Gulf Stream and Kuroshio SST fronts: Effects on cyclones and climatology”. We hope the revised title addresses the concern of the reviewer and makes it more understandable.

It has been shown by Vanniere et al. (2017) and recently by Marcheggiani and Ambaum (2020) that cyclones tend to destroy the low-level temperature gradient within the cold sector due to a strong air-sea heat fluxes, but that it is restored within a few days following the cyclone passage. Could the authors comment on whether their spatially defined results for cyclone and non-cyclone environments are consistent with this temporal analysis.

This is an interesting point and we agree that our results could be interpreted in a similar way, though their definition of cold sector might often lie outside what we refer to as cyclone area. It has also been shown that cold air outbreaks, which contribute most significantly to the air-sea heat exchange, are sometimes only remotely associated with synoptically developing cyclones. We edited the manuscript to contextualize our findings with these studies.