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# *Interactive comment on* "SST fronts along the Gulf Stream and Kuroshio affect the winter climatology primarily in the absence of cyclones" *by* Leonidas Tsopouridis et al.

**Anonymous Referee #2**

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This manuscript investigates the effect that SST smoothing of the Gulf Stream and Kuroshio has on extra-tropical cyclones reaching maximum intensity in the region, and attempts to explain whether the well-known impact of the SST smoothing on the mean state can be better understood when cyclones are present or not.

The topic is important and very worthwhile exploring, however I have some serious concerns regarding the scientific approach and interpretation of results. This includes the lack of consideration of some previous work directly relevant to this problem, some questions over the data and methods used, and a lack of testing regarding the sensitivity of the results to specific definitions. Furthermore, I unfortunately find many of the



conclusions to be unfounded. Specific comments are listed below, I put an (M) next to ones I consider major.

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(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).

Parfitt, R., & Czaja, A. (2016). On the contribution of synoptic transients to the mean atmospheric state in the Gulf Stream region. Quarterly Journal of the Royal Meteorological Society, 142(696), 1554-1561. O'Neill, L. W., Haack, T., Chelton, D. B., & Skyllingstad, E. (2017). The Gulf Stream convergence zone in the time-mean winds. Journal of the Atmospheric Sciences, 74(7), 2383-2412. Parfitt, R., & Seo, H. (2018). A New Framework for NearâĂŘSurface Wind Convergence Over the Kuroshio Extension and Gulf Stream in Wintertime: The Role of Atmospheric Fronts. Geophysical Research Letters, 45(18), 9909-9918 Masunaga, R., Nakamura, H., Taguchi, B., & Miyasaka, T. (2020a). Processes Shaping the Frontal-Scale Time-Mean Surface Wind Convergence Patterns around the Kuroshio Extension in Winter. Journal of Climate, 33(1), 3-25. Masunaga, R., Nakamura, H., Taguchi, B., & Miyasaka, T. (2020b). Processes shaping the frontal-scale time-mean surface wind convergence patterns around the Gulf Stream and Agulhas Return Current in winter. Journal of Climate, 33(21), 9083-9101. Catto, J. L., Jakob, C., Berry, G., & Nicholls, N. (2012). Relating global precipitation to atmospheric fronts. Geophysical Research Letters, 39(10).

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.

(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 ($\sim$100km). It is also well known that you need eddy-resolving ($\sim$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?

Smirnov, D., Newman, M., Alexander, M. A., Kwon, Y. O., & Frankignoul, C. (2015). Investigating the local atmospheric response to a realistic shift in the Oyashio sea surface temperature front. Journal of Climate, 28(3), 1126-1147. Hewitt, H. T., Bell, M. J., Chassignet, E. P., Czaja, A., Ferreira, D., Griffies, S. M., ... & Roberts, M. J. (2017). Will high-resolution global ocean models benefit coupled predictions on short-range to climate timescales?. Ocean Modelling, 120, 120-136. Ma, X., Chang, P., Saravanan, R., Montuoro, R., Hsieh, J. S., Wu, D., ... & Jing, Z. (2015). Distant influence of Kuroshio eddies on North Pacific weather patterns?. Scientific reports, 5, 17785. Lee, R. W., Woollings, T. J., Hoskins, B. J., Williams, K. D., O'Reilly, C. H., & Masato, G. (2018). Impact of Gulf Stream SST biases on the global atmospheric circulation. Climate Dynamics, 51(9-10), 3369-3387.

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?

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.

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)

(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.

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.

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.

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

(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 mofidied by SST gradient, but what about all those that aren’t undergoing maximum intensification right there?

(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. 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. 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 $\sim$ 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.

(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 conveyer 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.

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

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

Lastly, I noticed a number of spelling/grammatical errors, I suggest a thorough check. (e.g. line 168, dipolar etc.).

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