Comment on os-2021-102
Anonymous Referee #1

Referee comment on "Seasonal extrema of sea surface temperature in CMIP6 models" by Yanxin Wang et al., Ocean Sci. Discuss., https://doi.org/10.5194/os-2021-102-RC1, 2021

I find that the paper is a valid contribution and I find that the methodology applied is right. I have few minor comments that the authors will hopefully polish for next version of the paper.

1. Page 2, line 58. The authors mention that they assign a 2ºC threshold to the biases in Tmax and Tmin to exclude points from the analysis. I wonder whether they have previously performed some sensitivity analysis to analyze the impact of this threshold. I guess that such an error might be more or less important at points with higher or lower variances of SST. I think this should be checked.

2. Page 4, line 70. “a small bias in a multi-model mean...individual models” seems like a little tautology to me.

3. I find that the RMSE values in figures 2, 3, 5 and 6 might be joined in a single table, so that a better analysis of systematic behaviors (if any) could be performed.

4. I find the palettes in Figures 5 and 6 difficult to grasp. Probably the fact that some isolated points are high in absolute value require a large scale. However, since most points are of a low absolute value, it is difficult to grasp the differences from one map to another. I suggest that perhaps the authors should use a non-equally spaced palette to increase resolution in lower values.

5. In Figure 7, I suggest some labels referring to regions are added to the individual panels to improve readability “WestEqPac” in panel “a)”, “NWIndOc” in “b)” and so on.
6. Page 12, line 120. I am not sure that the increase in storminess can be assigned to heat fluxes into the storms but separated from increased atmospheric baroclinicity (Kushnir, 2002), not explicitly mentioned by the authors. I think this point must be revised.

7. Page 12, lines 126-129. I guess that Myers et al. (2021) is a good reference to support the authors’ hypothesis here.

8. Page 14, lines 179-181. I suggest the authors to fit a simple sinusoidal signal here (period T=12 months) and the fraction of variance explained would allow the authors to show which areas respond to one or the other case.
