Weather Clim. Dynam. Discuss., referee comment RC2
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Comment on wcd-2022-3

Anonymous Referee #2

Referee comment on "Storm track response to uniform global warming downstream of an idealized sea surface temperature front" by Sebastian Schemm et al., Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2022-3-RC2, 2022

Summary: This paper examines the response of the storm track to uniform warming in an aquaplanet configuration of a GCM with a SST gradient added over a fixed longitudinal range in the midlatitudes. The paper also examines the response of individual cyclones. This paper is written in a clear manner, and the experimental design is interesting. The analysis utilizes a robust and useful EKE budget/framework that the authors have developed. The figures are all easy to interpret. The explanation for cause and effect are ok, but not expansive or 100% convincing, especially in terms of the regions that have a decrease in storm track activity within the tripole pattern.

My recommendation: minor but necessary revisions.

Minor comment that applies to the Abstract and the Conclusion sections.

As I read the paper, I couldn’t stop thinking about the stationary wave and its projected response to changes in the tropics with anthropogenic climate change (e.g., Wills et al. 2019). I note that this Wills paper does not provide a solution, and in fact does not touch directly on the issue of the storm tracks and stationary waves interacting (see the paragraphs under Fig. 4 in the Wills et al. paper). However, I think that this manuscript should include a bit more discussion on the role of the changing stationary waves in the CMIP6 models.

In the introduction, the authors discuss previous work that shows a role for changes in the tropics and subtropics affecting the storm tracks. But these ideas are not revisited later in the conclusion section. And the abstract seems to suggest that the entire response can be captured in an atmosphere without stationary waves. For me, it is tricky to understand how their model captures the tripole pattern of the North Atlantic storm track response to anthropogenic warming in a model with no mountains. Perhaps I am just mis-reading the abstract and conclusions – i.e., the change found in their idealized model is suggestive of the tripole pattern but incomplete – in which case, simple changes to the wording and some additional caveats would suffice.

Or, if you feel that the storm track response in your idealized model matches well with the CMIP6 models, then I would appreciate if the authors add a discussion in the conclusion section explaining why the presence of a stationary wave is not necessary for capturing the North Atlantic storm track response.

It might also be helpful to show the change in the 250 hPa jet in your 4K run for comparison with that from the CMIP6 models, e.g., Harvey et al. 2020, has the jet and the storm track available for a comparison. In it, you can see an intensification of the upper-level jet on the equatorward side of the Gulf Stream region. This corresponds to region 1 in your Figure 2 I think.

Harvey, B. J., Cook, P., Shaffrey, L. C., & Schiemann, R. (2020). The response of the northern hemisphere storm tracks and jet streams to climate change in the CMIP3, CMIP5, and CMIP6 climate models. Journal of Geophysical Research: Atmospheres, 125, e2020JD032701. https://doi.org/

Wills, R. C. J., R. H. White, and X. J. Levine, 2019: Northern Hemisphere Stationary Waves in a Changing Climate. Curr. Clim. Change Rep., 5, 372-389, doi:10.1007/s40641-019-00147-6.

Line-by-line minor comments:

L50: I would argue that Brayshaw et al. show the SST gradient to be secondary or tertiary, with the upstream mountain and the land-sea contrast both having more significant roles in setting the location and orientation of the Atlantic storm track.

L53-60: I agree with this summary. I think there are a few other recent papers that you might take a look at as they provide additional context for thinking about the role of SST fronts and storm intensification, e.g.,: Tsoporidis et al. 2021 and/or: Reeder et al. 2021

Tsopouridis, L., Spengler, T., and Spensberger, C.: Smoother versus sharper Gulf Stream and Kuroshio sea surface temperature fronts: effects on cyclones and climatology, Weather Clim. Dynam., 2, 953–970, https://doi.org/10.5194/wcd-2-953-2021, 2021.

Reeder, M. J., Spengler, T., & Spensberger, C. (2021). The Effect of Sea Surface Temperature Fronts on Atmospheric Frontogenesis, Journal of the Atmospheric Sciences, 78(6), 1753-1771.

L111: Given the strength and spatial extent of the SST gradient that you are imposing, its spatial scale, and the fact that you have tilted it off the zonal access, it seems like what you are doing is replicating a combination of the Gulf Stream and the land-sea contrast that exists in winter in the vicinity of the Gulf Stream extension. I think it would be best to include some statement to this effect.

Related to my big-picture comment above: the manner in which you have added the SST gradient implies that you are including something of a stationary wave in the model. I think this is something work mentioning. In your discussion of Fig 1a, you refer to a trough in the EKE field but I am curious to see the Z500 anomaly with respect to the zonal mean. Also, I wonder how the storm track responds of the SST front is not tilted, which is more like the Kuroshio.

L213: The difference in the Pacific is likely also related to the lack of co-location of the western boundary current and the coastline, and the fact that the Gulf stream has more north/south variation whereas the Kuroshio is relatively zonal and, there are clear differences in the meridional orientations of the jets above the western boundary currents in the Pacific (more zonal jet) and the Atlantic (meridionally tilted jet due to many factors, see L50 comment).

L277-279, you write:

“Apparently to the north of the SST front and downstream the re-organization by the SST front in the warmer climate is such that external baroclinic conversion efficiency increases, …”

This sentence is hard for me to follow. When I read it, I assume it is missing a comma after the word downstream. Is that all, or it there also a word missing somewhere?

L280: Your interpretation of the intensification being related to the eddies becoming more efficient in tapping the mean potential energy reservoir is interesting to me. By this same logic, in the region where baroclinic conversion of EAPE decreases, especially in the area south of the SST front, is your interpretation that the eddies in this region have become less efficient? Or is it simply that there are less eddies in that region?

L 297-300, you write:

“The change in resolved-scale condensation and evaporation, i.e., due to saturation adjustment, is of similar importance near the SST front in terms of magnitude. However, the absence of a clear dipole structure in the zonal mean indicates that the global increase in resolved-scale condensation and evaporation is stronger compared to that resulting from the cloud microphysics parameterization (Fig. 5b).”

I am not sure what you mean by this last sentence. The lack of a dipole in the zonal mean indicates what? And isn’t the lack of a dipole highlighting the fact that the condensation (presumably in the storms’ warm sectors) is the process that is changing the most with warming?

How does the lack of a dipole imply something about the cloud microphysics? I see that cloud microphysics does have a dipole, so I am have an idea where you are going with this, but could you expand on this?

L359: Figure 7: I would find it helpful if you placed the numbers 1,2,3 from Figure 2d on Figure 7. My sense is that the location of the changes in cyclone track density are not 1-to-1 with the location of the change in EKE. This is not a huge surprise. But some discussion of this would be nice.

L408-409, you write:

“the interaction between the diabatically-induced circulation and the cyclone also advects the cyclone poleward.”

Is this statement shown already in a specific paper? It seems like this would be another one that depends critically on the location the strongest diabatically-induced circulation.

L411: Do the poleward displacement changes have any bearing on the SST tripole in the storm tracks?

Related to this minor comment, here is a commentary that the authors can take action on or not, I leave it to you: the Lagrangian tracks element and the storm track + EKE element are a bit disconnected. Both relate to the SST front, but otherwise, the two elements are not currently woven together into a single story. This is not a game changer, as it stands, it seems a bit like you have two separate components in this manuscript.

L435: You write:

“The SST front organizes the flow such that the baroclinic growth becomes more efficient downstream of the front, that is, the eddy heat flux better aligns with the baroclinicity vector.”

This to me, is the one statement in the paper (and I acknowledge that this is also stated in the results section) where the author offer an answer as to why the present of the SST front might lead to the tripole pattern in the storm track response. Maybe I am missed some other statements on the matter? As it stands, this statement offers an explanation for the increase in the storm track, but it does not give an explanation as to why there are the two minima. Would any disruption in the zonal flow lead to this same response? You could at least test the question of the role of the tilt in the SST, at that would have some correspondence to the Pacific vs the Atlantic storm track basin.