Review of WCD-2019-6:

"Extratropical cyclone induced sea surface temperature anomalies in the 2013/14 winter"
by
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Recommendation: **Major revisions**

General Comments:

The Authors present an interesting analysis using ERA-Interim data to address the question how extratropical cyclones influence the SST in the Atlantic. They showcase one particular year that featured a significant SST anomaly and try to attribute a large fraction of this anomaly to anomalous cyclone activity in the same winter.

The manuscript is well written and the figures are clear, though the panel labels are sometimes difficult to see as they are on top of shaded figures. Overall, the paper presents a valuable contribution to the field and employs a novel diagnostic to attribute the surface fluxes to individual cyclones. However, there are several points in the paper that need further clarification, which are indicated in the comments below.

The mixed layer calculation has a caveat, because the authors assume that the depth has no variations throughout the year when they make seasonal budgets. One particular issue with that is that as the mixed layer depth changes, the sea state properties, in particular the stratification below the mixed layer, become important when the mixed layer depth increases. The actual heat content in the mixed layer will depend on the sea state below the mixed layer as well when net surface fluxes cause mixing. The entrainment of sea water below the column would need to be considered when the fluxes imply a net change in mixed layer depth. It would thus be interesting if the authors also show the seasonal tendency of the mixed layer depth in figure 3, not only the tendency in SST. Given the actual change of mixed layer depth together with the ocean stratification below the mixed layer could yield an estimate of the entrained energy into the changed mixed layer from below. This additional term in the heat budget could be accounted for and contrasted with the net surface forcing of the SST tendency.

Regarding the methodology of cyclone frequency, it is not clear if every cyclone is counted multiple times for the track densities of if some kind of anti-aliasing was employed. This would also influence how storm track activity is defined, as fewer but slower moving storms would yield a higher storm activity in terms of cyclone density compared to the same number of cyclones in a season with higher phase velocity. It would be great if the authors could further clarify how the cyclone track densities were calculated and how exactly one can thus understand an increased activity of cyclones. It would also be of interest if there were more extreme cyclones that particular year of interest, especially as the authors limit their analysis to the more intense systems.

A large fraction of the fluxes in the Gulf Stream region are associated with cold air outbreaks, of which a significant fraction is not necessarily associated with cyclones in the storm track region. Could the reduced QN fluxes in 2013/2014 south of the Gulf Stream region as well as in the Nordic Seas be thus actually associated with a reduced number of cold air outbreaks? For the Nordic seas, which also feature a significant anomaly in the presented analysis, Papritz and Spengler (J. Clim., 2017) showed that cold

air outbreaks account for the larges fraction of the surface fluxes in this region. Thus, the apparent anomalies are most likely mainly attributable to variations in cold air outbreaks and maybe only indirectly or in a reduced way associated with extratropical cyclones. Papritz and Grams (GRL, 2018) investigated the weather regimes associated with cold air outbreaks in the region of interest in the manuscript at hand. It would be interesting to put their findings and the given role of cold air outbreaks on the surface fluxes in the region in context with the presented findings.
In addition to cold air outbreaks, the role of cold fronts for surface fluxes in the Gulf Stream region has also been discussed recently, e.g., Parfitt and Czaja (2016) and other recent studies by the first author. It would be great if the authors could provide further context of the presented work to these studies.

The method to define the QN with the cyclone masks is not clear enough. It is difficult to follow what is actually summed up. At each time t for a given cyclone, the position of the cyclone and the preceding 30 hours positions are used, but is this done for every timestep in the cyclone evolution? How would this differ to just taking the swath with circles around all cyclone positions along the entire cyclone track? It would be great if the authors could provide further details about the employed method.

Specific Comments:
Page and line numbers refer to the ones on the manuscript.

P1 L7: The connection between the "cold wake" and "climatological variability" is not quite clear in this sentence. How is the size of the cold wake associated to climatological variability?

P1 L21: The argument about the role of cold fronts has also been discussed more recently, e.g., Parfitt and Czaja (2016) and other recent studies by the first author. What is the context of the presented work to these studies?

P2 L29: After citing the study by Zolina and Gulev (2003), the reader is a bit confused about the thus far identified fluxes associated with extratropical cyclones. If there is a controversy, it would be great if the authors could further highlight these conflicting results and possibly indicate as to why they are conflicting or if they will address these contrasting results.

P2 L28" …of the wind driven…

P2 L44: The authors comment on the role of ocean dynamics in the western Pacific, where oceanic advection probably plays a dominant role. However, the reader is left wondering if not similar arguments would also apply to the western Atlantic, the focus of this study, where strong oceanic currents are present. Are there no studies quantifying the role of oceanic anomalies in the western Atlantic? Good if the authors can also comment on the region of their interest in this context.

P2 L51: Another, more direct, connection between cold air outbreaks, cyclones, and the low-level baroclinicity in the western Atlantic is provided by Papritz and Spengler (2015) as well as Vanniere et al. (QJ, 2017).

P5 L128: "the winter"

P7 L140: See general comment about change of mixed layer depth throughout season. Some additional discussion about the influence of mixing and entrainment in the ocean would be valuable.

P7 L144: “heat fluxes occur”

P7 L147-149: This is also the argument of a recent study by Ogawa and Spengler (2019), who also emphasized the role of synoptic eddies on the climatological fluxed in the mid and higher latitudes.

P9 L183: “the cyclone lifecycle”

P11 L203: “the surface flux”

P12 L216: It is not necessarily obvious from the referenced figures that the storm track was more active, see general comment on cyclone track densities.

P13 L223: It is difficult to see how the QN anomaly and the storm track anomaly is “consistent”. There appear to be more cyclones detected over the Gulf Stream region in the anomalous winter, though the net negative QN fluxes in this region appear to be reduced when compared to climatology. How can this be reconciled with the previous findings of the cyclone relative QN fluxes and SST changes?

P14 L225 and following: The methodology is not quite clear, see also general comments.

Fig. 9 caption: “red crosses show”

P15 L246: “conclusion does not”

P16 L250: It is not clear that the results indicated in this paragraph consider the data based on the cyclone swaths from the previous section.

P16 L254: The actual percentage of the SST difference cannot be really directly contributed to the fluxes, as it is a mix of local fluxes and advection, as well as entrainment from below that caused the total change. There might be compensating effects that cannot be accounted for in such a crude attribution without actually calculating a full budget considering all tendency terms.

P17 L262: Can the authors comment further on the relative contributions of potential other effects that make the attribution to individual cyclones difficult?

P17 L266: The statement about “higher than average cooling” appears to be rather regionally confined and there were also larger areas where this particular season featured reduced air-sea heat exchange. The authors should comment on this complex structure and put it in context to the observed cyclone distribution. Especially the western Atlantic area with reduced fluxes appears difficult to explain given the increased number of cyclones (Fig. 8f, 11d).