

Reviewer comments to: Zibell et al., revised for publication in Weather and Climate Dynamics

The authors revised the manuscript to address the concerns from the reviewers. My greatest concern was the readability of the manuscript, and it has been improved. The scientific results are understandable. However, the presentation and readability could still be improved, and the analyses could be better organized compared to other papers in this journal. Specifically, the connection between the three result sections can be enhanced, and readability can be improved by removing some details that may hurt the flow. Below I list some remaining concerns and suggestions for revision.

Major comments

1. There are three large sections in the paper. These three sections can be better connected.

1.1. For example, the analyses in section 3 can be used to justify the flux attribution method, which is used in sections 4 and 5. Where does the flux threshold roughly locate, if put on Figures 4 and 5? This will demonstrate that the flux attribution method is better than simply using cyclone masks.

1.2. In Figures 3 and 4, cyclones with the deepest minimum and strongest intensification are defined as the top 10%. In Figure 8, the authors investigate the strongest and weakest 200 cyclones. Then in Figure 10, 'strong' refers to the top 25%. The definition varies from section to section, making it difficult to perceive the analysis as systematic. Having consistency throughout sections will improve the flow of the analyses. This is something I pointed out earlier, but has not been addressed.

1.3. In sections 3 and 4, analyses of strongly intensifying and strong cyclones are emphasized. However, in section 5, categorizations by intensification and intensity result in no robust relationships between heat transport and cyclone numbers (lines 442-444). The paper starts as if strongly intensifying and strong cyclones would be important, but in the end, they are not. It would be better if this disconnect could be reconciled. More broadly, the robust conclusion from section 5 is that cyclone numbers and total atmospheric heat transport are correlated (Fig. 11). This conclusion does not rely on flux attribution or categorization by intensity or intensification rate, which makes it not synergistic with the conclusions from the previous sections.

2. There are details and discussions that can be neglected, or don't seem to be too important.

2.1. Is Equation 1 used in the main analyses? If not, it can be removed.

2.2. Lines 35 to 55 is a long single paragraph containing different key ideas. Lines 35-39 can be a different paragraph from the rest or be removed.

2.3. Lines 270 to 275. Is this detail necessary for the whole paper?

2.4. Lines 288-289. This seems like an unnecessary detail.

2.5. Lines 472-474. This seems like an unnecessary detail.

2.6. Lines 521-526. The authors discuss in one sentence to remove stratospheric contributions, but in the next sentence discuss that stratospheric contributions are already small. Please revise.

3. Some unclear presentations and discussions can be improved.

3.1. Lines 291-293. How can we say that the cold sector is broader than the warm sector from the figures?

3.2. Descriptions around lines 345-354 (Figs. 8c and d) are difficult to follow.

3.3 Lines 301-302. This seems like an overstatement according to Fig. 6c.

Minor comments

-Line 62: Please clarify what are 'signals'.

-Line 266: hPa is missing after 850.

-Line 319: Is this correlation calculated by taking each year as an independent sample?

-Line 380: 'extent'?