Review of "Composite sharpening by vortex symmetrization and normalization of tropical cyclones" by Andrina Caratsch et al. (Manuscript ID: egusphere-2025-6186)

Overview: The authors introduce a new technique for compositing tropical cyclone structure and physical processes by symmetrizing vortex structure using 10 distinct detection sectors for the storm's radius of maximum wind and outer size. While preserving information about these different radii to allow for investigations of storm asymmetry, they then produce a normalized, storm-centered, Cartesian analysis that the authors argue sharpens mesoscale structures like supergradient winds and eyewall convective heating. The authors contend that their "SyNC" technique can strengthen the analysis of composite TC structure, particularly as the community continues to approach global storm-resolving modeling.

I recommend major revisions. The manuscript is well-written and organized, the figures are particularly engaging and well-designed, and the technique described throughout the manuscript has potential to address important problems in TC structural and climatological research. I have no doubts that it will be a valuable addition to GMD in time, and many of my comments below are more minor in nature, asking for further clarification at points throughout the manuscript. Some of my outstanding questions are more significant, however, surrounding the applicability of SyNC to other modeling frameworks and its ability to reliably capture asymmetric TC processes such as vortex tilt and eyewall replacement cycles. I will outline these major comments below with specific examples, then conclude with a list of line-by-line minor suggestions. Thank you for your effort on a compelling manuscript, apologies for my delay in reviewing, and I look forward to reading the next version!

Major Comments:

1. *Applicability for some important mesoscale structural components of TCs*: The authors do well to explain how SyNC captures axisymmetric features like supergradient outflow above the boundary layer, diabatic processes in the eyewall, and subsidence within the eye. They also argue that their methodology accounts for structural asymmetries caused by storm motion, vertical wind shear, and vortex tilt. While I would agree that sharpening boundary layer and eyewall processes is useful, I think there are other applications that are important for SyNC to be a "game changer" in the field. This includes:
    a. Lines 16-17, Line 356: You discuss challenges of the technique when the vortex does not exhibit a Rankine-like wind profile. You also mention some uncertainty where outer latent heating maxima could be interpreted as eyewall or rainband features. **My question: Could SyNC be applied to identify and track secondary eyewall development?** I am admittedly

unsure if ICON in its current form would explicitly simulate such a process (though the 3 km HAFS has encouragingly done so in recent years). I will suggest that the authors at least try to perform a case study on an eyewall replacement with SyNC, but if this is not possible, at least some discussion/speculation in the Conclusions section on whether or not this can be done would be useful in my view.

   b. Lines 159-160: Allowing the RMW to be different at each vertical level, to me, would make eyewall convection that is normally slanted appear more upright in the resulting composite. So I agree that eyewall tilt is accounted for. But would this actually account for *vortex* tilt? Is the center allowed to vary with height based on a different threshold such as a geopotential minimum or vorticity centroid? See Ryglicki and Hart 2015 for an overview of different center finding techniques aloft, which has since been expanded upon. As written, it is unclear how vortex asymmetries are accounted for outside of the horizontal plane.

2. *Methodological questions that may need to be expanded upon*:
   a. Lines 133-134: Are genesis and lysis defined as the first and last time steps identified by your tracking algorithm? I mostly ask because the definition can vary significantly, especially for lysis where powerful extratropical cyclones can be perhaps mislabeled as "decayed" after transition.
   b. Lines 153-154: For reproducibility and applicability to different model frameworks – how straightforward is it to alter the resolution of the projected grid, and is it important that the projected grid approximately match the effective resolution of the model you are applying the SyNC technique to? This should be discussed in the manuscript, in my view.
   c. Line 268: "While weaker storms are relatively rare". Are they? This may be an artifact of the tracking methodology, where you filter out any TCs that do not achieve a lifetime of maximum intensity of 990 hPa or lower. Bourdin et al. (2024) introduced a "Category 0" (1005-990 hPa) to account for simulated tropical storms, which you may consider including in your analysis while keeping the longevity criterion you use in your tracking. You already acknowledge that SyNC has challenges with weaker TCs, however, so if you choose not to expand your analysis to >990 hPa TCs, you should at least acknowledge the tracking approach as a factor in the intensity bias.
3. *On the discussion of statistical power and "reduced within-group variance"*: I will repeat that I see the utility of SyNC in sharpening inner-core features. But is reducing variance necessarily a good thing? On one hand, differences between composite means are more likely to be significant and physically meaningful when there is little variance within the sample. However, many researchers may be interested in explicitly quantifying the *variability within a composite*. Can SyNC do this? You may consider briefly discussing this application in the Conclusions.

Line-by-Line Minor Comments:

- Line 6: Briefly specify in the abstract how the size of the TC is defined.
- Line 36: Another excellent paper to reference when discussing VWS would be Rios-Berrios et al. (2024) - A Review of the Interactions between Tropical Cyclones and Environmental Vertical Wind Shear
- Line 42: Again, how do you define size here? The radius of 17 m/s winds? The radius of the outermost closed isobar? Some other metric? On that note, add “wind” before “radii” on Line 44, assuming you use R17 as your outer size metric.
- Line 55: Minor typo – correct “withing” to “within”.
- Lines 83-84: Are the monthly mean SSTs you refer to specifically from 2005, or are these averaged across a broader climatology? Specify this, and what dataset SSTs are prescribed from.
- Lines 106-108: Be a bit more specific about the tracking algorithm (though I appreciate that you allude to Enz et al. 2023 for more details). What is the necessary amplitude of the local pressure minimum/vorticity maximum/upper-level temperature anomaly? What specific vertical levels are you looking at for the latter two? Also, I believe Line 107 should be changed from zeta_min to zeta_max.
- Lines 263-265: I would argue that a 5 km model simulating sub-900 hPa TCs is not very surprising, given that the community has achieved convection-permitting operational NWP resolutions with models like HAFS and NAM that do explicitly simulate Category 5 hurricanes. It may be worth briefly mentioning any known biases that ICON has in simulating TC structure, climatology, and intensity from other recent studies, besides just noting that Judt et al. (2021) found a better alignment between observed and modeled TC frequency/ACE with 5 km resolution.
- Lines 283-284: Can you verify the direction of the wind shear in this particular example to support this claim? A very simple technique you can use is averaging the 200 and 850 hPa winds in a 200-800 km radial annulus surrounding the TC to calculate the vector difference. Consider including an inset on Figure 5 with the motion and shear direction arrows.
- Figure 7: Consider adding sample size information into this figure or the corresponding text discussion, and reminding the reader in the text that you are classifying TCs according to their lifetime maximum intensity for Figures 7d-g, so that TCs near the beginning or end of their life cycles could vary widely in their intensities within an individual composite.