Review of 'Composite Sharpening by Vortex Symmetrization and Normalization of Tropical Cyclones'

In this manuscript, the authors develop a new compositing framework for tropical cyclones (TCs), termed SYmmetrized-Normalized Cyclone (SyNC) composites, intended to address limitations of traditional center-aligned composite approaches. The authors argue that standard TC composites tend to smooth storm-scale features because storms of varying size and structure are averaged together without accounting for differences in eyewall location or storm extent. To address this, the authors detect the radius of maximum wind (RMW) and the outer storm radius defined by the 17 $\mathrm{m\,s^{-1}}$ wind threshold in multiple azimuthal sectors and at each vertical level, and use these radii to symmetrize and radially normalize individual storms prior to compositing.

The method is demonstrated using convection-permitting ICON simulations of the 2005 North Atlantic hurricane season (a very active one, I might add), with ICON-simulated storm characteristics compared against observations. The authors show that SyNC composites retain 'sharper' representations of eyewall- and vortex-relative features, including vertical motion and diabatic heating, compared to traditional center-based composites. They argue that the approach reduces within-group variance and improves the ability to distinguish between storm categories, while noting reduced applicability during early storm stages when coherent vortex structures are not yet established. Overall, the study frames SyNC as a tool for analyzing vortex-relative circulation and microphysical structure in mature TCs, particularly in high-resolution model output.

There are a mix of strengths and weaknesses in the manuscript, in my eyes. Some aspects I find particularly appealing are the standardization that removes latitude dependence and the ability for a user to apply different levels of normalization (with SyNC representing an "extreme" normalization, and a simple storm-centered composite being more "moderate," etc.). I also think the ability to retain an asymmetry index/factor is a unique aspect of the framework and provides useful insight into the symmetry of the storm. This could provide insight during a storm's lifetime or across storms with similar characteristics.

However, some elements of the normalization strike me as overly aggressive, and I am not fully convinced that the resulting increase in composite sharpness necessarily translates to improved physical insight. In particular, the treatment of vertical structure raises questions about whether important, well-known aspects of TC dynamics (e.g., central vortex tilt) are being suppressed.

Given this, I think the paper could be suitable for publication in GMD eventually, but with some required revisions to address the comments below. More details follow, but broadly I believe it would be helpful for the authors to very clearly define the tradeoffs inherent in the SyNC approach, including which physical characteristics of tropical cyclones are preserved versus removed by the normalization procedure. There is also room to more clearly spell out the rationale for specific design choices and to more explicitly link parts of the work to existing literature.

Major comments

- The physical tilt that occurs in the eyewall is a known behavior associated with angular momentum conservation (see Stern and Nolan [2009] and references therein). I am not 100% convinced that removing this tilt and vertically aligning the eyewall (Fig. 8) is useful in that context. I am not necessarily saying there is no merit to this approach, but I think the authors need to do a better job justifying the consistent normalization with height when the storm is not expected to be vertically aligned, even when the vortex center at each layer is first computed and then stacked. I suspect this step is fundamentally where much of the composite "sharpening" comes from, but it has not been made clear to me that this sharpening reflects physically useful information.

- Some decision choices need to be more concretely defined. For example, the 8:1 ratio for R17:RMW appears to be drawn from Chan and Chan [2012], although this is never formally stated. More generally, regarding TC structure between the RMW and the outer radii, I encourage the authors to review and engage with some of Dan Chavas' work on the topic (e.g., Chavas and Knaff [2022], Chavas et al. [2025], and references therein).

- The model verification section (Section 3) feels somewhat tacked on. There are no spatial maps or other visualizations commonly used for model evaluation. The authors do compare some broad statistics in Fig. 3 that provide confidence that the model produces a reasonable spatial distribution of TCs (e.g., via the latitude metrics in the bottom row), but it would be helpful to formalize this evaluation further. I am not familiar with the Enz tracking algorithm, but I assume it has been vetted against reanalysis products and is able to track realistic tropical cyclones. Biases in the tracker may also lead to some biases in statistics.

- Regarding the model configuration itself, it may be worth providing more context in the limited-area model (LAM) discussion. Given the size of the domain, I would not expect lateral boundary forcing from ECMWF HRES to strongly constrain the interior solution, although this is not definitively stated. Based on the analysis focusing on the statistical distribution of simulated TCs, it appears that no interior nudging was applied and that each ensemble member developed its own internal meteorology. In addition, the use of prescribed SSTs does not permit cold wakes or other air-sea feedbacks, and this limitation would be worth explicitly noting (there is substantial literature on this issue over the past decade).

- Regarding the TC pressure-wind relationship (Section 3.1), I do not think this analysis materially affects the main results, but I found the inclusion of this section somewhat confusing. Pressure-wind relationships are typically defined at the surface and include the correction from gradient wind aloft to the surface through turbulence. They are a useful diagnostic for evaluating models because they help link resolved dynamics with near-surface parameterizations [Chavas et al., 2017, Nardi et al., 2022] and provide information about effective model resolution [Reed et al., 2015]. It is therefore unsurprising that the pressure-wind relationship degrades above a few hundred meters. If winds at or above ~1 km height were better 'matched' to observations in this framework, it might actually suggest a poorly configured model.

- The authors argue that SyNC composites enhance statistical power (Section 6.2) by reducing within-group variance, enabling the detection of differences between TC groups that would otherwise be missed. While this is a reasonable expectation in principle, I am concerned that the demonstration is somewhat circular. Because the method forces RMW to a normalized radius of 1 by construction, it is unsurprising that variance decreases near the eyewall and that more grid cells pass significance thresholds there. The paper would be strengthened by showing that newly significant regions correspond to physically expected and interpretable contrasts between weak and intense storms, rather than focusing primarily on the number of significant grid cells or p-values at individual locations.

- I understand the motivation for comparing traditional center-based composites to SyNC in later figures, but I wonder whether comparing two model configurations (e.g., different parameterizations or resolutions of ICON, or ICON versus ERA5) would provide a more informative test of the method than stratification purely by intensity. As an aside, while not required for this manuscript, I would have been interested in seeing ERA5 evaluated within this framework.

Minor comments

- Composites can be constructed in latitude-longitude space by projecting fields to great-circle distances. This is implicitly what the authors mean when they say "bring TCs to the equator" in Line 27, but it may be worth stating this more explicitly. Note that there is no requirement for longitude to be fixed, as the only geometric distortion arises from converging meridians; a cyclone projected to any longitude will appear identical as long as it is at the equator.

- Line 226: This is perhaps semantics, but the simulated central pressure is more reliably simulated (compared to surface wind) in lower resolution models (e.g., 25km) due to the effective resolution of the models not being able to support observed RMWs and the associated inner core pressure gradients

[Chavas et al., 2017, Hodges et al., 2017, Zarzycki et al., 2021]. In Judt et al. [2021], the pressure-wind errors are likely more tied to surface layer schemes or PBL parameterization since the dynamical cores should (in theory) be able to capture RMWs down around 15-20km.

- Line 283: It may be worth briefly calculating vertical wind shear here. A common approach (e.g., DeMaria and Kaplan [1999]) is to remove the vortex and then compute the shear between two levels (e.g., 850-250 hPa). It would be easy and would add some confirmation to the speculation in '... likely due to wind shear pushing the vortex...'
- Line 366: What do the authors mean by "saturation adjustment"? I presume this refers to tendencies associated with parameterized latent heat release in ICON. The linkage between this discussion and the subsequent figures could be clearer in the text.
- The authors discuss extratropical cyclones a few times (in the abstract and introduction). My preference is to perhaps remove or downplay them, particularly in the abstract, since all work here is axisymmetric.

Typos

- Line 48: "loosing" should be "losing"
- Line 55: "withing" should be "within"
- Line 80: The domain is listed as "105° E to 18° E," but this is the North Atlantic; these should almost certainly be °W, not °E.
- Line 88: "Cloud droplet's growth" should be "Cloud droplets' growth"
- Lines 80 vs. 256: The northern boundary is given as 45° N in the domain description, but later the text refers to the domain being limited to 55° N. This inconsistency should be resolved.
- Line 123: 'dynamic core' should be 'dynamical core'
- Line 258: "median central pressure median of 938 hPa"
- Line 264: Might be worth also citing Davis [2018].
- Line 273: R33, R25, and R17 are referenced for validation, but only R17 is formally defined. Given the use of standard TC wind thresholds (34, 50, 64 kt), it would be helpful to clarify these definitions.

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