Dörffel et al. use two measures of the transfer of kinetic energy across scales to investigate the dynamics in tropical cyclones. Using the DRI index, which measures kinetic energy transfer across the scale of 30km, the authors identify local regions of upscale and downscale KE transfer, and show altitude and temporal dependence of the KE flux. They also compare axisymmetric and asymmetric components of the rate of conversion between KE and APE at a larger scale.

The study is broadly aimed at introducing a new methodology for investigating the cross-scale production and transfer of KE in a TC, which I found interesting and valuable. I can imagine how the method would indeed provide insights into the structure of energy transfers around TCs, and so it seems like the main point came across well. The included analysis was largely suggestive rather than being compelling, which is appropriate given the stated motivation of introducing a method. I hope my comments below make the method more intuitive so that others use it in the future.

My main comment (1 and 2 below), is that the introduction leads the reader to believe the "asymmetry" the paper is focused on is related to centerline tilt, which in my terminology below is "ensemble-mean asymmetry". However, the results section proceeds to mostly ignore centerline asymmetry (because the TCs are basically vertically oriented) and discuss what I call "single-timestep asymmetry". I think a better exposition of what is meant by "asymmetry" would greatly strengthen the paper, as the methods and results seem interesting. There are also some scientific claims of statistical significance that need revisiting.

General comments

1. It seems useful to me to distinguish two different types of "asymmetry". Consider an idealized TC with no tilt, perhaps in a simulation at steady state. The fields representing the ensemble mean state, which could be obtained for example by a long time average, would be axisymmetric about the TC centerline. Nonetheless, any given timestep would clearly not be axisymmetric, for example if there was a stronger transient inflow on one side of the TC than the other. It seems the authors are initially discussing single-timestep asymmetry, e.g. the framing around "flow structures" and "convection patterns" (L19) read to me as describing single-timestep asymmetry. The authors contrast that example with Pauluis and Zhang who "restrict to symmetric structures by azimuthally averaging the data effectively considering an axisymmetric storm" which in fact would eliminate both types of asymmetry. It then sounds like the Bhalachandran citation (L32) is again discussing single-timestep asymmetry if the azimuthal fourier modes are computed for individual time steps.

   A transition occurs at L37 with the statement "Asymmetries of the flow fields originate from asymmetries in the environmental conditions," which would only apply to ensemble-mean asymmetry and not to single-timestep asymmetry. Because the atmosphere is chaotic, a nearly perfectly uniform initial condition, with azimuthally symmetric forcing, would result in a TC that is not azimuthally symmetric at any given timestep. An isolated cloud could form on the north side, for example. Thus, the novel tilted-centerline approach is only advantageous relative to a vertical centerline for the case when the ensemble mean TC is tilted (which it is when shear is present as the authors argue).

   Later, it is clear that the analysis, using both G_1 and the DRI, only analyze single-timestep asymmetry and do not consider ensemble-mean asymmetry which, for the CM1 simulation, would correspond to centerline tilt. There is therefore a tension with the introduction which seemed to emphasize ensemble-mean asymmetry (e.g. L37,L86).

   Some specific lines could be revisited:

   - L37 as mentioned above - true for ensemble mean, false for single-timestep. Overall the paragraph appears to only discuss ensemble-mean asymmetry, which is in tension with the methods.
   - L47 seems to be setting up the tilted centerline approach, which accounts for ensemble mean asymmetry, as being a motivation for the study. As I describe below it is not considered in the results section.
   - L81 "few studies investigated the role of the spatial organization of backscatter on the localized dynamics and on the vortex intensity" - could refer to either
   - L86 "Since we argued that it is merely the vortex tilt that couples to asymmetries of the convection field" - only applies to ensemble-mean asymmetries, again in tension with methods

2. The novelty of the tilted centerline seems to be, if not the primary goal of the paper, a moderately significant motivation, e.g. L84 "The purpose of our study is to present a proof-of-concept methodology to evaluate the organization of across-scale energy pathways around a tropical cyclone core, thereby acknowledging the importance of the tilted centerline."

   After the introduction I was expecting the paper to quantify the advantage, or at least to focus on the tilted vs. not tilted centerline approaches more. Perhaps the introduction should just be worded differently, because there was no such analysis.

   And in the conclusions, L405 states "we observe that the structure organize along the tilted centerline." which seems a bit tautological, given how the structures were defined, which the authors agree with from the following sentence. The authors also state (L409) "both cases did not provide substantial tilt." which I agree with, based on Fig. 4. This is in tension with the introduction where the centerline tilt was framed as being a novel consideration (e.g. L47 as I mentioned above)

3. The reanalysis data has a grid resolution that is effectively equal to the filter scale used for the DRI calculations, meaning that the increments $\delta\mathbf{u}$ used to compute DRI are probably differences between adjacent grid points. This raises concerns given that models rarely resolve the flow accurately all the way to the Nyquist, and the scale range analyzed is within the hyperdiffusive range for the model. Because the DRI measures KE flux across scale, the analysis measures the transfer between resolved KE flux and SGS flux which would be parameterized. The authors state a related worry on L371, but the ERA5 analysis is very difficult to interpret in light of this. I actually find the analysis of the CM1 simulation sufficient given the goals of the study. ERA5 is fine to include but would need a much larger filter scale, which may or may not make sense.

4. Setting aside the ERA5 analysis, the dataset contains one RW event and one or possibly two RI events. Correlation and predictability claims are therefore overstated in the conclusions especially (L415: "the presented analysis opens a promising route towards understanding and predicting TC intensity changes... For all the presented test cases clear signals preceded or correlated with these events of interest.") The "prediction"/"preceding" language seem to refer to L336 ("changes in the Fourier-mode-1 energy transfer actually precedes intensity changes by about 3 h."). Presumably this is referring to Fig. 8 but I do not see evidence of the 3-hour lag there. Such a claim would need to be shown to be robust to the significant variability present in the red line in Fig. 8.

   For the correlations claims (Fig. 3, 7, 10, 12, L266, L322, L357), Spearman's correlation coefficients are computed and p-values are given. The p-values given at L323,L366 seem to assume independence of the data points in the figures, which does not hold for time-correlated data such as this, so the p-values vastly overestimate the significance. I would want many independent TC simulations, or a very long simulation with many independent RI/RW events, to establish claims such as these. Luckily, these claims don't seem load-bearing for the main stated goal of introducing novel methodology.

Specific comments

1. The DRI is later described (e.g. 4.1.1, L379) as a measure of divergence in addition to scale transfer of KE. I think this is a plausible interpretation, based on the dot product of the filter function gradient with the velocity increment in the definition, but feel somewhat uncertain myself. If the authors think it's a reasonable interpretation, I would lightly suggest adding it to Sect 2.3, it might help give a reader intuition. If it's not a good interpretation, the phrasing in Sect 4.1.1 (L278,L281) should be reworded.

2. What's the reason to use APE and not moist APE? I.e. potential temperature is used rather than equivalent potential temperature. Can the authors comment on this choice? One would expect latent heat release to be important in a TC.

3. DRI is presented in normalized units on plots, but it has units of $\mathrm{m}^2/\mathrm{s}^3$ from Eqn. 6, which seem interpretable to me as the energy dissipation rate at that scale. What is the reason to nondimensionalize here?

4. L90: "without relying on restrictive assumptions inherent to spectral analyses" Which assumptions?

5. Equation 3 seems to define x as a vector ($x \in \mathbb{R}^3$), whereas in Eqn. 4 it is a vector component. Can `\mathbf{}` be used?
6. L130: "In addition, the vertical curvature was approximated at every horizontal slice to then discard centerlines that exhibited nonphysical oscillatory behavior." Unsure what exactly this means. Vertical curvature of the centerline? Oscillatory how, and why would oscillation be unphysical?
7. L442: Revisit this paragraph. Was it copied from the main text in an earlier draft? e.g. "Details of the energy-budget equations are given in appendix A." - but this is appendix A.
8. L18: On my first reading I wasn't sure to what "symmetric" and "asymmetric" referred to. It is only later stated these are relative to the TC centerline (i.e. axisymmetry), and then only implicit around L24/25.
9. L46: Unclear what "fluid structural properties" refers to.
10. L51: Unclear what "scales of asymmetry" means.
11. L129: Up to the authors: if it's simple, the method could be described in short.
12. L280: My interpretation: Masking only the positive values captures only those vertical levels which constitute the boundary layer, regardless of the actual height of the boundary layer at that time. In other words, the BL is defined by those levels in which there is a positive DRI. Correct? You don't actually show "the temporal evolution of the BL top", which reads to me as describing the BL height as a function of time. Perhaps this could be reworded, assuming I understood the issue?

    In any case, I am fine with this definition of the BL as any definition is somewhat arbitrary regardless. There does seem to be a clear BL signal in Fig. 2.

    What about the converse, the negative values above 11.5km?
13. Figure 4 is very cool.
14. L297: I'm not quite sure I agree here, just based purely on the visuals in Fig. 4. I don't see the width of the structures significantly change across a-f, contrary to "positive and negative contributions concentrate towards the center of the storm, while the weakening phase (fig. 4c,d) is accompanied by wider dipole DRI structures". I also don't see any "emergence of a dominant symmetric structure" here. For one thing, it's not clear how you could even tell whether the structures are symmetric given that we are only seeing one side of the TC.

    I also don't quite follow the offered explanation. Parameterized entrainment/detrainment are tied to convergence/divergence, but how does that relate to narrowing or broadening of the cyclone? Also, parameterized entrainment/detrainment would be a sub-4km phenomenon, whereas the plot shows 30km convergence/divergence. Presumably a 30km phenomenon is well resolved at 4km, but it's certainly possible that an unrealistic parameterization could propagate error upward. I do think the scale discrepancy could be mentioned. But, in any case, an explanation might not be required here given that I don't quite see a significant change to the TC width or its symmetry properties.

    For the same reason I disagree with L411 "mostly symmetric in the boundary layer, and mostly dipolar in the bulk of the vortex"

    Also, without height labels it's difficult to tell where the "upper mid-troposphere" is in the 3D visualizations.
15. L315 states "[the diabatic energy transfer equation] corresponds to the kinetic energy increase due to the symmetric and the asymmetric diabatic heating". I don't see this spelled out fully, but if I interpret it correctly, the symmetric component corresponds to G computed using the Fourier-0 mode because the zero mode corresponds to the mean, which is azimuthally constant (and therefore axisymmetric). In contrast the asymmetric component is largely the 1-mode with lower order contributions from higher modes, and therefore that component varies azimuthally given that it's a wave. If this is correct, can it be spelled out a bit more, or can the text be clarified if I got this wrong? Lines 471, 326, 143 all seem to basically state this, but I think it should be more explicit, probably around L143 where it's introduced.

Technical corrections

1. L16: Please define RI and RW.
2. L29: `\citep`
3. L191: "stems" - right word?
4. Page 9 - Can the units be converted to SI (or SI reported alongside) for comparison with the idealized case? Particularly wind speeds.
5. L344: did you mean quantitative? also "a" missing on L345