Reply to the referee reports on the paper

Characterizing inertial and diabatic energy transfers in tropical cyclones
Changed title:
Dynamics of tilted atmospheric vortices under asymmetric diabatic heating

by Tom Dörffel, Natalia Mikula, Lisa Schielicke, Theresa Kiszler, Davide Faranda, Bérengère Dubrulle, Nikki Vercauteren

Reviewer 1:
We thank the referee for their very valuable comments and suggestions, all of which have been accounted for in preparing the revised version of this paper as described below. Next to the revised version of the manuscript, we provide a color-coded version highlighting the changed text passages in red for better detectability.

First, we address the **general comments** given by the referee:

(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 does 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.*

We generally agree with the concern the reviewer raises, and we thank the reviewer for stimulating the following thought: While an initially upright vortex (*i.e.*, without tilt) will develop individual convective cells that clearly break symmetry on the level of individual time steps, in an ensemble-mean sense the vortex remains symmetric, as the locations of the individual manifestations of the convective events average out to axisymmetric patterns. Referring to the work of Dörffel, Papke, et al. (2021) and Dörffel (2022), we argue that asymmetries in the environmental conditions (*i.e.*, vertical wind shear and/or asymmetries in the sea-surface temperature) lead to asymmetric flow structures *i.e.*, a tilted vortex. That again, however, acts to condition cloud convection, and – in the sense of multiscale cloud convection – is leading to asymmetry that persists ensemble-averaging. The result is the large-scale organization of convection that is controlled by both, the by environmental asymmetry and that of the storm itself (Wadler et al., 2021; Alvey, Zawislak, and Zipser, 2015; Callaghan, 2017; Rios-Berrios, 2020). We would therefore rather introduce the distinction between *cloud-scale* and *vortex-core scale* asymmetries.

On the vortex-core scale, the convection pattern as well as the flow pattern can thus be considered as a composition of both, symmetric and asymmetric components that are invariant under ensemble-averaging. The two methods we propose, implicitly (the DRI) or explicitly (the diabatic transfer) allow for these vortex-core scale asymmetries while they are not sensitive to the cloud-scale asymmetries. We want to point out that the diabatic transfer is explicitly constructed around the notion of vortex-core scale asymmetries.

To strengthen the point above, we want to mention that both methods are genuinely spatial filtering operations, and as such they disregard the individual localization of cloud-convective ensemble realizations rather than posing an *almost* statistically invariant filtering operation by averaging over the vortex-core scale.

We used the term *almost* statistically invariant, and point out that the spatial filtering is not a Reynolds (idempotent) operator. This property would be necessary to allow for the methods to be used disregardless of applying them to single-time step or ensemble-mean data. By choosing a filter scale that is much larger than the convective scale, *i.e.*, the scale of the

ensemble variability, a significant difference is unlikely when applying the methods to individual ensemble members, *i.e.*, single-time step data.

On applying the method, however, we might be confronted with another issue: Either the data is resolving the individual convective features, then we argued that the filter-scale being much larger comes at our rescue. If, however, the data resolution is comparable with the filter size, we "see" the data already filtered. The method then picks up the collective effect visible in the data which open the interpretation to a quasi-ensemble average where the coarse-resolution data has a manifold of corresponding high-resolution representations, *i.e.*, the ensemble members. This is akin to the concept of parametrization.

We thus conclude that the design of both methods, in fact, renders the distinction between "ensemble-mean" and "single-time step" irrelevant at a statistical level.

Nevertheless, with his concern, the reviewer sheds light on the perception of the methods and how they are interpreted. We therefore address this issue by modifying the text at lines 37ff in the following way: "Asymmetries of the flow fields originate from either asymmetries in the environmental conditions, *i.e.*, large-scale wind shear and ocean surface temperature (Wadler et al., 2021), or as a manifestation of stochastic small-scale convective processes. While the former leads to pronounced vertical tilt of the vortex center the latter is subject to the preconditioning by the environment."

(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)

Thanks to the reviewer for pointing out this inconsistency in the line of reasoning. While it is true that, with the given data, it was not possible to quantify the added value of a tilted centerline, we show that the tilt emerges as a feature of the flow structure, even if it is not a design feature of the method, as it is for the DRI. Because of the limitations discussed in the paper and below (item 3), the relevance of the tilt-induced dynamics remains limited as well.

The notion of the tilt enters as part of both the symmetric and asymmetric diabatic transfer coefficients G_0 and G_1. For the former, the tilt is relevant insofar as it acknowledges that the centerline coordinates vary with height; for the latter, the tilt directly enters as the driver for the potential-temperature perturbations Θ'_1. In Figure 8, we observe only marginal correlations between the dynamic evolution of the storm (indicated by $v_{\max}$) and the asymmetric diabatic transfer coefficient G_1, yet it gives rise to further investigations with data exhibiting more prominent tilts.

We thus account for the comment by the following changes:

L.84: "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 existence of the tilted centerline. The study will focus on the combined importance of diabatic and inertial energy transfers."

L405: "For DRI we observe that the structure organize along the tilted centerline, although the method did not explicitly take a notion of tilt into account."

(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 δu 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.*

We agree with the reviewer point of view that the ERA5 data does not contain enough added value. Thus, we dropped references to the ERA5 dataset and its analysis from the manuscript.

(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.

This reviewer raises a number of points which we address accordingly:

- We rephrase the sentence starting on line 322 (now l. 283): "Through fig.6, we find a monotonous correlation between intensity and diabatic transfer of $\rho = 0.85$, at least below the saturation of intensity."
- We rephrased the statement on L415ff to: "We conclude that the presented analysis opens a potential route towards understanding and predicting TC intensity changes on the basis of energy budgets. For the presented test cases we observe evidence that the indicator signal correlates with intensity changes."
- Added l.226 (updated numbering): "Particularly at lower intensities ($v_{\max} < 50\,\mathrm{m\,s^{-1}}$), the rank correlation appears rather strong."
- We replaced the figure 5 (originally 7) by a scatter plot of G vs. $v_{\max}$ since the original one only accounted for G_0. This change is reflecting the discussion around l. 284.
- "Similar to the time evolution of Fourier-mode-0 diabatic transfer, the Fourer-1 mode is correlated to the time evolution of the intensity of the storm. Here, and in more detail in Dörffel, Papke, et al. (2021), we argue that it is the perturbation of potential temperature that opens a pathway for diabatic heating to couple to the storm's energy balance (cf. eq. (6)). The potential temperature perturbations are the result of two processes, namely the balanced primary circulation developing a warm core, *i.e.*, the symmetric mode, and the tilt-induced dipolar modulation (Dörffel, Papke, et al., 2021; Dörffel, 2022). Diabatic heating can couple to these Fourier-1 modulations and, depending on the relative orientation of dipolar potential temperature perturbation and (effective) diabatic heating, it can either act to amplify or to attenuate the storm. In case of the storm presented here, we find that throughout most of the storm's development lower to mid-level Fourier-mode-1 kinetic energy transfer is mostly neutral with episodes of negative impact. At least one of these incidents (t=130 to 140 h) is temporally adjacent to the major phase of attenuation."
- The discussion related to lines 357 and 366 has been discarded with dropping the ERA5 data from the manuscript.
- We removed the explicit p-value discussion and revised the statistical interpretation to avoid overstating significance in the presence of serially correlated time series, while keeping the main methodological conclusions.

Here, we address the **specific comments** made by the referee:

(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.

We have added the following paragraph to the introduction of the method in sec. 2.3: "The definition of the DRI start with the idea that for each point in space there is a measure of radially directed kinetic energy fluxes at the surface of a sphere with radius l. If the test function would be a Heaviside step function in the radial coordinate, this definition would coincide with the definition of the divergence of kinetic energy as $l \to 0$. If we weaken the definition by using a differentiable test function, we end up with the original Duchon-Robert index, only the measure over the surface of the l-sphere turns into a weighted integration with most of the weight located close to the radius of the original sphere. Hence, there is a connection between the divergence of kinetic energy and the DRI measured for a scale l in the sense of a limiting process $l \to 0$. The advantage of the construction of the DRI is two-fold: For once, it measures the scale-specific convergence of kinetic energy while it filters out larger- or smaller-scale contributions. Further, with the smooth definition of the test function, it is a tangible object to implement on space-discrete data where a jump-discontinuity would not be representable. With that being said, kinetic energy is, among others, subject to advective transport, thus mass fluxes carry kinetic energy, and particularly converging mass fluxes of laminar flows lead to signals in the DRI. We need to highlight, that the original construction of the DRI is three-dimensional and based on the assumption of incompressibility (Duchon and Robert, 2000). In that case, the pure advective transport would not lead to convergence of kinetic energy due to mass fluxes, as the mass fluxes do not converge at leading due the anelastic constraint (Päschke et al., 2012). In our construction, however, we explicitly brake the divergence constraint by deploying the DRI in the two horizontal dimensions. Convergence and divergence of two-dimensional mass fluxes are explicitly possible and thus measured by the DRI. In fact, with the resolution provided for the dataset, 4 km grid resolution are not sufficient to measure turbulence at the 30 km scale of the DRI. Hence, the flow of the test case is considered laminar."

(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.*

Considering the fluid dynamical mechanism that has been derived in Päschke et al. (2012) and Dörffel, Papke, et al. (2021), it is the potential temperature and (dry) APE being responsible for the intensity changes. While equivalent potential temperature and moist APE (CAPE) are well established concepts in the field of cloud convection, here, the explicit distinction between the moist processes and dry fluid dynamics is what gives rise to the dissection of the interaction of both (Dörffel, 2022).

To account for the reviewer's comment, we added "Although widely adopted in the context of cloud convection, we explicitly refrain from applying the concepts of equivalent potential temperature and moist APE in our description of the process. These concepts intertwine (dry) fluid dynamics and moist thermodynamics thereby obscuring the interaction mechanism we are interested in. By using potential temperature and (dry) APE we give rise to the analysis framework to be discusses in this study." to line 154ff.

(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?*

Thank you for pointing out this issue. In fact, DRIU coincides with $\mathrm{m}^2/\mathrm{s}^3$. We replaced all occurrences of DRIU accordingly, and also fixed the units of all figures containing the diabatic transfer indices G to $\mathrm{kgm}^2/\mathrm{s}^3$. Lastly, we fixed the data normalization in fig. 2 to be consistent with fig. 1.

(4) *L90: "without relying on restrictive assumptions inherent to spectral analyses" Which assumptions?*

We agree that this statement is misleading and therefore, we removed it from the manuscript.

(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?*

We fixed the symbolic representation of the spatial coordinate triple to x in Eq. 4.

(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?*

We acknowledge the confusion of this explanation, in particular the ambiguous use of curvature and tilt, and thus added the following passage to the manuscript: "The approach of Mikula et al. (2022) defines the centerline as a cluster mean of centerlines that are computed based on a parallel-coordinates approach estimating a multi-parametric definition of the centerline. Outliers of the cluster typically exhibit strongly oscillatory centerlines and unphysically large local tilts, whereas the cluster members are more regular. In that sense, the local tilt is part of the clustering criteria and the regular cluster consists of centerlines with physical tilt amplitudes.''

(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.*

Thank you for highlighting this issue. We dropped the sentence as it turns

out to be not necessary in this context.

(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.*

We changed the statement to "They restrict to structures symmetric around a vertical centerline and azimuthally average the data. Effectively, they are considering an axisymmetric storm."

(9) *L46: Unclear what "fluid structural properties" refers to.*

We changed the sentence to "In that theory, the tilt controls how APE is generated and converted to KE during phases of intensification or weakening based on the geometric properties of the fluid flow.".

(10) *L51: Unclear what "scales of asymmetry" means.*

We address the notion of asymmetry as deployed in Bhalachandran et al. (2020). The authors categorize Fourier-modes into symmetric, large and small-scale asymmetries according to their (increasing) wavenumber. We adopted that nomenclature here, but acknowledge the confusion that arises from the missing explanation. To resolve the issue, we added more explanation to the original text passage: "It was found that the dominant mechanisms for rapid intensity changes are not only the baroclinic conversion from APE to KE for each wavenumber individually, but also the transfers of KE among the symmetric, the large and the small-scale components in the azimuthal wave spectrum."

(11) *L129: Up to the authors: if it's simple, the method could be described in short.*

To keep the manuscript concise and to focus on the main message, we refrain from adding further details of the method.

(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 11.5 km?

Regarding the boundary layer, we modified the paragraph of the highlighted line to: "The DRI analysis, in fact, allows us to trace the boundary layer, since positive values correspond to converging air masses that carry kinetic

energy. This is in congruence with the recent boundary layer theory of Dörffel, Doppler, et al. (2026) for tropical cyclones introduced three distinct flow regimes: (i) the friction-dominated lowest layer of about 1 km height; (ii) the convection-controlling layer of about 3 km height; and (iii) the free tropospheric bulk flow reaching to 10 km height. There is a transition from converging transport of energy and momentum to a diverging flow of the same between the convection controlling layer (ii) and the free tropospheric bulk flow (iii). We use this property to define the boundary layer based on the DRI by masking for positive values in the lower 1.5 km. This masking allows us to capture the temporal evolution of the boundary layer top, *i.e.*, the height where DRI changes its sign."

(13) *Figure 4 is very cool.*

Thank you very much!

(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-4 km phenomenon, whereas the plot shows 30 km convergence/divergence. Presumably a 30 km phenomenon is well resolved at 4 km, 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.

Thanks for the critical advice to restructure the interpretation of fig. 3. We changed the criticized text passage to "The three-dimensional visualizations of fig. 4 show a positive structure in the upper mid-troposphere that has a correspondence in fig. 5 where the DRI in that region (7–11 km) is averaged and plotted by the red curve. It remains open what might be the physical origin of this signal. Typically, numerical models parameterize entrainment and detrainment as part of the cloud-convection scheme. In the spirit of the interpretation of the two-dimensional DRI we use here (cf. end of sec. 2.3), what we see might be related to the upscaled residual flow of competing

entraining and detraining air masses (Dörffel, Doppler, et al., 2026; Montgomery and Smith, 2017; Smith and Montgomery, 2024; Gregory, 2002) as parameterized by CM1 and "seen" at the filter scale of 30 km. For the secondary circulation to establish we need convergence (enhanced through entrainment) in the lower parts of the troposphere and divergence in the upper parts (enhanced through detrainment). Entrainment in the upper troposphere might counteract and partially cancel this circulation and thus lead to stagnation or attenuation. Further analysis is required to draw more robust conclusions which we relay to future work."

We have also changed the sentence one line 286 to "Fig. 4 reveals the complexity of the three-dimensional structure of the DRI values taken around the storm at six given time steps." added this sentence to the caption of fig. 4: "It is being noted, that the actual rendering of the three-dimensional figures apply a colormap with amplitude-dependent transparency ("alpha")."

(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.*

We account for the comment by updating the sentence starting on l. 146: "G_0 and G_1 represent the symmetric and asymmetric source terms of kinetic energy, *i.e.*, the generation of kinetic energy accounting for either the axisymmetric components w_0 and Θ'_0 or the higher-order asymmetries w_1 and Θ'_1."

Thank you as well for suggesting **technical corrections** which we implemented accordingly:

(1) *L16: Please define RI and RW.*

Now, the sentence reads: "Mechanisms that could explain rapid intensification (RI) and rapid weakening (RW), *i.e.*, changes in 10 m-wind speed of more than 30 kn ($\tilde{}15\,\mathrm{m\,s^{-1}}$) within 24 h (Wood and Ritchie, 2015; Kaplan and DeMaria, 2003; Kaplan, DeMaria, and Knaff, 2010), can be studied through energetic pathways."

(2) *L29:* `\citep`

The citations is changed accordingly.

(3) *L191: "stems" – right word?*

Obsolete after dropping the ERA5 dataset from the manuscript.

(4) *Page 9 – Can the units be converted to SI (or SI reported alongside) for comparison with the idealized case? Particularly wind speeds.*

All the units have been converted to SI units (with winds in $\mathrm{km\,s^{-1}}$ printed alongside).

(5) *L344: did you mean quantitative? also "a" missing on L345*

The sentence is now: "For a more quantitative statement, however, we need to further analyze suitable cases that exhibit the necessary features." We do not see a missing 'a', however.

Publications to support our reply to Reviewer 1.

Alvey III, George R., Jonathan Zawislak, and Edward Zipser (Nov. 2015). "Precipitation Properties Observed during Tropical Cyclone Intensity Change". In: *Monthly Weather Review* 143.11, pp. 4476–4492. ISSN: 1520-0493, 0027-0644. DOI: 10.1175/MWR-D-15-0065.1.

Bhalachandran, Saiprasanth et al. (Jan. 2020). "Characterizing the Energetics of Vortex-Scale and Sub-Vortex-Scale Asymmetries during Tropical Cyclone Rapid Intensity Changes". EN. In: *Journal of the Atmospheric Sciences* 77.1. Publisher: American Meteorological Society Section: Journal of the Atmospheric Sciences, pp. 315–336. ISSN: 0022-4928, 1520-0469. DOI: 10.1175/JAS-D-19-0067.1. URL: https://journals.ametsoc.org/view/journals/atsc/77/1/jas-d-19-0067.1.xml (visited on 09/15/2021).

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Dörffel, Tom (2022). "Moisture-Induced Dynamics of Tilted Tropical Cyclones". eng. Accepted: 2023-10-30T13:27:48Z. PhD thesis. Berlin: Freie Universität Berlin. URL: http://dx.doi.org/10.17169/refubium-41065 (visited on 11/15/2023).

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Gregory, D (2002). "The Mass-Flux Approach to the Parametrization of Deep Convection March 1997". In.

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Mikula, Natalia et al. (2022). "An Interactive Approach for Identifying Structure Definitions". In: *Eurographics Conference on Visualization*. Vol. 41. 3. Wiley & Sons Ltd.

Montgomery, Michael T. and Roger K. Smith (Jan. 2017). "Recent Developments in the Fluid Dynamics of Tropical Cyclones". In: *Annual Review of Fluid Mechanics* 49.1, pp. 541–574. ISSN: 0066-4189, 1545-4479. DOI: 10.1146/annurev-fluid-010816-060022.

Päschke, Eileen et al. (June 2012). "Motion and structure of atmospheric mesoscale baroclinic vortices: dry air and weak environmental shear". en. In: *Journal of Fluid Mechanics* 701. Publisher: Cambridge University Press, pp. 137–170. ISSN: 1469-7645, 0022-1120. DOI: 10.1017/jfm.2012.144. URL: https://www.cambridge.org/core/journals/journal-of-fluid-mechanics/article/motion-and-structure-of-atmospheric-mesoscale-baroclinic-vortices-dry-air-and-weak-environmental-shear/AD673C4004B6520D455E3ACE507976B9 (visited on 09/15/2021).

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