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 2:
We thank the reviewer for their very valuable comments and suggestions, all of which have been addressed 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.

The reviewer made the following **broader comment**:

(1) *This reviewer has some fundamental problems with the approach adopted here. Many of the flaws are common to virtually all attempts at modelling and diagnosing tropical cyclones. The arbitrary selection of scales (scale analysis) fails in the face of scaling and scale invariance. The treatment of the kinetic energy as a small fraction of the internal and gravitational energies needs to be recognised and made explicit. The role of radiative transfer is inadequately represented. The identification of spectral wavenumbers with physical waves is incorrect in the absence of driving and restoring forces for each.*

I sympathise with the difficulty of the subject, and the amount of work involved. If this paper is to be published, it needs to make major revisions that at least recognise the existence of the shortcomings listed.

The authors cannot entirely follow the critique expressed with this comment. While the exact value of the filter scale is somewhat arbitrary, it is motivated as being in the range below the vortex-core scale which is motivated by asymptotic means (Päschke et al., 2012; Dörffel et al., 2021). To strengthen the point, we changed the sentence on line 244 to "The 30 km scale is chosen on the basis of the vortex core size as being in the range of the vortex-core scale (Päschke et al., 2012; Dörffel et al., 2021)." We do not see a need to explicitly recognizing the obvious fact that kinetic energy is small compared to the atmosphere's potential and internal energy. In this study we address the *available* potential energy which of the order of kinetic energy. With the representation of radiative transfer in CM1, we make use of the model's default implementation which is proven to be robust in a plethora of studies. There is no identification of spectral wavenumbers with physical waves. If a passage may be interpretable as such, we kindly ask the reviewer to point us to the same.

Comments by page and line:

- *Line 16:*
  Define RI and RW. Guessing Rapid Intensification and Rapid Weakening; more importantly what is "rapid"?

RI and RW stand for rapid intensification and rapid weakening. They are defined as rapid intensity changes of ≥ 30 knots of the maximum 1-minute wind at 10 m height over a period of 24 hours. According to reviewer 1's comment, we changed the sentence on l. 16 to "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.".

- *Lines 19–29:*
  Lagrangian trajectories calculated from a model analysis or assimilation are really Lagrangian sampling of an Eulerian field. Note that the energy of the wind field is a small fraction of the internal energy and the gravitational energy [1,2].

  We will add (addition: blue) the following to the original sentence from the manuscript: "Pauluis and Zhang (2017) analyzed the change of equivalent potential energy along Lagrangian fluid trajectories that were not observed, but reconstructed from winds in an underlying Eulerian analysis."

- *Lines 30–54:*
  The inherent symmetry of air is that of continuous translation. That symmetry is broken on all scales from the smallest (the mean free path) to the largest (a great circle) in the atmosphere by the persistence of molecular velocity after collision [3]. There is unbroken scale invariance over the whole range [4]. It follows that the selection of particular scales is incorrect [5]. The identification of Fourier wavenumbers with physical waves without specified forcing and restoring forces has been questioned [6].

  We addressed this issue already in the broader comment.

- *Lines 55–83:*
  This discussion does not acknowledge the scale invariant reality and the associated nonconvergence of variance. The selection of particular scales is therefore arbitrary. This view impacts lines 99–103. It has implications for the whole methodology as well, in particular the identification of Fourier wavenumbers with actual waves without specification of the driving and restoring forces for the latter.

  As we do not consider wavenumbers to be actual waves, we consider this comment obsolete.

- *Line 209:*
  Atmospheric radiation should be represented by something better than the Rotunno and Emanuel parametrization. See [7].

  We agree that the among others, the radiation scheme is simplified in our idealized tropical cyclone simulations and we therefore added "Of particular interest is the question of how universal our findings apply to TCs and how

much they depend on the specific choice of physical parametrizations of sub-grid-scale turbulence, cloud convection, and radiation, among others." to line 392. However, our main objective is to investigate and compare energy transfers for two idealized, but distinct cases of background shear and no background shear and how this affects RI and RW. Please note, the relaxation formulation of Rotunno and Emanuel (1987) is part of the standard, pre-configured CM1 3d hurricane test case.

- *Lines 243–247:*
  Hardly surprising given that reanalyses make scale assumptions too. They have systematic shortcomings regarding their energy content [8].

  In the light of reviewer 1's critique regarding the analysis of ERA5 reanalysis data, we decided to drop the analysis from the updated version of the manuscript. Thus, the comment raised here renders obsolete.

- *Lines 378–383:*
  This reviewer regards the DRI as based on faulty assumptions, per previous comments.

  With the main purpose of this paper to assess the ability of the DRI to signal intensity (changes), we propose the method as a tool rather than an exact measure of energy transition.

- *Lines 398–404:*
  What drives the deep convection? The Gibbs free energy of water evaporation from the surface. There are longitudinal gradients of winds, temperature and pressure: angular momentum is not conserved in those circumstances.

  There is no doubt about this statement risen in the paper.

- *Lines 416–423:*
  Any "more sophisticated framework" needs to recognise scale invariance and the energy shortfall in reanalyses and climate models. Selecting scales is arbitrary and leads to error. Both radiative and thermodynamic approaches are inadequate.

  It is true that the proposed methods are not able to quantify energy transitions exactly as both of them only take the kinetic energy compartment into account. We argue, however, that this is sufficient to detect intensity changes.

REFERENCES (given by Reviewer 2)

[1] Dutton JA. The Ceaseless Wind: An Introduction to the Theory of Atmospheric Motion, 2nd Ed.; Dover Publications, Mineola, New York, 1986; Chapter 14.

[2] Gill AE. Atmospheric-Ocean Dynamics, Academic Press, San Diego; 1982; page 75.
[3] Tuck AF. 2022. Scaling up: Molecular to meteorological via symmetry breaking and statistical multifractality. Meteorology 1: 4–28.
[4] Lovejoy S. 2023. Scaling, dynamical regions and stratification: How long does weather last? How big is a cloud? Nonlinear Process. Geophys. 30: 311-374.
[5] Lovejoy S, Tuck AF, Schertzer D, Hovde SJ. 2009. Reinterpreting aircraft measurements in anisotropic scaling turbulence. Atmos. Chem. Phys. 9: 5007-5025.
[6] Tuck, A.F. The generation of wind velocity via scale invariant Gibbs free energy: Turbulence drives the general circulation. Entropy 27: 740.
[7] Varotsos CA, Lovejoy S, Sarlis NV, Tzanis CG, Efstathiou MV. 2015. On the scaling of the solar incident flux. Atmos. Chem. Phys. 15: 7301–7306.
[8] Tuck, A.F. 2025. Why the real atmosphere has more energy than climate models: Implications for ground-based telescopes. Atmosphere 16: 56.

Publications to support our reply to Reviewer 2.

Dörffel, Tom et al. (2021). "Dynamics of tilted atmospheric vortices under asymmetric diabatic heating". In: *Theoretical and Computational Fluid Dynamics* 35.6, pp. 831–873.

Kaplan, John and Mark DeMaria (Dec. 2003). "Large-Scale Characteristics of Rapidly Intensifying Tropical Cyclones in the North Atlantic Basin". In: *Weather and Forecasting* 18.6, pp. 1093–1108. ISSN: 1520-0434, 0882-8156. DOI: 10.1175/1520-0434(2003)018<1093:LCORIT>2.0.CO;2. (Visited on 09/29/2026).

Kaplan, John, Mark DeMaria, and John A. Knaff (Feb. 2010). "A Revised Tropical Cyclone Rapid Intensification Index for the Atlantic and Eastern North Pacific Basins". In: *Weather and Forecasting* 25.1, pp. 220–241. ISSN: 1520-0434, 0882-8156. DOI: 10.1175/2009WAF2222280.1. (Visited on 09/29/2026).

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).

Wood, Kimberly M. and Elizabeth A. Ritchie (2015). "A Definition for Rapid Weakening of North Atlantic and Eastern North Pacific Tropical Cyclones". In: *Geophysical Research Letters* 42.22, pp. 10, 091–10, 097. ISSN: 1944-8007. DOI: 10.1002/2015GL066697. (Visited on 09/29/2026).