Review of "A New Simple and Accurate Measure of Baroclinicity"

General comments

The authors derive a new expression of baroclinicity based on dry specific entropy, which is equal to the standard Eady growth rate under quasi-geostrophy. They justify the use of this new measure for the North Atlantic storm track by the means of a correlation analysis. The central result of the manuscript is then that the variability of storm track-mean baroclinicity derived from dry entropy is largely governed by the mean dry entropy at the Northern boundary of the storm tracks.

I enjoyed reading this manuscript. It is concise, well written, and I think the idea that storm track baroclinicity can be reduced to dry entropy at the Northern boundary is genuinely interesting. However, the manuscript is at times too brief and fails to embed the main result in the existing literature, while also not providing their own use case. In particular, I'm concerned about the following:

(1) I think the authors need to better motivate their focus on area-mean baroclinicity. A large part of the literature addresses the problem of why this baroclinicity is so localized given the fact that it is fundamentally maintained by the differential heating on planetary scales. Many studies have shown that diabatic effects are essential for maintaining baroclinicity exactly within the narrow latitudinal band of the storm tracks (Hoskins and Valdes, 1990; Hotta and Nakamura, 2011; Papritz and Spengler, 2015; Weijenborg and Spengler, 2020), some of which the authors have used to motivate their simplified expression. But the area-mean baroclinicity cannot answer that same question, because it essentially reflects this large-scale differential heating. So, what question exactly are the authors interested in answering?

(2) I think the statistical analysis could be more thorough. At the moment, several independent analyses are used to construct the result that only the poleward entropy is relevant for area-mean baroclinicity. Can you think of a way to quantify the role of the poleward and equatorward boundaries in the variance of the mean baroclinicity? For instance, the expansion of the Hadley cell in a warmer climate has been related to shifts of the midlatitude baroclinicity and storm tracks (Shaw et al, 2016), which would be expressed in a change in the equatorward entropy. How would this be reflected in your framework?

(3) I have some doubts about the utility of area-mean baroclinicity. Baroclinicity is locally meaningful, and it has a cousin, the APE, which is meaningful in an area-

integrated sense. Fixing the entropy at the two latitudinal boundaries does not, however, fix the APE content of such a state, because the APE is quadratic in the deviation from the reference state. Imagine, in a given domain, a very localized front in an otherwise barotropic environment vs. a more softly sloping front across the whole domain. You might end up with the same mean baroclinicity, but the steep front will have much more APE. So, the two cases with the same mean baroclinicity could end up with very different storms. This concern could be addressed by either (1) more conceptual framing around your result, that explains what exactly the poleward entropy and mean baroclinicity can tell us, or (2) by adding additional analysis to explicitly show the utility of your framework for solving a certain problem.

Specific comments

L40: For someone not familiar with Pinto et al. (2014) this sentence is hard to understand. It would be good to add one more sentence explaining the thinking behind Pinto's paper and then contrast it to the diabatic maintenance theories.

L43-45: This part doesn't convince me. The biases in forecasting and in the climate context haven been shown to heavily depend on the representation of subgrid-scale processes, such as latent heating (e.g., Schemm, 2023; Pickl, 2023), yet you suggest to resolve these issues by reformulating baroclinicity as *dry* entropy.

L52: Please indicate the vertical resolution precisely. Also, could you motivate here why you choose this exact vertical range? Over the Gulf Stream within cold air outbreaks, this range will often lie within the turbulent boundary layer, while in a warm sector of cyclones it will be in the free troposphere. How does this matter for your analysis?

L75: Atmospheric scientists generally like to use potential temperature, which is also why Papritz and Spengler (2015) express baroclinicity as the slope of potential temperature surfaces. Now, dry entropy is also constant on surfaces of potential temperature, but their gradients are not equal. They scale with a logarithm. How do you think this matters when you compare your entropy framework to the isentropic slope representation of baroclinicity? I'm just curious about it, no need to include this in the manuscript if you think this is irrelevant to this section.

L84: Reanalysis data is not real world data (i.e. measurements), please rephrase accordingly.

L150-154: If you compute the variance equally for potential temperature, do you find the same latitude dependence, or does the logarithm inflate the variance of entropy? Because d_s = c * d_theta/theta. So the same deviation in theta is divided by a smaller theta to the

North, so the resulting d_s will be larger. In that sense, your conclusion would follow simply from how d_theta and d_entropy relate.

L154-156: I'm not convinced by this statement and Figure 3 alone. I agree that s_north has a stronger seasonal cycle in absolute terms than s_south, but the variance of s_north is also a lot larger. So the effect on the integral's variability might be larger in absolute terms for s_north, but I don't think you can conclude that s_south is simply noise. You could quantify this easily by performing a multiple linear regression and/or checking the correlation of s_north and s_south.

L160-164: I'm not sure I follow this argument. How can you conclude from the regression analysis that s_south is not relevant if it was not included in the regression in the first place?

L164-166: I think the relation of s_north and s_south and their relative importance for the mean baroclinicity should be quantified more rigorously.

L215-219: Similar results have been found for APE, where APE accumulation on the poleward side of the storm tracks governs storm track variability (Federer et al. 2026). I suggest discussing here whether your results confirm or extend this study.

Technical comments

L12: Please spell out North Atlantic in the abstract

L33: …to analyse the…

L175: North-America -> North America

References

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