A new simple and accurate measure of baroclinicity – Response to Reviewers

Response to Reviewer 1

We would first like to thank the reviewer for giving their time and effort to review this manuscript, the positive and constructive comments are very useful and well appreciated. These comments have led to several improvements in the revised manuscript.

Biddiscombe et al. suggest an entropy-based measure of baroclinicity instead of using vertical wind shear. The manuscript is nicely structured. The analysis comparing vertical wind shear with the horizontal entropy gradient is interesting. It is also useful to see the sensitivity test comparing fixed and varying stratification (N). In the second half of the paper, the authors show that sector-zonally averaged entropies at a wide range of latitudes poleward of 30°N capture the variability in the mean meridional entropy gradient between 30°N and the respective latitude over the longitude range 105°W–15°E.

Beyond this, I am somewhat unsure what exactly I am left with after reading the manuscript. The authors state that they are working on the implications of their results for the North Atlantic storm track. The manuscript would be more complete if some of that analysis were included. At present, it is somewhat short, and given that it is not uncommon to use horizontal temperature gradients to represent baroclinicity, I think it lacks some novelty. I would therefore strongly suggest including additional analysis to answer the very interesting question that the authors raise: what are the physical implications of box-averaged baroclinicity being well represented by entropy at the northern box latitude alone?

Thank you for these comments, in the revised manuscript we have made the objective of the paper clearer by including an additional section outlining how this work is applied to the storm track, as detailed in response to the comments below.

Major

As mentioned above, I strongly suggest including some analysis discussing the physical implications of box-averaged baroclinicity being well represented by entropy at a northern latitude alone.
We agree that an example of the application of representing baroclinicity with entropy at a single latitude line (σ_n) would help to clarify what the implications of our work are. A new section (section 4 in revised manuscript) is now included detailing how our new way of approximating area mean baroclinicity allows for a much simplified and straightforwardly interpreted budget for baroclinicity. While we discuss the budget to demonstrate the purpose of the work, we intentionally do not include detailed analysis of the budget itself since this requires a much larger discussion than is appropriate for this work. In the revised manuscript we explicitly state that our work on the entropy budget is in the process of being written as a paper.

The following comments address how the manuscript discusses baroclinicity, eddies, and APE. Some of the concerns are linguistic, while others are physical. Given the topic of the manuscript, I will raise these as major comments, although they are relatively minor.

- 14 and onwards. The authors begin by discussing cyclones, then go on to discuss eddies as both a noun, implying Lagrangian thinking, and an adjective, implying Eulerian thinking. This is likely to be confusing to a non-expert reader. I therefore suggest clarifying how cyclones and eddies are related, as well as specifying the spatio-temporal scales that the eddies represent.
  The reviewer is correct to point out that the word "eddy" generally refers to both the Lagrangian feature as well as the Eulerian derivative of some background state. The paper uses both, depending on context. In several cases both meanings would be appropriate. One issue is that an eddy in the Lagrangian sense is not synonymous with a cyclone; a distinct anticyclone is also an eddy. We have tried to be as careful as possible to distinguish them in the revised text.

- 17-19. The phrase 'in the absence of eddies' implies that baroclinicity and APE are associated with some kind of mean state, since defining eddies requires defining a mean state. This should be mentioned in the Introduction. At present, it reads as though baroclinicity and APE are associated with unfiltered states. If that is the authors' intention, I suggest that the Introduction discusses cyclones rather than eddies.
  These lines have been rephrased to avoid confusion. "Eddies" in this case referred to cyclones and other synoptic-timescale features which during their baroclinic development act to weaken horizontal temperature gradients – this is stated more explicitly in the revised manuscript.

- I think this statement is only correct if the authors are referring to the radiative cooling of continental air as constructing baroclinicity (Schnyder and Riboldi 2026). However, the construction of mean-state baroclinicity over the Gulf Stream extension, for example, involves the destruction of temperature anomalies (Swanson and Pierrehumbert 1997): an eddy transport of anomalously cold air is heated over the ocean, which damps the temperature anomalies and restores the mean-state baroclinicity. I think the manuscript would benefit from discussing a broader set of literature, including the two citations above and other papers on cold-air outbreaks. Again, clarifying whether baroclinicity refers to a mean-state quantity would help.
  Baroclinicity is throughout the paper used in the same way as the standard papers that we refer to: it is the observed temperature gradients in the upstream part of the storm track, effectively the Eady growth rate. While the paper does not focus on the mechanisms that set the baroclinicity, in the revised manuscript we included a brief discussion of these processes. The main focus of the paper, though is on introducing a new measure of baroclinicity that is more amenable to defining a dynamical model for the temporal variability of baroclinicity.

Minor

- 25-33. After having argued for the relevance of baroclinicity in the two previous paragraphs, the authors then switch, without clear motivation, to discussing APE. I suggest either dropping APE completely or clearly motivating why both quantities are needed to understand storm tracks, how they are similar or different, and why they are both relevant to this study.
  In this context we are using APE in an informal sense to indicate stronger temperature gradients. We agree this could be made clearer – in the revised manuscript this paragraph is mostly removed as it is not essential for our explanation of baroclinicity. Where the discussion of APE is kept, we instead refer to the temperature gradients it is associated with.

- 33-34. This is not necessarily true, but depends on the reference state. Since the authors cite Federer et al. (2024), I assume that they are referring here to local available potential energy (Novak and Tailleux 2018). If density as a function of pressure and temperature is close to the reference density as a function of pressure, APE will be small or zero regardless of the steepness of the isentropes. See Fig. 2 in Federer et al. (2024), where the reference state is computed over the channel model and the reference density as a function of pressure therefore closely aligns with the air in the baroclinic zone, since there is roughly an equal portion of cold and warm air in the model domain. In Fig. 2 of Federer et al. (2024), APE is zero where the isentropes are steep.
  We agree the link between APE and isentropic slopes is not as simple as we make it seem. Again, we are discussing APE informally as being co-located with steep isentropes which, as you point out, is not true in all circumstances. As previous, in the revised manuscript we have removed the problematic discussion of APE and are more explicit in saying "horizontal temperature gradients" when that is what we mean.

- 34-36. Interesting! What scales of baroclinicity would that be then, exactly? And how do you justify using high temporal resolution but spatial averaged baroclinicity?
  Thank for pointing out that we could be more specific. We are concerned with baroclinicity as it relates to the growth of baroclinic waves and the development of instabilities, where the dominant growth rate occurs for zonal wavenumbers 6-8 which can comfortably be described as large scale (Bretherton, 1966; Valdes and Hoskins, 1988). The conversion of strengthened temperature gradients to enhanced eddy activity occurs where meridional heat fluxes are strong, which is not a local process and is not impacted by local variations in baroclinicity, rather area mean baroclinicity over a correspondingly large region is the relevant scale. As suggested, in the revised manuscript we are more specific about the length scales for baroclinicity we are interested in.
  We use a temporal resolution of two hours, which is similar to other studies considering baroclinicity, perhaps a little finer. Our expression for baroclinicity is calculated with instantaneous data, so it is valid for all timescales and is not impacted by using spatially averaged baroclinicity. In a new figure (Fig. 5) we show that despite spatial averaging there is still high frequency variability in area-mean baroclinicity.

- 76. It would be useful to mention already here how N is treated: whether it is allowed to vary or held constant. Since the latter is quite common, it is not obvious that the former is used.
  We use this expression with both varying and constant N at different points. In the revised manuscript we state this and specify how we are treating N each time this expression is used.

- 84. Is the centre expression in (5) any different from the rightmost expression in (5) computed on the data? If not, why not stick with temperature gradients rather than entropy gradients?
  The question is: how would you numerically evaluate the centre expression. We would evaluate it as the finite difference $\Delta \ln T / \Delta y$ which is equivalent to how the rightmost expression would be evaluated. In the centre expression the same temperature gradient could lead to different growth rates due to the $1/T$ outside of the derivative. This is beneficially not true for the rightmost expression, as the same entropy gradient would correspond to the same growth rate regardless of temperature, the nuance here being that this temperature dependence is instead found in the entropy value itself (Eq. 7).

- 85. Please specify in the text, not only in the figure label, at which pressure level baroclinicity is computed and how N is treated.
  In the revised manuscript we state this in the text as well.

- 175. CAOs are often thought of as eddy involving processes? Ref. introduction and earlier comments.
  We would consider cold air outbreaks as eddy involving processes in the sense that typically an estab-

lished region of cold air over North America is advected south rapidly by weather systems. Though not stated in the original manuscript, in the additional section added to the revised manuscript we describe eddy and mean timescales for our purposes as being shorter than 10 days and 10 days and longer respectively.

- Figure 4. Have you tried holding the northern boundary fixed and varying the latitude of the southern boundary? One could potentially make a correlation matrix with northern boundary latitude along one axis and southern boundary latitude on the other, with colours showing the correlation coefficient. This might highlight the latitude combinations that are critical for representing the respective box-averaged variability, which could perhaps be linked to some physical mechanism.
  We thank the reviewer for this suggestion, we now examine this in much greater detail and have adapted Fig. 4 into something similar to this, making some interesting new conclusions. We find that Eq. 13 does not hold for latitudinally very small regions, and that the exact position of our $\langle s_{\text{south}} \rangle$ sampling to be less important than the position of $\langle s_{\text{north}} \rangle$.

- 218-219. How sensitive is this to the choice of pressure level? The contribution from latent heating e.g. might be more relevant at pressure levels above 700 hPa (300-700 hPa; Fig. 10 in Auestad et al. 2024).
  While these processes may be quantitatively somewhat different at different levels, the development of these cyclones by baroclinic conversion occurs in the lower free troposphere, around the steering level. Any process which has a significant impact on baroclinicity as it pertains to the development of cyclones will also result in a strengthening of the horizontal temperature gradient across the storm track at the steering level, for which 775hPa is an appropriate level.

Response to Reviewer 2

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

We would like to thank the reviewer for giving their time to read this manuscript and for their positive and constructive comments. We are very pleased to hear that the manuscript was of interest and was enjoyable to read.

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:

- 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?
  Thank you for your detailed comment, we agree that we could better justify our use of area-mean baroclinicity over a large region. A similar point was raised by reviewer 1 (see our response to their comment on lines 34-36), and thanks to both their comment and yours we have expanded our justification (lines 33-39 in the revised manuscript) for the use of area-mean baroclinicity.

- 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?
  We have substantially expanded our analysis on this and included a new figure. This is a very interesting question, we are currently working on a budget where we may touch on these questions.

- 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.
Our discussion of APE is informal, as we use it to indicate where isentropic slopes are steep. In the revised manuscript we are more careful with how we discuss APE. While baroclinicity may have local applications, our focus in this work is on baroclinicity as relevant to the development of cyclones based on the theoretical framework of Eady (1949). In this context area-mean baroclinicity is very meaningful, as it provides the inverse timescale for growth of the fastest growing baroclinic waves which are found to be zonal wavenumbers 6-8 (Bretherton, 1966; Valdes and Hoskins, 1988). For the scale of these wavenumbers, local variations in baroclinicity would have little impact to the development of an instability, rather it's the overall presence of high baroclinicity over a whole region which is relevant and described well by area-mean baroclinicity.
As mentioned previously, in the revised manuscript we have included an additional section which better motivates and demonstrates the use of this framework for constructing a budget for baroclinicity.

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.
We agree this is not clear for someone who hasn't read the cited paper. In the revised manuscript a brief description is added, stating that in Pinto et al. "patterns in the eddy-driven jet and Rossby wave-breaking are linked to cyclone clustering".

- L43-45: This part doesn't convince me. The biases in forecasting and in the climate context haven't 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.
We agree with your assessment of the literature, this line was not intended to reference subgrid-scale processes. As discussed, and now clarified in the revised manuscript, we consider the relevance of baroclinicity to the storm track to be found on large scales, not in subgrid-scale processes. We do not disagree with or contradict these papers. Incidentally, the dry entropy budget does not exclude latent heating processes. The dry entropy framework is relevant for baroclinic development.

- 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?
We now make sure to state explicitly the pressure levels we use in our calculations and justify our focus on the lower free troposphere as the vertical region where baroclinicity is dynamically relevant to the development of cyclones. We point out that the height of the boundary layer is a potential issue in this context, as it can easily affect temperature in the lower troposphere. A similar issue was encountered by Marcheggiani and Ambaum (2020) when looking at 850hPa data in a similar context. We have worked to remove this issue by considering a smaller vertical region with 800hPa as its base.

- 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.
The reviewer is correct to point out that this would lead to different results. We concentrate on the Eady growth rate because its value can be directly interpreted dynamically.

- L84: Reanalysis data is not real world data (i.e. measurements), please rephrase accordingly.
This is true, thank you for pointing it out. This is corrected in the revised manuscript.

- 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.
The reviewer is correct, but this effect is small. Typical potential temperatures might be 273K and the variations 10% of that. Overall, this effect is small and does not explain the increased variance in the north compared to the south, which is about twice as big (Fig. 3) while the temperature is not twice as small.

- 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}.
The correlation between $\langle s_{\text{north}} \rangle$ and $\langle s_{\text{south}} \rangle$ is stated in text as -0.15. Indeed, considering both boundaries is more accurate to σ_s (Eq. 7), however what we demonstrate in the work is that area-mean baroclinicity can be very well represented with just $\langle s_{\text{north}} \rangle$. For area-mean baroclinicity calculated with both boundaries (Eq. 12) and just the north (Eq. 13) the correlation is 0.952, which is more than the correlation of 0.910 between σ_u (Eq. 1) and σ_s (Eq. 7) which are equivalent in QG. This does not mean that the magnitude of $\langle s_{\text{south}} \rangle$ is not important, because it clearly determines the strength of the entropy gradient, but its variability is small such that very little accuracy is lost by treating it as a constant. We have discussed the role of $\langle s_{\text{south}} \rangle$ in more detail with a revised version of Fig. 4 and included additional statistical analysis demonstrating the importance of $\langle s_{\text{north}} \rangle$ over $\langle s_{\text{south}} \rangle$ in representing area-mean baroclinicity in this case.

- 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?
That $\langle s_{\text{south}} \rangle$ is not included is why we can conclude this – a correlation of 0.95 between $\langle s_{\text{north}} \rangle$ and σ_G is achieved when only incorporating variability in $\langle s_{\text{north}} \rangle$. In the revised manuscript we have added a discussion of the importance of the southern border, along with further arguments demonstrating $\langle s_{\text{north}} \rangle$ captures the time series accurately, suggesting there is no need to include $\langle s_{\text{south}} \rangle$.

- 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.

We have now expanded the discussion on the choice of latitudes for $\langle s_{\text{north}} \rangle$ and $\langle s_{\text{south}} \rangle$, finding that s_{south} does not explain much of the tendency in area-mean baroclinicity, while s_{north} explains 90% of the variance in σ_G. Fig. 4 is reworked, demonstrating that the latitude at which s_{south} is sampled is of much less influence than $\langle s_{\text{north}} \rangle$. We also include an additional figure that shows this new simplified measure, a linear regression based on $\langle s_{\text{north}} \rangle$, is accurate for eddy and mean timescales without needing to include the variability of $\langle s_{\text{south}} \rangle$.

- 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.
  Thank you for sharing this recent paper, we weren't aware of it. It indeed shares some similarity with our findings, that the local APE budget seems sensitive to processes to the north of the storm track more than the south. This result seems consistent with our work, which we indicate in the revised manuscript.

Technical comments

L12: Please spell out North Atlantic in the abstract
L33: . . . to analyse the. . .
L175: North-America $->$ North America
Thank you for finding these mistakes, they have been corrected.

References

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Papritz, L., & Spengler, T. (2015). Analysis of the slope of isentropic surfaces and its tendencies over the North Atlantic. Quarterly Journal of the Royal Meteorological Society, 141(693), 3226-3238.

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Response to Reviewer 3

We would like to thank the reviewer for taking the time to comment on our work.

General comments

The present study investigates the mechanisms driving the variability of low-level baroclinicity in the North Atlantic storm track region, by reformulating the Eady growth rate using entropy. Based on their simplified expression of the Eady growth rate derived by the authors, the authors show that most of the variability in lower-level baroclinicity averaged within the storm track region can be explained by low-level entropy at the northern edge of the region. Based on the finding, the present study argues that the area-mean baroclinicity of the storm track is mainly determined by processes on its poleward side, rather than internal processes within the storm track.

I find that the manuscript is overall well written and the topic fits well the scope of the journal, leading to the impression that it could be suitable for publication in WCD. However, revisions are required before the manuscript is ready for publication, as specifically outlined below.

Major comments (not necessarily in order of significance)

- It is not sufficiently explained why the use of sigma_s (and entropy gradient) is better than the more conventional definition (sigma_u and temperature gradient/vertical wind shear), which appears to be the key message of this study. I am confused when I come across the authors' argument in which entropy gradient is linked to temperature fields (e.g., L216). Please elaborate on the advantage of using sigma_s more clearly.
  We thank the reviewer for this comment, but we feel we have been explicit about this in the work, baroclinicity is only meaningful in the context of quasi-geostrophic theory in which these two expressions are equivalent. It has an advantage in that it is trivially interpreted, allows us to construct a new simpler expression for area-mean baroclinicity, and can be easily integrated into a budget for baroclinicity, as described in the work. In the revised manuscript we have described the budget and its purposes more clearly; an equivalent budget in which baroclinicity is considered as the vertical wind shear may be possible but would be far less simple. Equations 5 and 6 describe the relation to temperature fields.
- L182: I understand that sigma_s and sigma_u may be different in the real atmosphere and it is related to ageostrophy (e.g., super/sub-geostrophy within anticyclones/cyclones). Nevertheless, it is unclear why sigma_s tends to be larger than sigma_u in the upstream region of the storm track (Fig. 1). Can the authors explain this?
  Our explanation of this is in L84-92, one reader (CC1) pointed out they understood it well. Short of an explanation of gradient wind balance, it isn't clear what more we can do to explain the differences between the two measures.
- I am concerned about the time scale of the variability of area-mean baroclinicity. As seen in Fig. 3 (faint lines), the entropy variability at the northern edge is dominated by synoptic-scale variability. The present study's framework is reasonable, as long as the authors' primary focus is on describing the variability of baroclinicity without separating eddies and their background field. However, the manuscript includes some discussions on the separation, for instance, "baroclinic instabilities are large scale wave instabilities, growing on a baroclinically unstable background" (L123) and "what extent the persistence of horizontal temperature gradients is diabatically forced" (L38-39).
  I understand that it is difficult to distinguish eddies and their background in the area-mean baroclinicity with a synoptic-scale variability. Nevertheless, this limitation deserves further discussion. While

I agree that the conclusion that "processes outside the storm track region can modify the area mean baroclinicity as long as these processes also modify the mean entropy at the north border of the storm track" (L218-219) is supported by the results, the authors might want to further discuss the separation between eddies and their background. Such a discussion is necessary to address whether or not local-scale internal processes within the storm track itself are important for its development.
Thank you for this observation, you are correct that these statements would benefit from a discussion of the validity of eddy and mean separation for σ_n. In the revised manuscript we have included some discussion of this as an additional section and figure, demonstrating that our expression for area mean baroclinicity is accurate on both eddy (shorter than 10 days) and mean (10 days and longer) timescales. We are currently working on a paper for a detailed budget of baroclinicity, where we explicitly make the distinction of eddy and mean flows, but that is not the purpose of this paper.

Other comments

- L34: In my understanding, latent heating acts to maintain baroclinicity in the free troposphere. Please clarify this.
  We agree, this was explicitly stated in L34.

- L35-36: The part, "a physical understanding of baroclinicity and baroclinic instability depends on a larger-scale perspective in which the unstable mode of a baroclinic wave produces strong vorticity anomalies", is not easy to understand. Particularly, the meaning of "larger-scale" is ambiguous. Please revise the part.
  Thank you for this comment, we agree that specifying the scales we mean would help the reader's understanding. In the revised manuscript an additional paragraph is included after this sentence which describes the physical scales we are concerned with.

- L46-47 "this determines synoptic time-scale variability in the storm track, rather than detailed processes within the storm track itself": I understand that the temperature variability at the northern edge of the Norh Atlantic storm track region is important for baroclinicity within the domain. That said, I reckon that it is a bit too much to argue that it "determines" synoptic-scale variability rather than detailed processes within the storm track itself, as such processes modify temperature fields in the vicinity, to influence temperature variability at the northern edge. This also applies to the conclusion (L215-219).
  We agree that perhaps the word "determine" is too strong of a word to use in this context. In the revised manuscript we have used more specific terms.

- L52: Please replace "-105-15E" (and similar ones) with a more conventional notation.
  These have been changed.

- L67: It seems that a minus sign is lacking in the equation.
  Thank you for pointing out this typo.

- L95: wind shear $->$ vertical wind shear?
  This has been amended.

- L137: Eq. (12) assumes that zonal temperature gradient is negligible compared to meridional temperature gradient. This assumption does not generally hold and it would be better to state that it is an assumption.
  Indeed we assume that the mean flow is zonally directed, as is a standard assumption. What is relevant is the gradient in the direction of the eddy heat fluxes, which can be well captured by considering the meridional temperature gradient (Schemm and Rivière, 2019).

- L201: It would be more appropriate to add "area-averaged" before "Eady growth rate". In addition to the discussion of latent heating in the free troposphere, there have been many studies showing that western boundary currents and associated sharp SST gradients also play a role in the maintenance and restoration of low-level baroclinicity (e.g., Brayshaw et al. 2011; Hotta and Nakamura 2011). Please add a discussion of these processes.
  We have added 'area-averaged' to this line. SST gradients indeed help anchor the storm track in position, a line indicating their importance has been included earlier (L110 in revised manuscript) in the justification for our choice of horizontal area.

Miscellaneous

L33: "analyse to" $->$ "analyse"?
L218: these these $->$ these
These have been corrected.

References

Brayshaw, D. J., Hoskins, B., & Blackburn, M. (2011). The basic ingredients of the North Atlantic storm track. Part II: Sea surface temperatures. Journal of the Atmospheric Sciences, 68(8), 1784-1805.
Hotta, D., & Nakamura, H. (2011). On the significance of the sensible heat supply from the ocean in the maintenance of the mean baroclinicity along storm tracks. Journal of Climate, 24(13), 3377-3401.

Response to Community Comment (CC1)

Thank you very much for choosing to take the time to read and comment on our pre-print, these comments have been very helpful in improving the manuscript. We appreciate the attention to detail you have given to our work.

Community comment on “A New Simple And Accurate Measure Of Baroclinicity”

Before starting out, let me mention that I am NOT AN EXPERT on the topic. I am a Master’s student in Atmospheric Sciences. I am still writing this to make a constructive contribution to the manuscript, mainly by checking details reviewers don’t usually have time for. – I will add a major comment to the contents at the end, but take this with the caution that I am not an expert.
In terms of describing the topic of the paper, I agree with the reviewers: The manuscript introduces a reformulation of baroclinicity using dry entropy and test it via correlational analysis.
I think the manuscript displays a clear structure and is well written except for some minor details.

Derivations and equations:

Overall, I could not find any errors or typos in the formulas.

- L. 99: the choice of a running mean filter is probably okay. Since you want to remove variations of a specific frequency, a spectral filter seems to be the natural choice to me.
  We agree that this type of filter is more appropriate in certain contexts, but this running mean filter is used for simple cosmetic smoothing. For this purpose we do not require a precise sharp filter as you describe.
- Eq 12 vs. Fig 2: One has a subscript referring to constant N_0, one does not. Make sure that this is consistent in the text and the analysis.
  Thank you for finding this, it has been corrected.

Figures and Tables:

- Fig. 1: I like the figure and the caption, description is nice and clear. BUT: check the coordinates of the green box in the caption.
  Thank you for noticing this, in the figure caption we used the wrong set of coordinates, it has been corrected in the revised manuscript.
- Fig. 2: like it, caption is good. Check formula for σ_g (see above), add the pressure level used in the analysis to the caption.
  We have added the pressure level to the caption as recommended.
- L. 145 vs Fig. 2c: The quantities compared are not what you state. Make sure this is consistent in text and analysis.
  Thank you for finding this, it has been corrected in text.

- Tab. 1 and throughout the text: variance has units day^{-2}, i.e. that of the mean squared
  Thank you for noticing this, it's been changed.

- Fig. 4: I must have missed something here, as I do not understand why the two lines should differ. σ_s is independent of N_0, while N_0 is readily compensated by altering the coefficient q in σ_n. Whatever the difference is, it may be worth reformulating the text to make it clear. (Also, if the difference is σ_s vs. σ_f, then consider using the notation that is already nicely established)
  This is admittedly not an easily interpreted figure, it has been changed in the revised manuscript. The reason the correlation increases when using a fixed value of stability is essentially that we remove a variable.

Phrasing of difference between σ_u and σ_s:

L. 84-92 nicely state that the difference in theory accounts for ageostrophic effects.
L. 104 and 207 calls this difference 'numerical', which sounds rather suggestive of an error introduced by the methodology
L. 182 calls this difference 'entirely physical', referring back to the underlying quasi-geostrophic theory
This is confusing to the reader. The authors should be clear whether this difference is physical or methodological. I suggest quantifying both, e.g. by computing σ_u from the geostrophic wind components only to quantify the error inherent to the data chosen. The residual part of the difference can then be attributed to ageostrophic effects.
We agree 'numerical' has connotations which weren't intended. These have been changed in the revised manuscript.

Minor comments:

- L. 56, 78, 134: "minus sign" vs. "minus-sign"
  This has been amended.

- Geographical names: be consistent, e.g. "southern hemisphere" vs "S. Hemisphere" (one reviewer suggests a specific format)
  This has been amended.

- L. 162f.: "covariability" is a word I am not familiar with. If this is the same as the more common "covariance", I would suggest switching the formulation
  We are intentionally being somewhat informal here – covariance has an exact statistical meaning, but we are referring informally to the fact that $\langle s_{\mathrm{north}} \rangle$ and $\langle s_{\mathrm{south}} \rangle$ have some weakly negative joint variability.

- L. 166ff.: This conclusion needs reformulation. Weak (anti-)correlation makes it unsurprising that not both variables are good estimates, but does not state which of the 2 – if any – are good estimates.
  We agree our meaning isn't clear in this sentence. We have reworded it in the revised manuscript.

- L. 208/209/212: The numbers presented here show up for the first time, it is not clear where they come from.
  These are indeed important values to state, and they have been included earlier in the revised manuscript.

Major comment – again, I am NOT an expert:

Throughout the manuscript, a box from 30 to 50 N and 80 to 30 W is used to represent the entrance region of the North Atlantic storm track. All of the analysis conveys the message that it is therefore the baroclinicity of the North Atlantic storm track that is investigated.
In the text, it is introduced only as late as L. 188f., but earlier already in Figs. 1 and 2. Whether the following points are right are wrong, this must be moved earlier in the text.
The use of this region lacks motivation in the manuscript. I am not familiar with the literature, but the choice could be discussed in the context of existing literature.
Here are a few points that made me worry about the choice:

• Fig. 1: 50 N is a latitude with a strong climatological gradient in σ_s, and red shading (which the authors interpret as indicating the storm track) extends to the north of this boundary.
• Fig. 1: 30 N only seems to be at the southern edge of the storm track in the western part. Values east of 60 W likely do not experience influence of the storm track.
• Fig. 3: My reading of the variabilities is that the position of the northern border of the storm track is varying about 50 N, leading to the variability in dry entropy at this latitude, whereas the variability as not as pronounced at 30 N as most of the southern edge of the box is not influenced by variability in storm track position.

Overall, the variability in the storm track position may account for much of the assessed variability in area mean baroclinicity. While the chosen box may be representative of a climatological mean position, seasonal and interannual variability could dominate the signal.
I suggest discussing this possibility and possible rephrase to a more general area mean baroclinicity, rather than baroclinicity in the North Atlantic storm track. One possible way to remove this variability is to use a boundary of the storm track that varies over time. This is probably too large of a step for the current manuscript, but could be discussed.
In the revised manuscript we motivate more clearly and earlier on why we have made the choice of this region. The latitudes we have chosen are well established in literature (Hoskins and Valdes, 1990; Ambaum and Novak, 2014) to suitably contain the upstream region of the N. Atlantic storm track, and it is ultimately this upstream region that we are concerned with as this is where the conversion of baroclinicity into baroclinic instabilities is most meaningful. The climatological maxima of baroclinicity is relatively fixed in position during winter (Novak et al., 2015), making these latitude choices sufficient throughout the 30 year climatology we work from.
Regarding your comment on Fig. 3, it is less that the edge of the storm track itself is shifting (something difficult to quantify as the storm track cannot easily be instantaneously outlined) and more that temperatures can be forced by external air masses surrounding the storm track (and of course any number of other processes affecting air temperature).

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