Review of Biddiscombe et al.

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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?

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.

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.

- 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.
- 17-19. 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.

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.

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.

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?

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.

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?

85. Please specify in the text, not only in the figure label, at which pressure level baroclinicity is computed and how N is treated.

175. CAOs are often thought of as eddy involving processes? Ref. introduction and earlier comments.

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.

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).

References

Auestad, H. et al. (2024). “Spatio-Temporal Averaging of Jets Obscures the Reinforcement of Baroclinicity by Latent Heating”. In: *Weather and Climate Dynamics* 5.4, pp. 1269–1286. DOI: 10.5194/wcd-5-1269-2024.

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Novak, L. and R. Tailleux (2018). “On the Local View of Atmospheric Available Potential Energy”. In: *Journal of the Atmospheric Sciences* 75.6, pp. 1891–1907. DOI: 10.1175/JAS-D-17-0330.1.

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Swanson, K. L. and R. T. Pierrehumbert (1997). “Lower-Tropospheric Heat Transport in the Pacific Storm Track”. In: *Journal of the Atmospheric Sciences* 54.11, pp. 1533–1543. DOI: 10.1175/1520-0469(1997)054<1533:LTHTIT>2.0.CO;2.