Review of WCD-2024-2112: Synoptic perspective on the conversion and maintenance of local available potential energy in extratropical cyclones

Overview: This manuscript explored a relatively new technique of quantifying local available potential energy (APE) and applying it to cyclone dynamics for strong extratropical winter cyclones in the North Atlantic basin. Their results show a close connection between the extratropical tropospheric baroclinic zone ("polar front") and a strong gradient in local APE. They also assessed the movement of air parcels using trajectory analysis to explore the tendencies in APE, demonstrating strong APE to KE conversion upstream of a trough axis (advecting into the trough base) and strong KE to APE conversion downstream of a trough axis (ascending generally through the warm conveyor belt). I commend the authors on a well written manuscript that was generally easy to follow. I do have some suggestions for the clarity of the presentation in several places, some changes/new figures that I believe would help aid a reader in interpreting the results, and questions about the sensitivity of some of the analysis to choices in methodology. Thus, I believe this is on the border between a major and minor revision.

General Comments:

1. *Description of local APE and reference state:* I appreciate the challenges of word limits for manuscripts and the necessity of referring readers to prior literature for specifics on a methodology. That said, I would suggest that the authors add a bit more detail/clarity in sections 2.2 and 2.3 to help the readers understand the method a bit more clearly given the relatively new methods being applied here. For example, I found the description of how the reference state was computed (lines 143-145) challenging to understand/visualize, thus making interpretation of the APE figures later in the paper challenging in turn. It was unclear to me at times whether we were comparing a specific parcel to its specific reference state, or a column, or otherwise. Lastly – it may be beneficial up-front in this manuscript (rather than at the end) to discuss local APE in light of other metrics used for assessing energy for storms (eg. Eady growth rate/baroclinicity) and resistance to vertical velocity (eg. static stability).

2. *Discussion of APE changes linked to trajectories :* I appreciated the approach taken here by tracking parcels through the troposphere. However, I found the discussion surrounding figure 3 challenging. I could understand (eventually) how the method discussed in line 216-217 was applied to the analysis, but it made the interpretation really challenging. I can see the idea behind wanting to see how much APE is depleted from the starting location, but it made a) the interpretation of what the actual change in column APE was at a location really difficult, and b) made comparison to figure 9 really challenging. A more clear motivation here may be helpful, as well as clarification here (or at figure 9) about the differences in what we're looking at. One other approach could be to actually show either APE or dAPE/dt for your trajectories (akin to how you have pressure shown in figure 4). Further through this section, it would be helpful to really quantify the contribution due to omega/diabatic heating relative to the respective efficiency terms. Perhaps a box-and-whisker plot of distribution of the efficiency terms for the trajectories at different levels would help? There is good qualitative discussion here (see lines 264-266), but a bit more time spent on the quantitative side would really help. Lastly, your line 266-268 doesn't quite make sense to me r.e. linking the latent heat release to the second max of APE loss over the Canadian Arctic.

3. *Dynamic interpretation/explanation of results:* I appreciated that there were flavors of QG forcing, jet ageostrophic circulations, and PV thinking infused throughout the paper. That said, I think there were a few points where the discussion could've been taken into more depth and/or the QG/ageostrophic/PV discussions could've been more unified (eg. it's three perspectives to explain rising/sinking motion). For example, there was a lot of discussion on jet entrance/exit regions (which were at times not clear to me as entrance/exits rather than poleward flanks of the jet), but much less discussion on the role of curvature (where both the trough and upstream ridge can instigate ageostrophic circulations resulting in +/- omega), transverse thermally (in)direct circulations along jet streaks, and whether there was temperature advection occurring across the jet (which shifts where we see +/- omega in response to ageostrophic responses). Much of this could also be discussed from the perspective of PV theory (which emerges late in the paper but could've been interwoven throughout).

4. *Inclusion of CAOs:* Though I found the inclusion of the CAO case and composite interesting, I wasn't really sure how much it contributed to the paper. This may benefit from either removing it, or better motivating why you're including it.

Specific comments:

- Lines 53-54: Please clarify/elaborate on the 'why' of APE being in the polar middle and upper troposphere and how it is advected (eg. is it a material quantity?).
- Lines 61-62: For local APE, is it only accelerations/decelerations? For example, do all air parcels in the warm conveyor belt that contribute to a positive APE tendency explicitly have a deceleration occurring?
- Lines 110-112: Make two sentences – it's a bit hard to follow as written.
- Section 2.2: Please include units throughout here. It may be helpful to also provide equations for specific density and your diabatic heating rate.
- Line 170: From your figure 1, it appears less that an upper level trough is developing rather than propagating in from the west. Please be sure that it is developing (rather than propagating) if you use the term 'upper-level trough forms'.
- Lines 171-172: I think you need to spell out more how the collocation of high APE with the DT is demonstrating the connection to the large scale circulation (I don't disagree with you, but it needs to be more clearly demonstrated).
- Lines 179-180: Do we know that this is low APE advection vs. a time tendency due to conversion? In theory, one could compute APE advection to show this explicitly. The same goes for lines 183-184.
- Lines 202-204: This isn't really a stand-alone paragraph – please aim to elaborate or merge with another paragraph.
- Lines 206-207: The cross-section does show the cold dome and therefore a troposphere-deep cold anomaly. It does not, however, imply cold air advancing into the midlatitudes. Consider providing different evidence or re-writing.
- Figure 1 cross-section lines/Figure 2: Consider given start/end point markers for your cross-sections (eg. A-A', B-B', C-C') to avoid any ambiguity about the direction of your cross-sections.
- Lines 222-225: This may benefit from a schematic (this infuses with general comment 2 above).

- Line 227, 228 (and elsewhere): Careful on geolocation descriptions. For example – the Canadian Arctic is a pretty expansive area. But it appears your focus in figure 3 may be more on Hudson Bay/Quebec/Labrador? Likewise, I wouldn't consider the Gulf Stream to be adjacent to the Canadian Arctic. A reference geographic map (or outline of the Gulf Stream region) may be helpful.
- Lines 243-248: There's a large region of positive APE tendency south of Iceland that likely is playing a factor the in the cyclone evolution. It would be helpful to either look at trajectories/APE tendencies in that region as well, or to provide justification for not examining it.
- Lines 258-260: I would find it helpful to see some discussion of this also in reference to the efficiency terms.
- Line 280: I'm not sure the line 'detaches from the band of downward …' is the best choice of phrasing. Please reword.
- Line 283: I'm not sure I would classify this as the entrance region of the jet. It may be better discussed as downstream of the upper-tropospheric ridge.
- Lines 290-291: Though the trough may play a role here, so too does the upstream ridge (contributing to QG forcing for descent).
- Figure 5: I found it challenging to differentiate the dark green (2-PVU) contour from the black (300 hPa wind speed) contours. Please consider a slightly more contrasting color choice. I would also add a 'L' to represent your surface low position in both panels.
- Lines 322-323: I would be careful about making this statement. The surface cyclone can influence the upper-troposphere and vice versa (eg. cyclone growth positive feedback cycle), and your cross-section shows a troposphere-deep cyclonic cold anomaly and circulation.
- Figure 8: Aim to make figure 8b seems more its own independent panel (maybe a box around it?). I didn't notice it even when referenced in the text.
- Line 338: How many times steps overlap as being both in the delta-Pmax category and the dmax category?
- Line 341-342: Again, not sure this is the jet entrance region. I suspect curvature dynamics are playing more of a factor.
- Line 345: I think you can lean into PV thinking for the occlusion stage here (eg. evidence of a stacked cyclonic PV anomaly circulation from surface to dynamic tropopause, along with the surface low receding into the cold PV hook.
- Line 351-352 (and elsewhere in this section): Consider adding some values to the description here.
- Lines 352-353: Is this PV field shown anywhere? It may be beneficial to include.
- Lines 372-374; 399-405: Consider bringing in some discussion of the poleward movement of your composite when discussion the radiative cooling of the free atmosphere.
- Lines 379-380: How sensitive is your analysis to the region chosen here? I can understand 2000 km from a synoptic scale perspective, but does this strongly impact the interpretation of storm-scale contributions?
- Lines 415-416: Why not take your cross-section through the center of the surface cyclone? Here your baroclinic zone is south of the surface cyclone simply because the trough is further south to the west of the cyclone (as you noted).

- Lines 425-426: I found this sentence unclear. Which positive adiabatic APE tendency?
- Section 4.2: Please provide information on how many CAO cases were involved.
- Lines 455-456: I think you need to reverse the order here (eg. trough leads to surface cyclone, not vice versa).
- Lines 469-470: Your zonal cross section implies a troposphere-deep circulation, so this feels a bit misleading as written to me.
- Lines 485-492: You mentioned the Bowley et al. 2019 paper in the introduction – though their results are a bit hard to interpret relative to yours (given the global zonal APE vs. local total APE perspective), you may want to include some of their interpretation here. For example, they found a dominant mechanism for synoptic scale APE increase to be ascent on the poleward flank of the wave guide in the exit region of the North Pacific jet, which fits well to the results of Koch et al. 2006 and your adiabatic generation interpretation here.
- Line 517-520: I think your points here would be beneficial to appear earlier in the manuscript when introducing the local APE framework to help unify the global vs. local perspectives.

Technical corrections:

- Line 75: Change ‘and to contribute’ to ‘and can contribute’.
- Line 98: I’m not sure ‘relies’ is the right word choice for your data here.
- Line 167: Please write out ‘potential vorticity unit’ the first time you define PVU.
- Line 343: Please add ‘cyclonic’ between ‘upper-level’ and ‘Rossby wave breaking’
- Line 430: Should this read ‘ascent leading to the positive adiabatic APE tendency’?