Review of **“Identifying Synoptic Controls on Boundary Layer Thermodynamic and Cloud Properties in a Regional Forecast Model”** by Jordan Eissner, David Mechem, Yi Jin, Virendra Ghate, and James Booth

In this paper the authors examine the PBL and the low-level cloud structure in the different areas of a midlatitude cyclone. Using the COAMPS regional model they simulate the transition of a front over the ENA site on Gracioca island using the observations to validate the model results. The results emphasize the differences in the PBL structure between the different sectors of the cyclonic system.

I think that the subject of the study can be of great interest to a broad range of readers and the results may apply both to regional/cloud modelers and researchers in the large-scale dynamics field. However, in order to be ready for publication the manuscript needs to be restructured to help readers reach the main conclusions of the paper. The manuscript in some places repeats itself or presents information that does not serve the analysis. For example, the main objective of the paper is understanding the low-level cloud structure in baroclinic systems, with an introduction which is focused on cloud formation and transitions between cloud regimes and an extensive discussion on the different microphysical schemes but not enough information regarding past research on cloud structures in midlatitude cyclones. I suggest that the main objectives of the research and final conclusions and abstract be rewritten and clarified, and that the introduction adjusted to better serve the reader when presented with the conclusions. Lastly, I would consider rearranging the conclusion section to better follow the outline of the paper and to ease the reader into the final conclusions. Here I detail further major concerns and more specific issues.

Major comments:

1. Aims of the current study (lines 80-86): the aims as written in this paragraph do not reflect what is later achieved. Aim #1 does not apply knowledge on midlatitude systems, but only to one case study. Aim #2 is a methodological plan, but it is unclear what is the more specific scientific objective it aims to address? (e.g., examine parameterizations, assess operational NWP skill? This is unclear).
2. Missing consideration of cyclone airstreams in the baroclinic environment: The introduction and discussion should acknowledge the importance of cyclone-related airstreams for shaping the cyclone environment, including the PBL regimes and clouds. Recent papers address these issues. Specifically for ENA, Ilotoviz et al. (2021) highlighted the role of dry intrusions for shaping cold-sector PBL regimes and clouds. Turnow et al. (2023) note the importance of rain onset for cloud transitions using LES, explaining differences among cold sector regions closer Vs. away from the cyclone center. Liu et al. (2021) studied cloud transitions in post-cold fronts regions of Mediterranean cyclones, relating the transitions to precipitation dynamics. Vast literature relates warm conveyor

belts to clouds along the cold front and in the warm sector, while other studies relate surface fluxes to the passage of cyclones and/or cold fronts. Since one of the aims of the current study is to examine whether known PBL regimes apply to baroclinic environments, these aspects should be better introduced and discussed again in this context.

3. Overall, the representation of boundary-layer clouds in COAMPS is not good (Fig. 10 mainly, but also in terms of cloud cover etc.). Therefore, the notion in lines 506-507 should be toned down. I was left wondering what can we still learn from the results of the evaluation study. For example, one of the key finding is the relatively small influence of the microphysics scheme on cloud parameters (which somewhat raises the question why is this aspect so prominent in the methods section?). Given the model deficiencies, what do we learn more specifically that can guide future model developments?
4. In the introduction (Lines 24-36) there is a long discussion on the transition mechanisms between the different cloud fields regimes. But later there is very little discussion on the subject in the results or the conclusions. Furthermore, it is stated that the model is not able to properly represent the decoupling strength (line 297) that is needed for these transitions to occur. So, what can be learned from the results about the importance of decoupling in this environment?

Specific comments:

1. Line 40: unclear what "following the polar jet stream" means.
2. Line 45: According to what is written In Sinclair et al. (2010) they find that in the cold sector the PBL is deeper, well mixed and unstable with stronger sensible heat fluxes but is several places (e.g., lines 454-455 and conclusions) it is written that the here you find that the PBL in the cold sector is more stable. This should be clarified, and along these lines, the differences between EIS and stability should be better discussed, as they are not interchangeable. (Also true for the citation of Naud et al (2018a) – line 54).
3. Line 46: define here what negative fluxes mean.
4. Line 47: replace "warm front" by "cold front"
5. Lines 71-78: this paragraph misses more recent works about latest model developments.
6. Line 89 onwards: It is better to start this section by motivating the location and the specific case study. Only then, describe the data in the context of the specific case chosen.
7. Lines 105-123: this is a heavy paragraph that is very hard to read. I suggest separating between variables measured and the instruments in a way that emphasizes the variables that will be later analyzed.

8. Estimation of PBL height: the "eye based"/"best estimate" should be more strictly described how the decision of BL height is made based on the measured or modelled profiles. Why is it not automated based on defined gradient criteria?
9. Lines 130-132: be more specific about the indices and thresholds used, and what do the values indicate? Also, for completeness, note how EIS is defined.
10. Line 148: "semi independent": note which of the above-mentioned observations are assimilated into the IFS model that drives ERA5.
11. Line 169: clarify if shallow convection parameterization is still active for the fine mesh.
12. Line 183 onwards: please motivate the developments elaborated in the next paragraphs.
13. Line 189: missing: q_r is...
14. Line 243: front detection: move the description from section 5.1 to the methods section.
15. Line 244: replace "originate" by "spatially connected"
16. Line 247: are the last 3 days still considered a "post cold frontal region"? and if so, in what sense?
17. Line 256: add "with the eastward propagation of the high-pressure systems" after " southerly component"
18. Line 270-272: how do you account for the horizontal drift of the sondes?
19. Line 286: "underestimates the temperature profile" this is unclear, and not true always/everywhere. It rather smoothes the temperature gradients.
20. Line 288: What does the high vertical resolution simulation show? Is vertical resolution the reason for the poor representation of the inversion?
21. Line 293: "overestimate of entrainment" – where is this seen in the figure?
22. Line 297: Elaborate what do we learn from the indices in Fig. 5? What is a reasonable range of numbers to be considered a good match? it is stated the model does not represent the decoupling well (compared to observations). I'm missing some more explanations on what the implication of this on cloud formation is and the PBL structure in the cold sector in the model.
23. Line 304: what do we learn from the results in this paragraph?
24. Line 330 (figure 8): the description of the moving window is not clear. Are you changing the location of the averaging box in the analysis or keeping it constant in the location of the measurements? Why not look at the changes in time at the same location/gridpoint and make a time series plot corresponding to the observations?
25. In line 323 it is written that "the simulations exhibit a degree of bias relative to the observations, but that bias is only minimally attributed to the differences among the microphysical parametrizations." And in line 349 – "Many but not all of the differences can be explained by the differences in microphysical parametrizations and assumed parameters" – This seems contradicting or does it refer to other differences. Please explain or clarify in the text.

26. Line 338: the results for cloud thickness show a very good match in the histogram (Fig. 8c), why are they so different in Table 2?
27. The method of centering the transect around the front is very interesting and useful. However, if the orientation of the transect is constant, doesn't it imply that some of the expected clouds in the northeastern part closer to the warm front are missed when the transect only "catches" the southern part of the warm sector.
28. Lines 369,374: why not match the field on 850 hPa and the height (currently 1 km) for the front detection? As it is now we expect a mismatch because of the slanting front surface with height. Also, it is unclear in Fig. 9 caption if the front is identified in ERA5 or COAMPS?
29. Line 372: the cyclone center is not visible.
30. Line 412: the location is too far north from the trades, the reference is therefore strange.
31. Lines 430-443: such high latent heat flux values are not uncommon in cold sectors of extratropical cyclones. Need to add relevant references on this.
32. The paragraph starting on line 445 and in the conclusion section attributes PBL height to the surface fluxes. However, the role of dry intrusions from the free troposphere were shown to enhance vertical mixing and PBL deepening by destabilizations, reconciling the strong inversion with enhanced vertical mixing and PBL height in the cold sector.
33. Line 498: at the end of section 5 there is a missing guidance of the reader: what can we learn about the relevance of the mechanisms outlined in the introduction from barotropic environments to the current case?
34. Line 542: Need to discuss the limitations of surface turbulent heat flux parameterizations.
35. Figure 3: what are the green dashed lines? The field of 500-hPa height appear here with no context – need to add this field to Fig. 2.
36. Figure 4: This figure is out of context and can be moved to the supplementary material for smoother reading. Please also add a bar to show the variability among the different model runs. Note errors in the caption of the curve colors, and spell our MAE.
37. Figure 7: which method is used for determining the PBL depth? Use the same tickmarks on both axes in the third panel too. Spell out MAE.
38. Figure 8: the black histogram in panel d is unclear. Are there missing bins?
39. Figure 13: unclear if the fluxes are observed or from ERA5? Which method is used for determining PBL depth?
40. Figure 15: this figure does not add new information that is not evident in the two figures that come before it or it is not clear from the text what it adds. In the text itself regarding this figure I would expect further discussion on the prevailing air streams in the different sectors of the cyclone that actually define the y-axis of the plot.

Technical comments:

1. Line 49: the sentence is unclear, please rewrite.
2. Line 276: replace "all" with "and"
3. Line 332: remove minus sign

4. Figure 2: The cold front line is not visible. In caption – remove "t" from "start"
5. Figure 5 caption: remove "n" from "ration"
6. Figure 10: missing panel labels (a,b,...) referred to in the text.
7. Figure 11 caption: what are the vertical lines?
8. Line 466: change “clout” to “cloud”
9. Line 488: fix “.)” to “).”

References

Ilotoviz, E., Ghate, V. P., & Raveh-Rubin, S. (2021). The impact of slantwise descending dry intrusions on the marine boundary layer and air-sea interface over the ARM eastern North Atlantic site. Journal of Geophysical Research: Atmospheres, 126(4), e2020JD033879.

Liu, H., Koren, I., Altaratz, O., Heiblum, R. H., Khain, P., Ouyang, X., & Guo, J. (2021). Oscillations in deep-open-cells during winter Mediterranean cyclones. npj Climate and Atmospheric Science, 4(1), 12.

Tornow, F., Ackerman, A. S., Fridlind, A. M., Tselioudis, G., Cairns, B., Painemal, D., & Elsaesser, G. (2023). On the impact of a dry intrusion driving cloud-regime transitions in a midlatitude cold-air outbreak. Journal of the Atmospheric Sciences, 80(12), 2881-2896.