Review **"Identifying Synoptic Controls on Boundary Layer Thermodynamic and Cloud Properties in a Regional Forecast Model"** by Jordan Eissner et al.

This is an interesting study which discusses the boundary layer cloud properties of a midlatitude synoptic cyclone. It uses regional weather model results, including sensitivity tests, as well a detailed comparison with various type of observations. Given the broad scope of the study, i.e. the study of boundary cloud (dynamics) in a large-scale environment, this is potentially interesting for both communities. Therefore, the framing is quite important, however here is still quite some improvement possible. I am reviewing this manuscript from a perspective on my expertise on large-scale atmospheric dynamics, and therefore this review has a slight focus on that.

1. I think the most important part which can be improved is the general motivation on what is new in this particular study and how it fits with earlier studies. There has been quite a lot research already on clouds and precipitation related to midlatitude cyclones. It would be good that the research gap is explicitly stated. In the conclusions the authors mention (lines 566-569) that they *"In this work, we demonstrate that the analysis method provides a pathway to compare boundary-layer cloudiness forecasts against observations in the context of the different sectors of midlatitude cyclones and in a manner that minimizes errors associated with misplacement of the cyclone center and frontal structures (i.e., phase error)."* If this is the main gap the study tries to address it would be good to explicitly state this in the introduction. Moreover, it would be good to also more explicitly state what 'the analysis method' refers too.
2. Related to this, in the first paragraph two distinct cloud-transition mechanisms are discussed: The deepening warming hypothesis and a cloud transition mechanism that relies on precipitation. Given the first objective of the study ("*to apply our understanding of boundary-layer clouds, largely relevant to barotropic atmospheres in the subtropics, and expand it to boundary-layer clouds accompanying midlatitude baroclinic synoptic systems"*, lines 80-81) some expectations are set that a direct comparison is made between these mechanisms and the case discussed. However, this is never explicitly done. I am not saying the authors should definitely do this, however I still find it hard to understand what the conclusion is regarding this first objective after reading the manuscript. This is also because in the conclusions the main focus is on the results regarding the second objective. It would be good to more explicitly state the conclusions regarding this objective.
3. The section on how the cyclones are detected and how the cross-frontal composites are made (section 5.1) is quite detailed, I would suggest to move that to the methods section, since that is also the section where the reader expects this information. Moreover, it is already used before in e.g. Figure 2.
4. The presentation of the figures is already much better with the corrections made, however could still be improved sometimes, see detailed comments below.

Minor comments:

Abstract line 1: It might be my ignorance, but can you really put this so strongly? Or do you refer mainly to the knowledge in a theoretical (boundary layer) framework? I think e.g. there is e.g. quite a lot of studies already e.g. on mesoscale rainbands already decades ago (e.g.

Houze et al., 1976, Matejka et al. 1981, Knight and Hobbs, 1988), and there are also quite some empirical studies on clouds associated with fronts and extratropical cyclones as well.

Line 8: “The Frontal region” does this refer to the cold or warm front or the fronts in general?

Line 41: Given the broad scope of the study and possible broad background of the readers, it might be good to introduce the regions of there one would large-scale vertical ascent in a typical extratropical cyclone.

Line 90-95: I think the discussion on the summer cloud properties in the Azores region is not strictly necessary and might be even be confusing, since this study focusses on a week in winter.

Line 100-104: I do not completely see why a positive NAO phase would not be representative for winter storm systems. I would earlier maybe state the opposite, since the negative phase is more associated with a blocked weather pattern over Europe. Furthermore, I would also argue that the position of the studied extratropical cyclone matches quite well with the average position of the storm tracks (see e.g. Neu et al., 2013). Therefore I would suggest to rewrite this part a bit.

Line 152: It might be good to already introduce the exact period of the model simulations, or remove this part completely here, since now it is scattered over two paragraphs.

Line 217: “Only over the ENA site” Which region is chosen for this?

Line 217-219: Given the use of the same Richardson constant of 0.5, do the authors have an idea why the underestimation in the regional model is only by about 400 meters, compared to the 800 meters using the sounding data?

Line 243-245: The system is already occluded probably, though it is hard to see from the chosen region, which might be the reason why your front is not originating at the centre. The phrasing suggests that this is not visible in the temperature fields, if this is not the case I would rewrite this part. Furthermore, as far I understand you are using a frontal detection at 1 km? Given the (potential) tilt of fronts with height, one would not always have 100 % correspondence anyways.

Lines 382-385/Figure 9: Why is the transect not centred in the middle of the cold front? And how is the exact location along the cold front determined?

Figure 2: Can the displayed region be extended a bit to the north? I understand that the main region of interest are the frontal regions, but now the studied cyclone is not (completely) visible at displayed time steps.

Caption Figure 2: “Red start” should be “Red star”

Figure 4: Over which region is the MAE calculated?

Figure 5: I also would suggest moving the legend out of the plotting area here. Moreover, the legend is also not completely clear to me, does it refer to the lines plotted or the coloured squares indicating the cloud boundaries?

Figure 8: The densities of panel (e) are beyond the y-axis. Futhermore I am a bit confused on that the total density (surface area below the lines) does not seem to add up to the same value for each subplot. Is this due to the different number of observations points in each of the subpanels?

Figure 10: I understand the choice of a blue colour to represent of the cold front, however given the blue colour map in most panels this makes it hard to distinguish from the background. I would suggest making the location of the front black.

Figure 11, caption: I wondered here why the authors to use the median here, since using only 8 forecasts might result in a skewed result. Have the authors compered to e.g. using the mean to see if the results would be very different? Moreover, is the median calculated at each point separately?

References:

Houze Jr, Robert A., et al. "Mesoscale rainbands in extratropical cyclones." *Monthly Weather Review* 104.7 (1976): 868-878

Knight, David J., and Peter V. Hobbs. "The mesoscale and microscale structure and organization of clouds and precipitation in midlatitude cyclones. Part XV: A numerical modeling study of frontogenesis and cold-frontal rainbands." *Journal of the atmospheric sciences* 45.6 (1988): 915-931.

Matejka, Thomas J., Robert A. Houze Jr, and Peter V. Hobbs. "Microphysics and dynamics of clouds associated with mesoscale rainbands in extratropical cyclones." *Quarterly Journal of the Royal Meteorological Society* 106.447 (1980): 29-56.

Neu, Urs, et al. "IMILAST: A community effort to intercompare extratropical cyclone detection and tracking algorithms." *Bulletin of the American Meteorological Society* 94.4 (2013): 529-547.