Weather Clim. Dynam. Discuss., referee comment RC1
https://doi.org/10.5194/wcd-2021-53-RC1, 2021

Comment on wcd-2021-53

Anonymous Referee #1

Referee comment on "A characterisation of Alpine mesocyclone occurrence" by Monika Feldmann et al., Weather Clim. Dynam. Discuss.,
https://doi.org/10.5194/wcd-2021-53-RC1, 2021

Main comments on WCS-2021-53

The paper is mainly focused on the development of a mesocyclone detection algorithm based primarily on radar data (plus COSMOS model data). The method is then applied to a 5-year data set for Switzerland, climatological aspects are investigated, and influencing variables such as large-scale flow, diurnal variation, and orography are considered. In addition, some relationship is made to one past hail study.

Basically I enjoyed the paper, I think it is informative and well done, and it should be made available to the community. Especially in the first part (development of the algorithm/climatological consideration), I had primarily minor comments (but in sum quite many). However, in the second part I have some deeper comments, so the paper needs to be reviewed again (Major revision).

My major points are:

- Minor major point, L61-66:
  The purpose and objectives of the paper should be better formulated in the introduction. The authors mainly describe the methodological approach. It would be more interesting to formulate the question about the physics (e.g. question about influence orography, synoptic...). What scientific questions should be addressed by the obtained data set?
- Minor major point, L75:
  The description of the COSMO data is inadequate. Which time period is used, which variable exactly? Which area? Resolution? Data generated by yourself, or from others?
- In some places it needs to be described more clearly how exactly the sample set is composed (see comments in the text; e.g. one track vs. all detection time steps).

- Section 4.1 to 4.3:
  Please put the results more in context with other work in Switzerland and similar studies in Europe; compare it (not only with Nisi et a., 2018; see PDF for more details / suggestion).
- Figure 5:
  - Please rethink the colorbar; NW, N, NE, SE is hard to see and distinguish (also think of potential Red-green color blindness).
  - Suggestion: As a supplement you could make very well a wind rose in dependence on the frequency. Maybe you can split the analyses over the 7 months to clarify if there are time dependencies?
- Section 4.2 and Figure 6b:
  I would be more cautious with your interpretation. Overall, Figure 6b shows a large variability. Your interpretation of 4 UTC sample should also take into account that there are very few data (8 or 9 cases) and you should be careful not to interpret too much into your data.
  Furthermore, which time regarding the track is the basis of your evaluation? Start time? The time in the middle of the track? All this can have an influence. When interpreting the figure, it is important to consider how the **duration** of the individual tracks is.

- Figure 7:
  Maybe it is interesting to split Figure 7 regionally? 1x north and 1x south of the alpine ridge?
  Perhaps your statement "the majority of rotating storms move uphill" is purely coincidental and has no real systematic background, as your statement can be attributed only to the region and associated synoptic flow direction.
- The authors should better explain how the thresholds in Table A1 were chosen.
  Are there any comparable values in the literature? Have sensitivity studies been performed with varying thresholds? How sensitive are the results due to the choice of thresholds?

Minor comments:

- In the attached annotated PDF-file, you will find several further minor comments, suggestions and reference suggestions.

Please also note the supplement to this comment:
https://wcd.copernicus.org/preprints/wcd-2021-53/wcd-2021-53-RC1-supplement.pdf