1 Reviewer #1

Summary: 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).
Thank you very much for your detailed review, which is very beneficial for improving our manuscript. We highly appreciate the input and are happy to improve our work based on your comments. In the following, we will address the remarks in detail.

Major Comments:

1. 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?
The purpose of the publication is to establish a baseline description, how mesocyclones occur in an Alpine environment. To highlight this, we have rephrased part of the introduction (e.g. lines 75-77 in the revised, tracked-changes manuscript below).

2. 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?
The description of COSMO data has been expanded and more details were added, see lines 88ff.

3. 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).
We have added more precise descriptions of this in the figure captions and the main text.

4. 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).
Overall, more literature has been added as reference. In multiple sections we expanded the comparisons to comparable work in Europe.

5. 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?
The colorbar has been revised, in addition to the style of the plot. As the "green" classes only represent very few cases (<5 %), we summarized them in one class marked in grey. We superimposed the complete thunderstorm tracks as lines over the rotation initiation points to demonstrate the propagation of

the single storms. In addition, histograms about the class distributions have been added. With only 5 years of data and a high inter-annual variability of severe convection, a seasonal analysis is not very robust and shows artifacts from single events even more than the diurnal cycle. However, this analysis would be very interesting for future work, once longer homogeneous data series are available.

6. 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.
   We have reprecised in the text (lines 312ff), that the time references the time of thunderstorm initiation. We also highlighted the small sample size as a source of uncertainty. However, with similar behaviour present in severe hail storms, where the sample size is much larger, we consider this an important parallel.

7. 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 uphill movement is indeed likely due to their location and propagation direction, however it still appears that tracks moving towards the main Alpine ridge into increasingly high altitude and complex terrain systematically weaken severe convection. We have rephrased this section (4.3) to clarify our conclusion. Additionally Fig. 7 has been split regionally into North and South.

8. 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? *These thresholds were selected from the existing operational thunderstorm radar tracking algorithm TRT at MeteoSwiss. In the development of TRT extensive threshold tuning was performed. We clarified the source of these thresholds in the text.*

Minor comments:
In the attached annotated PDF-file, you will find several further minor comments, suggestions and reference suggestions.
We incorporated the vast majority of recommendations. The final changes can be seen in the revised manuscript with tracked changes.

2 Reviewer #2

Summary: Overall, the manuscript is of interest to the science community and within the scope of Weather and Climate Dynamics. The scientific approach is mainly good. However, the results are not discussed thoroughly and more relevant references are needed. The presentation is mainly good. The figure captions are often to short and not precise; important information that is needed to understand and interpret the analysis is "hidden" in the rather long appendix.
Thank you very much for reviewing this manuscript. We value your feedback and hope to improve our manuscript with it. Below we will address your comments in more detail.

Specific Comments:

1. The discussion of the influence of the radar quality index on the derived spatial distribution of (mesocyclonic) storms is not very clear. E.g. What does a quality index of 0.5 mean? Should the results in this area be considered correct? Or underestimated?
   With no additional means of establishing a database of mesocyclones, we cannot derive a quantitative relationship between the quality index and the likelihood of successfully detecting a present mesocyclone. Generally, the lower the index, the higher the likelihood of underestimation.
   We present the quality index in this manuscript to remind the reader of the fact that the capability of detection of mesocyclones by a radar network in complex orography is limited by a number of factors, in particular visibility, minimum and maximum altitude of measurements, spatial resolution and data quantization. We merge these into one value, the quality index, in order to give a synthetic view of the underlying detection capability. We have added additional information on the interpretation in section 4.
   When looking at phenomena that are more influenced by a single component of the quality index (e.g. spatial resolution for small storms), one may choose to directly use said component. However, here, we are dealing with more complex limitations that need to be respected.

2. Meteorological expectations, derived distributions, limits of the radar network as well as assumed correlations between mesocyclones and hail are often mixed or used to support findings or assumptions in a rather ambiguous way.
   We have revised section 4, including the subsections 4.1-4.3 to clarify, which statements are derived from other studies, which refer to analyses in the publication, and which are hypotheses.

3. In the discussion it says "The accompanying relative quality index map helps interpret, where the mesocyclone detection is impeded by the physical limitations of the radar network." However, this information is not thoroughly used in the presentation and discussion of the results.
   We have included additional references to the quality index throughout the analysis in section 4.

4. The discussion of the "synoptic weather situation" is rather short. This could be elaborated. Also, it should be clearly stated which sentences are hypotheses and which are based on analyses (references).
   We have expanded this section, also by referencing further work done on this subject in Europe. We have clarified where our statements are derived from.

Technical corrections:

1. L. 62: Including these place names in a map would help those readers not familiar with the study domain.
Reference points were added in the description, to help identify these regions. To avoid overloading Fig. 1, which is used as a geographical reference in this paper, we refer to another paper showing the exact locations (line 69 f in the revised manuscript included below).

2. L. 88: 1 Apr to 31 Oct
In the context of restructuring this section, the sentence has been rephrased, referring to the period as the months April through October (line 83).

3. L. 123: distance from the radar.
Added to text (line 154)

4. Fig. 1 Caption: Is this the quality index only for mesocyclone detection or for all radar derived products?
Generally the quality index is applicable to all radar derived products, however not all limitations are valid for all meteorological phenomena. Including the max. altitude of measurements has particular relevance for convection, thus the quality index is tailored to convective phenomena and in particular rotation in convection. The figure caption has been expanded accordingly and we added information on this in lines 168 ff.

5. L. 151: Typically, negative velocities indicate a component of the wind towards the radar and positive values indicate a component away from the radar. Please clarify.
In Switzerland it is convention to label positive values as inbound. To remain consistent with other publications in the Swiss context, we opt to use the same convention, but highlight it in the text (line 191).

6. Fig. 3 Caption: Individual mesocyclone detections (there could be detections in successive radar scans – thus multiple detections per storm) or mesocyclone storm tracks per km^2? Wording should be clear and consistent throughout the manuscript.
These are indeed individual mesocyclone detections, this has been clarified in the caption of Fig. 3. Additionally we made adjustments throughout the manuscript to clarify, whether we are referring to complete tracks, or single detections.

7. Fig. 5 Caption: Spatial distribution of detected mesocyclones classified by the synoptic flow. The colours denote the prevailing wind at .... level. The percentages given in the box ... .
After revising this Figure, the caption was rephrased and expanded.

8. All maps: Could you use latitude / longitude instead of "swiss x-/y-coordinates"?
Using the local geographical projection provides us with a greater deal of accuracy in the projection of the maps. For orientation purposes the coordinates are now shown in km instead of m, to provide a better idea of scale, and the lower left corner is labelled with lat/lon coordinates for reference.

9. L. 224-225: Please explain.
   With the revision of Fig. 5, this section now has a better graphical reference and should be more clear to the reader (lines 285 ff)

10. L. 239: How are storm area and track length defined?
    The definitions have been added in line 307.

11. L. 245: The influence of the low number of cases on the results should be discussed.
    We have added statements discussing the low number of cases, and why this feature is still noteworthy, in spite of the uncertainty (lines 312 ff).

12. L. 255: What is the influence of the radar data quality on the mesocyclone detections at high altitudes and its intensity estimation?
    We reinforce the observed spatial trend by referring to radar-independent thunderstorm climatologies (lines 337 ff).

13. Fig. 7: Use a boxplot instead. Also provide the number of cases per selected altitude bin.
    The observed decrease is a decrease in the maximum achievable rotational intensity. In any given location the majority of detections have weak rotational intensities. An investigation of the behaviour of different quantiles showed that the decrease is mainly present in the upper quantiles. We added additional information about this to the text (lines 323 ff).