Interactive comment on “Simulations of the 2005, 1910 and 1876 Vb cyclones over the Alps – Sensitivity to model physics and cyclonic moisture flux” by Peter Stucki et al.
Anonymous Referee #1
Received and published: 27 June 2019

General comments:
The authors downscale three historic flood events caused by so-called Vb-cyclones and analyse the conditions under which the high-resolution simulations capture precip- itation over Switzerland best. They compare model set-ups with different initialization periods, parametrizations and nudging. The most important factor determining the result with respect to precipitation is the correct representation of the cyclone path.
The article is well written and structured. It addresses three extreme precipitation events and is therefore within the scope of the journal. The authors systematically analyse different model configurations and initial data sets which allows them to identify influencing factors of higher and lower importance. The result is a good reference for other scientists applying dynamical downscaling to study extreme precipitation events. It also shows the pros and cons of using 20CR reanalysis for initializing regional model simulations, as it turns out that deviations to observations increase for events lying further in the past.
There is however one aspect that is missing in the paper. The authors downscale to a precipitation resolving resolution of 3 km. As a motivation they cite Zängl 2007 and Prein et al. 2015 who analysed the 2005 event (page 3, line 29). The simulations performed by Stucki et al. would allow to study the effect of convection resolving simu- lations versus simulations with convection parametrization also for the two other cases. The paper would strongly benefit from adding an additional section which discusses the effect of the last downscaling step in more detail.

We thank the reviewer for the clear and helpful comments. Specifically, we appreciate the valuable note suggesting analyses on the effect of the last downscaling step and the associated convection. This is indeed an important aspect that we have neglected in the previous version. To address the suggestion, we have now redrawn Figure 5 and added two panel rows showing the contribution of convective precipitation in the 9-km domain, where precipitation is parameterized. This gives insights in the process of (embedded) convection during the events. In addition, we add a new Figure 11. It shows surface weather simulated for a historical maximum-precipitation member at three instances in time. With this, we can better show how shifts in the cyclonic flow and moisture transport translate into regional to local surface weather and precipitation patterns.

Specific comments:
p.2 l.4: The cited article (BAFU 2005) is missing in reference section.
We thank the reviewer for the hint. It is in fact Bezzola et al. 2008, which is available in the reference list.

p.2 l.12: Cyclogenesis of Vb cyclones can be outside the Mediterranean region but most of them pass the Golf-of-Genoa region (e.g. Messmer et al., 2015).
We have adopted the suggested wording, and we agree that a broader definition is more adequate. It reads now as follows: “Cyclones on a Vb track are associated with heavy to extreme precipitation over Central Europe, and particularly north of the Alps (Hofstätter et al., 2016, 2018;

Nissen et al., 2013). Most of the cyclones following the Vb trajectory are generated in the western Mediterranean region, and most of them pass the Golf-of-Genoa region (Hofstätter and Blöschl, 2019; Messmer et al., 2015). For this, they are also called Genoa Lows at this stage; Bezzola et al. 2008)**”**

Table 2: Does a positive mean absolute error means that the model initialized with 20CR produces more precipitation than observed and that initialization with ERA40 (p.11) overestimates precipitation even further? This is probably not the case as it would be in contrast to Fig. 2. Please define what a positive MAE value indicates. From looking at Fig. 2 I also don't understand how MAE48h can be 27.75 for sp10 when averaging over 10 ensemble members. Shouldn't the error be much higher?
We have added a short explanation of MAE in Sect. 3.2. MAE gives absolute values, that is, the deviation or distance from the observation – regardless of its sign. The bias is actually negative. We also checked the calculations and come to the same results.
The new text reads as follows: “The MAE measures the average distance between forecast and observation, and is preferred over RMSE because it is more resistant to outliers, and over correlation coefficients because we are more interested in accuracy than linear association (Joliffe and Stephenson, 2012).**”**

p.11 l.28: Please give a short explanation how cyclone fields are calculated (1-2 sen- tences). To understand what is shown one shouldn't need to read another paper. Please also clarify if cyclone fields and cyclone tracks are both calculated at SLP.
We agree that this needs to be clarified. We have inserted a short explanation, and we have adapted the caption in Figure 4. The new text reads as follows: “The algorithm detects cyclone fields in terms of a finite area around a regional SLP minimum, that is, by a closed SLP contour line. The regional SLP minima for each cyclone life cycle are stored as cyclone tracks, and the presence or absence of a cyclone is represented in a binary field for each grid point and time step.**”**

p.12 l.17 +others: You use storm track and cyclone track synonymously. This is confusing as you only show cyclone tracks. The term storm track is mostly used for the variance at synoptic time scales (e.g. Hoskin and Hodges, 2002: New perspectives on the Northern Hemisphere winter storm track. Journal of Atmospheric Sciences, vol. 59, Issue 6, pp.1041-1061). I suggest that you change "storm track" to "cyclone track" everywhere in the manuscript.
We fully agree; the suggestion has been adopted.

Fig.4 first row: Genoa cyclogenesis is not visible on these figures.
We agree. Shown are cyclone tracks at the 500-hPa level in the 2° by 2° reanalysis. We have made this clear in the caption.

Fig.4 middle row: There is a time step indicated in each panel but the caption suggests that different time steps are combined. This is confusing.
We have rephrased the caption and we emphasize now that it summarizes the ensemble at one specific time step. The caption reads as follows: “. (g,h,i) Cyclone centers identified for sea level pressure at (g) 21 August 2005 18 UTC, (h) 13 June 1910 18 UTC, and (i) 10 June 1876 18 UTC, the same time steps as in (d, e, f). Colored dots and the tick marks in the color key indicate the

number of cyclone tracks located at a specific grid point at the respective time steps. Greyscaled lines mark the cyclone tracks over the period of 48 h before to 48 h past the respective time steps. Darker (lighter) grey shades indicate more (less) cyclone tracks along a certain path**”**

Fig.4 last row: Please redraw. It is almost impossible to see the cyclone tracks. In ad- dition, the caption states “as in middle panels” even though g, h and I show a sequence of days while the d, e and f show single time steps. I also don’t understand why the numbers on the colour bars differ for g, h and i.
The panel is redrawn, with larger and darker lines, a smaller map area and larger circles. In addition, we have rephrased the caption. We also describe the last panel better in the text now, which in fact supports our conclusions. This is also in line with a suggestion by Reviewer 2.

Fig. 5a: There is a very prominent precipitation peak before the event starts. The text does not give any explanation for this peak.
This is true. We have now addressed the peaks in the text, including that they are convection-driven. We have also added the simulation results from WRF-ERA-Interim. We cannot fully conclude on the reasons for the overestimation during the spin-up, but found that at the very beginning of the model initialization, the interpolated temperature field is coarser in 20CR-WRF for the 9-km domain, which leads to areas with high temperatures over the Alps and excess moisture production in the first hours of the simulation. This mechanism is now summarized in the text.

Figs 7, 8 and 9: Are the black dots supposed to indicate the land sea mask? This does not come out neither in print nor on my screen. Please redraw.
This is a common issue for all reviewers. Apparently, this happened during rasterization of the original vector format to PNG format, such that it can be inserted in the submitted Word document. We still have trouble to produce a good PNG file, but hope that the image quality is now sufficient. The PDF to be submitted for print production should be in good quality.

p.24 last paragraph: The decreasing differences to observations with time in 20CR are probably due to the increasing quality/amount of data that is assimilated in 20CR. It would be good to mention how the input data for the 20CR product has changes between 1976 and 1910.
This is a good suggestion. We inserted the according numbers from the ISPD database in the conclusion; they are really quite illustrative.

Fig. A2 caption: “(time periods as in Figure 2)”. Fig. 2 only shows 2005 Fig. A2 shows all 3 episodes.
We thank the reviewer for this remark; it should refer to Fig. A1, of course.

Technical corrections:
p.9 l.11: Value in the table is 0.59 the value in the table captions is 0.49 (sp10 48-hour precip). Which one is correct?
We thank the reviewer for this hint. The value in the table is correct; we have corrected the one in the text.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess- 2019-174, 2019.

Further changes made

- **Additional references: Hofstätter and Blöschl, 2019; Zbinden, 2005 (Annals of MeteoSwiss); Cioni and Hohenegger, 2019; Coppola et al., 2018; Compo et al, 2015; Joliffe and Stephenson, 2012; Wernli et al., 2008.**
- **Figure A2 redrawn with the maximum- and minimum-precipitation members. This makes more sense since we focus on these in the text.**