Reply to Review from Referee 2 of nhess-2019-27 "Characteristics of a Hailstorm over the Andean La Paz Valley" by Marcelo Zamuriano et al.

We thank the referee for the fruitful comments and suggestions to improve the quality of the manuscript. We take them gladly into account and we have enumerated them followed by our replies marked with R:

Major comments

1) First of all, the authors must clear that the insights found on the mechanisms underlying the development of this storm are indeed insights on how the WRF simulations works, more than on how this specific storm really evolved. In fact, the observed data are so few, that while the authors can state that the control run has a general agreement with the low resolution satellite-derived observations, they can't prove that the local-scale features evolved follow- ing the details simulated by WRF, since there are no radar data, no lightning data and no hail data to compare with. Also, there is no clear comparison with the only available precipitation data, that is, "The most important rainfall quantity was therefore registered around La Paz next to the mountain slopes (with measured values of around 50 mm)." I assume that 50 mm of precipitation is the daily accumulated value, while the simulations data of Figure 10 show only 3-hours accumulated rainfall, with maximum peaks of about 20 mm. That is not a good forecast if what reported by Hardy (2009) is true, that is, that there were 39.4 mm in only one hour.

-R: We understand the referee's concern about the observations limitations that are not able to validate the main features simulated by WRF. We therefore have clarified the numerical nature of the mechanisms by modifying the title to "Numerical Insights of a Severe Convective Storm accompanied by Hail and Flash-flooding over the Andean La Paz Valley" (that can be still modified in our final version). We also have realized during out reply to Referee 1 that our rain-gauge precipitation analysis was rather superficial and we have updated our manuscript and figures to better reflect our findings.

2) While there is much emphasis on hail, from the title of the paper to the tentatives "to investigate the physical processes leading to the hailstorm formation", in the presented material there is really no specific information on hail and in particular on what would favor a severe hailstorm instead of, for instance, a severe storm not characterized by much hail (that is what one would expected from the analysis of only one case study). The Hailcast moduleis coupled into WRF, but I can't find anywhere any clear Hailcast result, like, for example, a map of simulated maximum hailstone diameters. Only a small blue line (very hard to note) is overlapped in Figures 4 and 10, to show hailstone "around 5 mm" or "between 4 and 6 [mm]" of diameters, which seem too few for evaluating the hailcast forecast. Lastly, how important was the hail aspect for the damaged produced by this storm and, in particular, for the 69 casualties? I suspect that the flood aspect (with saturated terrains and steep slopes) could have been more important. In conclusion, maybe that emphasis should be given more on the flash-flood aspects than on hail (and the title changed). Otherwise, the key role of the hail should be better highlighted.

-R: The referee is correct that the flash-flooding was responsible for the high number of casualties. We understand the relevance of the flash flood and our updated title takes into account this. We also realize that the hail role in the flash flood is not very clear from our manuscript. As stated by Hardy (2009) the hailstones played a role in blocking the sewage system and directed the flows over the streets; hence the crucial importance of hailstones reproduction by WRF. We acknowledge the referee's point about HAILCAST and the hailstone size that are rather small for hail forecast evaluation. We suspect that WRF underestimated the storm severity and we add some comments about the possible reasons for that, accompanied by clearer figures and captions.

3) The material is not presented in a very linear form and there are many repetitions, like for example between Section 4 Discussion and section 5 Summary and Conclusions. I encour- age the authors to remove most repeated material and to merge together sections 4 and 5.

-R: We have realized that our discussion section can be improved and we may have jumped into conclusions without presenting stronger arguments. We gladly accept the referee's suggestion to revise and remove repeated material; at the same time we have updated this section in order to present better our findings.

4) Figures are really too small and very difficult to understand. I suggest to put only one "large" figure per page. Please add at least one map of hail simulated by Hailcast. Because of these major comments, I kindly ask for a major revision.
-R: We take into account this comment and we have updated the figures to make them larger and easier to understand.

Minor comments

5) Abstract: The iconic hailstorm "iconic" is an appropriate term? I would remove it here and during the whole manuscript.
-R: Perhaps this term is not appropriate, we decided to use rather the term "historical" or just remove it along the manuscript.

6) 1, 19: taking into consideration Bolivian farmers perception of extreme events. Is it pertinent?
-R: This sentence points the lack of systematic hail observation system and most of the knowledge comes from farmer perception. We have rephrased this sentence.

7) 1, 20: Andean farmers perceive an increase of the frequency and intensity of storms and hail. How much scientific can be such statement?
-R: Local traditional knowledge is still widely used by farmers. While it can not be considered very "scientific", it has been shown that it can be complemented with scientific information for a better communication with local people in rural areas of the Central Andes (Rosas et al. 2016). We point this issue and make clearer our goals in the revised manuscript.

8) 1, 25: was described as an unprecedented crisis. Maybe crisis is not the best word to use here.
-R: We have changed it to "emergency"

9) 2, 1: (that resulted in 69 casualties How they died? Because of flood, wind gust, giant hail?
-R: In conformity to our reply to comment 2, we have updated this sentence to make clear the flash-flood was the main driver for casualties.

10) 2, 5: the knowledge is taken from local peoples perception of hail frequency and intensity. That is too few to write a paper on "hailstorm".
-R: The goal of our original sentence is to highlight the observational limitations but at the same time the urgency of a better understanding of this kind of events over the region. We have better justified the motivations and updated the title.

11) 2, 5-7: The lack of formal physical process knowledge about local thunderstorms formation over this region is evident as we take as example the explanation given by the SENAMHI about the plausible mechanisms for this particular cell formation. I don't think that criticizing SENAMHI explanation is relevant here. If their explanation "might sound trivial for a super-cell formation", also your explanation that convective cells "were triggered by a mix of low level wind convergence, surface heating and orographic forcing" (page 10, line 8) might sound trivial to others.
-R: We can see that the term "trivial" might not be the best one here. We have reformulated this sentence.

12) Introduction: In this section I would expect some explanation about literature studying hailstorms cases, like for example the work by Kunz et al. (QJ RMS 2016), if emphasis on hail will be maintained. Also some references to orographic precipitation studies could be added, like for example seminal works from Rich Rotunno, Robert Houze, Daniel Kirshbaum and others.
-**R:** We are happy to expand the introduction with more hailstorm cases in other regions. This is also useful for later on when we discuss how our case study can be compared with other similar studies in other regions.s.

13) 3, 4: 1745 LST: I searched in Internet for what means LST and it is the "Local Sidereal Time", while you probably was reffering to the "Local Time" (LT). Please correct all LST and describe the relation between LT and UTC time for your specific location.

-R: The local solar time (LST) is defined in page 1, line 24. We decided to use it since we discuss daytime thermal circulation and we use such terms as "late morning" and "early afternoon" along the manuscript. However, we understand the referee concern about not using UTC time and we added the LST - UTC times correspondence (LST = -4 UTC) in our revisions.

14) Figure 3a: I can't really see the observed rainfall distribution. Maybe you can plot the accumulated values at each SENAMHI station located in that area?
-R: We realize many figures may not be well seen and we have updated them.

15) 4, 16: We study the role
-R: Thank you, we have used this correction.

16) 4, 21: We assess the presence of the main ingredients for a hailstorm to occur (moisture, instability and lifting): First of all, that language is more appropriate for a cookbook recipe than for a scientific article. Second, if there are -unfortunately- some people that oversimplify the thunderstorm forecasting problem to such a level, they do it for thunderstorm in general, not specifically for "hailstorm". So, please, describe some general features of the environmental conditions that favor thunderstorm development and, if you have any evidence, of specific conditions that instead favor hailstorm formation. Otherwise, simply list the parameters that this study investigates.
-R: Both point are fair. We have rephrased this paragraph in order to convey better the methods used and main findings.

17) 4, 23-24: The low level moisture transport vectors were calculated following IVTU = 1 200
qudp
 g SFC . . . It is calculated from the surface SFC up to 200 hPa.
I really can't understand how can you call it "low level" moisture transport if it is calculated up to 200 hPa!
-R: The referee is correct, the calculation is made for all the column up to 200 Hpa. We have removed "low level".

18) Figure 2a-2d: Please define what are "sensor count"?
-R: We have added the definition.

19) 4, 27-28: the presence of low level water vapor is not well captured in this band but its corroborated with infra-red image at 12µm (not shown).
Why infra-red at 12µm should provide information on low level water vapor?
-R: The referee is right that this band is not useful to detect low level water vapour, but rather surface temperature and moisture. Since the results are not much different to the water vapour band (6.5 µm channel) we opted to use the 12 µm channel to explore the soil moisture and saturation that could have played a role in the flash-flood. We have added figures from both bands (water vapour and infrarred) to the appendix and rewritten this paragraph in order make it clearer. We keep the visible band results in the manuscript's improved Fig.2.

20) 6, 8-9: We note that the models rainfall spatial distribution corresponds very well to the clouds locations in Fig. 2a-b). While there is a general agreement, I would not say that forecast and observation fits "very well". Can you provide any verification measure of the agreement between observations and forecasts?
-R: We hope the updated Fig. 2 will be able to show the agreement. A comment about verification measures was also added.

21) 6, 14-15: Thus WRF is able to simulate the event with its most important features. Not so sure, also because you do not have enough observations to describe in detail the event fea- tures.
-R: We have reformulated this sentence with the updated figures.

22) 6, 20: (with measured values of around 50 mm).
In 24 hours? What is the WRF forecast in the same period and location? Is 50 mm in one day an exceptional rain value? BTW, what is the rain climatology for that location and period of the year?

-R: We realize our precipitation analyse from raingauges was rather superficial. We have improved Fig. 3a and added information about rain climatology.

23) 6, 24: The analysis of the large scale characteristics and the few observations available provides insufficient information about the three basic ingredients for a thunderstorm: moisture, instability and lifting.
Maybe it is better to remove it, since it does not add any useful information.
-R: We have replaced this sentence.

24) 6, 28: in order to explore the chronology of the precipitation. “chronology” is appropriate?
-R: Perhaps "evolution" is more appropriate. We have changed this.

25) 7, 3: A closer look to the maximum radar reflectivity. A closer look to the simulated maximum radar reflectivity.
-R: Yes. Thank you for pointing this out.

26) 7, 6: even hailstones of around 5 mm are simulated at the centre of the two formed cells. Is it the Hailcast maximum diameter? Or the mean diameter? How it compares with observed hailstone diameters (e.g. from media report) and with the locations were hail was reported? Why not showing also a full map of hail diameters as simulated by Hailcast?
-R: This information is indeed missing. HAILCAST provides the mean hailstones diameter on surface alongside with their standard deviation. The hailstones diameters over this region are relatively small (personal communication with SENAMHI). We have added a sentence about how simulated and observed hailstones sizes compare.

27) 7, 11: comes from the Amazon avoiding the cordillera obstacle “avoiding” is the appropriate term?
Figure 5a-c: Reference vectors are 50 [kg m−1s−1] ?
Figure 5d-f: Reference vectors are 3 [ms−1]?
-R: We have replaced the term and updated the figure captions.

28) 7, 29: in the proximity if the rain-band. in the proximity of the rain-band
-R: Corrected now.

29) 8, 2: the atmosphere is saturated until 400 hPa with important wind shear favoring hail and graupel formation. Can you add a reference to support the hypothesis that wind shear favor hail and graupel formation? In figure 6f I can see a strong directional shear at about 500 hPa, but for example also in figure 6e there is a strong directional shear at about 550 hPa, even if wind directions are completely differ- ent. So, the important issue was really the wind shear or rather the wind direction and intensity absolute values?
-R: This comment is pertinent and reinforces the need of an extended literature review in other regions. We have added further references to the introduction section and used it here to argument the updated discussion.

30) 8, 7: and wind shear from surface to 6000 magl (Fig. 7a-c)
How you define wind shear? Is it the “bulk shear”? I.e. taking the magnitude of the vectorial difference between wind at 6 km and wind at 10 m? Please explain.
-R: It is indeed the vectorial difference between wind at 6km and 10 m. The definition is now included.

31) 8, 13: Nevertheless, convergence is not enough to explain deep convection. Is that a scientific explanation?
9, 9: showing that breeze and orographic lifting are enough for producing rainfall. Is that a scientific explanation?
9, 17: leaving the cordillera storm free with isolated hailstorms. Sorry, can you rephrase?
10, 6: following the thermo-topographic circulation. Can you explain better?
10, 13-14: This propagation allowed both cells to join each other resulting in a precipitation band. This auto-propagation mechanism has been observed Please, can you explain better?

-R: We recognize the referee's concerns in this section. We may have been overly confident in our figures and we realize a stronger argumentation has to be made. We hope the revisions made to this section will meet the journal standards and better convey our findings to the readers.

32) 10, 17-21: The presence of sufficient wind shear. . . as shown in Fig. 7f.
This part seems not much relevant with the experiments described. I suggest to remove it or to move it to Section 3.
-R: We have moved this part to section 3.

37) 11, 5-6: satellite information and reanalysis suggests that this severe event was in fact part of a mesoscale convective system. That option was never analyzed nor mentioned before the Conclusions. Please remove it or discuss it with supporting facts already in the previous sections.
-R: The referee is correct that the mesoscale convective system nature was not analysed and we have accepted his advice to remove it.

38) 11, 20-21: And the surface heat flux suppression (NOHEAT) highlights the importance of surface energy fluxes for atmospheric instability. Isn't it a trivial result?
-R: It is indeed trivial, and doesn't highlight the main result that even without heat fluxes, WRF is able to simulate convection by orographic influences (Fig. 10 d). We have modified this part.

39) 11, 22-23: highlights the complex interaction between large scale circulation, orography and local features in the formation of hailstorms over the tropical Altiplano. Sincerely, I have not found any specific information that can explain why a hailstorm was formed, instead than a thunderstorm (or supercell) not particularly characterized by hail.
-R: This is true and we have therefore modified the title and the text within this manuscript.

40) 11, 23-24: A semi-comprehensive scheme of participating mechanisms can be found in Fig. 11. Please, explain how figure 11 describes this (participating?) mechanisms in details or remove this figure.
-R: We have added a description of figure 11.

41) 11, 29-30: the proposed mechanisms of this hailstorm formation should be confirmed by high resolution observations. You already said that these observations are not available.
-R: We concede observations are not currently available and our intention was to stress the need of an appropriate observation system that would be able to confirm our findings. Maybe not in the near future, but we hope this study will stimulate decision makers to take a step towards a modernization of the Bolivian weather observation system.

REFERENCES

Rosas, G., Gubler, S., Oria, C., Acuña, D., Ávalos, G., Begert, M., Castillo, E., Croci-Maspoli, M., Cubas, F., Dapozzo, M. and Díaz, A., 2016. Towards implementing climate services in Peru–The project CLIMANDES. Climate Services, 4, pp.30-41.