Reply to Referee 3 in respect to his Review „Characteristics of a hailstorm over the Andean La Paz Valley“ by Marcelo Zamuriano et al.

The manuscript examines a severe convective storm event associated with large hail and heavy rainfall that occurred in February 2002 over the La Paz region in South America. Due to the scarce availability of suitable observational data, WRF simulations complemented by sensitivity studies with different model setups were conducted. The main objective of the study is to identify the mechanisms and processes most relevant for the triggering and maintenance of the convective storms. The topic of the paper is basically relevant for NHESS. However, there are a number of important issues that have to be considered and addressed before the paper can be published. Above all, an in-depth discussion of all figures, the findings and the most important results is necessary. All my suggestions and comments are listed below as major, minor, and (a few) editorial points.

We are thankful to the referee for his/her valuable comments and suggestions. We have gladly taken this review into account to improve the manuscript. The referee's points are followed by our replies starting with an R:

Major revision points:

1) The results and illustrations are mostly discussed very briefly, superficially and descriptively. A thorough, in-depth discussion and interpretation is lacking. This applies in particular to the sensitivity studies and to the application of the Hailcast model. In order to use them in a sensible way, a much deeper discussion and interpretation of the results is necessary. Furthermore, the interconnection of all subsections has to be improved.
-R: After having re-read our original manuscript, we are aware that our discussion section can be vastly improved and solid arguments are still lacking. We hope our revised version can overcome this issue in particular with respect to the observations from rain-gauges, the HAILCAST model, the discussion section and sensitivity studies.

2) Please explain in which way the result can be generalized and how the findings may apply to comparable events.
-R: We have added this part to the conclusions section.

3) I do not agree that the model capture well the main features of the severe thunderstorms. There are large discrepancies in both the location and precipitation intensity. Even the two major convective cells are not well reproduced by the model. The conclusion (Sect. 3.1.2) that the model is able to simulate the main features and, thus, capture the mechanisms decisive for the triggering and maintenance of the storms is not justified.
-R: We agree with the referee that our arguments in the original manuscript are not very well elaborated. Further analysis and a deeper look to rain-gauges observations even suggest that WRF underestimated the intensity of the event. However, because it is hard to quantify by how much given the observations limitations we didn't develop more. The lack of studies of this nature on the region makes also hard to contrast our values with similar events. We have updated the manuscript in order to convey better our main findings.

4) The effect of a lake breeze that is relevant for convection triggering cannot be derived solely from the wind field, but requires analyses of temperature / moisture gradient or vertical lifting. As sea/lake breezes have a limited vertical extent of a few hundred meters, they cannot be detected at 500 hPa, not even over the elevated terrain around Lake Titicaca.
-R: We realize we have mixed lake breeze (using surface wind) and 500hPa wind circulation without explaining the reasoning behind it. The main point of our original sentence was to relate surface to low level circulation (500 hPa) for the purpose of introducing wind shear and to relate it to the moisture suppression from the Amazon discussed during the synoptic analysis. We have conducted revisions to elaborate better this section.

5) The discussion Section (together with the Conclusion Section) is more a summary than really a discussion (this applies at least for the first half of the text). I'd like to see a more thorough discussion and a better synthesis of the results instead of repeating what was already written. I also suggest to extend the comparison with other related studies and references.
-R: As stated in our reply to comment 3, it is hard to contrast with similar studies in the central Andes because of the lack of them. Nevertheless, our findings can be contrasted with similar studies in other regions. We have added a literature review about similar events in other regios to our introduction and updated the discussion section accordingly. We hope the revised manuscript reflect the improvements with better synthesis of the results.

6) Be accurate in your citations!
Sect. 2.1.2: I'm puzzled about the statement "known uncertainties of precipitation estimates over complex terrain (Rasmussen et al., 2013)" as this does not make sense for the TRMM algorithm. The cited reference investigated the range of the rain bias in storms containing four different types of convection in extreme radar echoes over South America, but did not investigate a relation between bias and terrain characteristics. The study of Kunz et al., 2018 investigates a supercell over low-mountain ranges and not over the Alps.
These are just two examples that I've checked.
-R: We have taken more care to our citations in the revised manuscript.

7) The number of cited references is rather small.
-R: As discussed in our reply to comment 5, we have expanded our literature revision and added further references. Also the discussion section expands the references used.

8) Most of the Figures are too small and/or have a too low resolution; it's hardly possible to see any details. Furthermore, as most of the readers are not familiar with the study area, anadditional figure that indicates all areas and cities referenced in the text (Altiplano, Amazonas, ...) would be helpful (Fig. 3a is too small!).
-R: This was a recurrent problem for all referees. We agree that the figures can be hard to see and we have updated them in our revised manuscript.

9) The English language requires a thorough check by a native speaker. I have listed a few corrections, but these are too numerous to fully list.
-R: The referee is correct to point out the English errors. We are following your advise and making the revised manuscript checked by a native speaker.

Minor points
1) The title should be changed as the paper goes beyond the investigation of one single hailstorm.
-R: We have considered that a title that better summarize the outcome of the research would be: "Numerical Insights of a Severe Convective Storm accompanied by Hail and Flash-flooding over the Andean La Paz Valley"

2) Abstract: Can you highlight the most important findings (e.g., We show the importance of orographic configuration... is too general; be more specific). Consider to move the last sentence after the 2nd one.
-R: We agree the abstract is too general and it has been updated to include more specific findings.

3) The use of LST is weird: Twelve noon local solar time (LST) is defined as when the sun is highest in the sky, i.e., it is a function of the geographical coordinates. Do you mean local time instead (LT)? What is LT in UTC at La Paz?
-R: Since we use official local times, we have replaced LST by LT. We have also introduced the equivalence between LT and UTC (LT = -4 UTC) in the manuscript.

4) Introduction, 1st paragraph: Give a brief reason why hail hazard assessment is not available. But also note that there are some studies available that uses either satellite data (e.g., Cecil and Blankenship 2012) or reanalysis (e.g., Prein and Holland 2018) to estimate hail frequency, that may be mentioned (e.g., by giving an estimate how frequent the region is affected by SCSs, similar events in the past, ...).

-R: We agree this needs further development and we have pointed out the observation limitations over the region in the updated introduction. We have additionally included further literature review in order to explore how this event can compare to similar ones in other regions.

5) Introduction, 2nd paragraph: give some more details about the event: total rainfall accumulation, hail sizes, wind gusts; what was the reason for the large number of casualties?
-R: We have included the details of rainfall accumulation. We also have expanded the introduction including the possible effects from hailstones over the flash flood that was responsible for the casualties. 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 have made sure to include this information in our manuscript.

6) P2L5: “The lack of formal physical process knowledge” is unclear: I think you mean what makes this region special related to thunderstorms compared to others? The physical processes for thunderstorm formation are the same all over the world.
-R: The referee is correct. This sentence has been rephrased.

7) Introduction, last paragraph: Say a few more words about the motivation and objectives of this study to be published. Only saying the goal is to better understand the processes is too simple. What are the research questions / hypotheses? What are the reasons for performing sensitivity studies?
-R: We have added a paragraph to the motivations and objectives, including the reasons for performing sensitivity studies.

8) P3L14: how many gauges? If all stations are shown in Fig. 3a, you cannot say “around 1 km distance” given the large spatial differences.
-R: We have expanded the stations network description alongside with the station network climatology in order to assess the severity of this event.

9) P3L16: “some data quality issues”: what do you mean?
-**R:** We have added some examples.

10) Sect. 2.2.1, 2nd paragraph: give some more details about the schemes and the configurations you have used (besides: write out YSU); also give a reference for the radar-forward operator you have used to create Fig. 4. Furthermore, you should motivate here - or maybe better in the introduction (see minor point 7)- the reasons why you have conducted the sensitivity experiments. What do you expect from those additional runs?
-R: We have modified the introduction in order to contain the sensitivity studies motivations, and we have added more details to the description of the schemes and configuration used.

11) 2.2.2: Why did you not consider deep layer shear (DLS) or storm-relative helicity SRH, which are important ingredients for supercells? The unit of IVTU/IVTV in 100 g (s m)-1 (yes, this is the result when using the units you stated) is very strange and not an SI unit. I strongly recommend to insert dp not in hPa, but in Pa. Why do you integrate until 200 hPa?
-**R:** The referee is correct. We thank you four this remark. Specific humidity units must be in kg kg^-1 and dp units should be in Pa, we have corrected this. We haven't used the SRH in our manuscript since the main goal was not to asses the supercellular nature of the event. However, and in agreement with the other referees, we accept it would be useful to discuss this parameter in order to compare our case event to storms in other regions. We have added a small discussion regarding SRH in the revised manuscript. Finally, we decided to update our IVT formula until 50 hPa since the integration should be made over the full column; the original calculations are not much different to the updated calculations since the Bolivian High position (Fig. 1a) regulates the upper levels flow coming from the west and this flow is dry in general.

12) Always put a blank between two units (e.g., ms-1 could be m/s or millisecond).
-**R:** Now is corrected.

13) How is CAPE computed? Mixed layer CAPE, most unstable CAPE, ...?
-R: We refer to the most unstable cape. We added this information.

14) P5L4-6: As you did not investigate runoff or flash floods, soil saturation is irrelevant. You may mention this in the introduction, but not in the result section.
-R: the referee is correct. We have moved this sentence to the introduction.

15) Sect. 3.1.1 / Fig. 1b: Showing only one isoline of the geopotential at 200 hPa (also without labelling) does not make sense as the flow at lower levels, which are most important for the triggering, could be completely different from that at 200 hPa.
-R: The 200 hPa level geopotential is generally used as proxy for moisture transport over the Altiplano (Garreau, 2003). We agree that a measure of the intensity (by labelling it) is lacking since its position and intensity results on the enhancement (southern position) or suppression (northern position) of moisture transport towards the Altiplano. We acknowledge that a configuration favoring the moisture was not shown and we decided to add such configuration in Fig. 1a.

16) P5L13-14: what do you mean by “northward displacement of the Bolivian High”?
-R: We have added the normal summer configuration of the Bolivian High in Fig. 1a (favoring moisture transport). We have modified this sentence in accordance to the figure update.

17) P5L18: specify what is meant by “mesoscale features”
-R: We added a sentence specifying such features.

18) Fig. 2: There is a discrepancy between blue shaded contours and the color bar at the bottom (besides: the numbers are illegible).
-R: Fig. 2 has been updated.

19) P5L24: “...show a remarkable spatial consistency...” this statement is too general and too optimistic; of course, there is some consistency (mainly at 17 LST), but also some discrepancies. This is what one would expect as you compare visible cloud areas with rainfall.
-R: We agree we may have been too optimistic here. We have rephrased this sentence in accordance to the updated Fig. 2

20) P5L25: “Titicaca lake, the Amazon region, and the eastern cordillera...” These regions / features should be indicated in one Figure.
-R: These features are indicated in Fig. 3a. However, we concede it is hard to see and we therefore have moved and updated it into Fig. 1.

21) P5L28-30: “important convection...shallow convection”. From the visible channel solely, you cannot distinguish the intensity of convection; “TRIMM is not able to capture any light rainfall”. Why do you suppose that rainfall already started at that time?
-R: We realize Fig. 2 alone do not provide enough evidence for sustaining these claims. So we decided to rewrite this sentence with a newer version of Fig. 2

22) P5L7: Why do you consider longwave radiation here? Explain and motivate this (and recall that Figs. 2a-d show the visible channel).
-R: Past studies (Sicart et al. 2015 and Sulca et al. 2018, among others) have shown the usability of OLR (outgoing longwave radiation) for cloudiness detection over this region. However, its usability is limited since it contains information about most longwave spectrum and it's hard to discriminate low level vapor to cloudiness, especially during the morning. We discuss this part a bit more in the revisions.

23) P5L10: The Figures show radiation and precipitation, and not water vapor as stated here.
-R: The referee is right. We have rewritten this sentence.

24) P6L25: Also consider vertical wind shear.
-R: We have considered it in the results section and we have revised the methods section to make them better interconnected.

25) Sect. 3.2.1, 1st paragraph: So what? What can you conclude from the Hovmoeller plot?
-R: We present more arguments before jumping to conclusions in our revised manuscript.

26) Sect. 3.2.1, 2st paragraph (see also major comment 4): The strong north-westerly flow east of the lake between 11 and 13 LST can also originate from the cold pool of the convective cell. This may also explain the large flow divergence at 13:00 around that cell. Note that sea/lake breezes have a rather small vertical extent so that the associated wind field is of minor importance for the movement of the convective cells.
-R: The referee makes an interesting statement about the enhancement of the lake breeze by early convective activity over the lake. We have included further comments on this feature on the revisions.

27) Fig. 5a-c: the color code of the figures and the color bar do not match; what are the areas indicated by the brown color?
-R: Brown area is orography. We have replaced it with dashed contour lines indicating the 4000m orography line.

28) P7L10: “While the surface humidity follows the lake breeze...” I cannot see any relation.
-R: We have modified the figures and the text accordingly to better argument the relationship between lake breeze and humidity transport.

29) Sect. 3.2.2, 2nd paragraph (see comment 26): sea / lake breezes typically have a vertical extent of a few hundred meters. They cannot be identified at 500 hPa. Even though at 14:00 there seems to be a frontal boundary involved, which was not the case at 11:00 and even at 12:30. In the previous paragraph, however, you suggested the wind field even at 11 and 12 is associated with the lake breeze. Furthermore, I’d suggest not using different times for the plots (12:30 in Figs. 5-6 is not shown by Fig. 4). Finally, relate the fields to the convective systems.
-R: Major comment 4 also addresses this misunderstanding. We realize we have mixed lake breeze (using surface wind) and 500hPa wind circulation without explaining well the reason within. So we have rewritten this part to make ir clearer. We also recognize the difference of times for Figs. 5-6 not shown in Fig. 4 and we have decided to modify Fig. 4 to make them compatible. In addition, we added some comments between the fields and convective systems relationship.

30) Fig. 6: It’s hard to see the CAPE. I suggest to include additional Figures showing only CAPE. Furthermore, it’s not clear what Figs. 6a-c show.
-R: We have modified Fig. 6a-c and the text accordingly.

31) P7L24-26: It’s rather confusing to term that sensible heat is released as the Figures show the vertical heat flux; Solar radiation is surely not the only reason for increases in CAPE (cf: the largest increase in CAPE in the northeast is associated with lowest sensible heat flux.)
-R: We agree a deeper discussion on sensible heat and CAPE is needed here and we have subsequently modified this part.

32) P7L30: As the city heat-island effect is not relevant for the convective storms, I suggest to omit this statement.
-R: We have rewritten this statement.

33) Fig. 7/8: It would be easier for the reader to show shear as amount and not as vector.
-R: We have modified Fig. 7/8 to show shear in a more readable manner.

34) Sect. 3.2.4: In some places reference is made to convection triggering. However, convection on that day is triggered earlier as shown in Fig. 4. Be sure what is really meant here: maintenance, triggering of new cells, or – as you describe – the merging of scattered convection to a larger band showing some basic features of a squall line.
-**R:** We have followed the referee's suggestion and modified the text correspondingly.

35) P8L12-14: Explain where the cold pool is located (I cannot see a convergence line; besides, the cold pool cannot be situated over the convergence line). It’s not possible to speculate about a supercooled state without showing hydrometeors.

-R: We have modified Figs. 7/9 to point the cold pool locations and we have added further discussion. the referee is right about the speculation about a supercooled state and we decided to modify this to “cold atmospheric environment” (above the freezing level)

36) P8L21: This explanation for a cold pool is wrong; rather evaporation and sublimation cooling by hydrometeors drives the cold pool.
-R: We have corrected this sentence

37) P8L28-29: Literature?
-R: We have based this claim on Fig. 8a-c. But we realize this sentence can be expanded with further arguments, which we have made.

38) P8L7: “convection is present without thermodynamic instability” this is a contradiction in itself.
-R: We have rephrased it.

39) P9L13: How is hail formation suppressed? Changes in microphysics, intensity/size of the updraft, or what else?
-R: We realize the mechanism is missing and we have modified the text to include plausible explanations.

40) Experiment with no lake: when you state that the wind field for this realization is similar to a lake breeze, than I doubt even more that really a lake breeze is responsible for CI.
-R: We have updated this section taking into account the additional analysis of lake breeze enhancement by early convective activity.

41) Caption Figure 10: Make clear that the smaller “hailstones” with a diameter of 4 mm are treated as hail in WRF only; according to WMO definition, hail has a minimum diameter of 5 mm.
-R: We have adopted your suggestion. We also added a comment about the hailstones diameter standard deviation simulated by WRF (same order as the mean diameter)

42) Discussion: All the above critic points likewise apply to the Discussion Section
-R: We accept this comment and we modified the discussion section accordingly.

43) P10L13: Cold pools behave as density currents. Thus, their propagation results from the interaction between mean flow and density current. They do not directly propagate with the mean wind. Furthermore, cold pools emerging between two cells leads to flow divergence, which prevents and not favors cell merging. The term “auto-propagation” is not appropriate here.
-R: We agree this term can be misleading in this context and we decided to rephrase the sentence.

44) P10L17-18: Low to moderate wind shear does not allow for substantial hail formation as it also affects the strength / width of the updraft (cf. Dennis and Kumjian, 2017 in JAS).
-R: We have included additional discussion about how our findings can be compared to other regions and we have included the wind shear parameter in it.

45) P10L24: The conclusion about a relation between instability and surface fluxes are not justified as the largest increases in CAPE occur in a region with lower fluxes (NE parts).
-R: We have expanded this discussion in order to relate CAPE to other parameters.

46) P10L26: The reduced temperature gradient was not shown.
-R: This is true. We include the values in our revisions.

47) Section 4, last paragraph: The question of a possible trend in the frequency of hailstorms is irrelevant for a single case study.
-R: We have moved this paragraph to the conclusion part in order to motivate further research.

48) Conclusions: The most important results should be more clearly identified and highlighted. Clearly show what is new, what the reader should have learned from the study.

-R: We agree with this comment. We hope the updated conclusions communicate better our findings.

49) P11L6: Usually an MCS is defined as an ensemble of thunderstorms that produce a contiguous precipitation area on the order of 100 km or more in horizontal scale in at least one direction (e.g., AMS glossary). This definition does not apply to the 19 Feb. storm.
-R: This is correct and we decided to remove the MCS part.

50) P11L6 (blocking): You haven't shown that blocking really occurred on that day. This, of course, would increase the substance of the paper.
-R: We agree with the referee in this part. We have consequently added further evidence for blocking in our revised manuscript.

51) P11L23-24: I suggest to move this part to the discussion section, but also to explain in detail Figure 11.
-R: We agree that Figure 11 should be detailed within the text and we have added this explanation. It has also been moved to the discussion section.

52) Typos / Small corrections
-R: We thank the referee for all the small corrections that we made sure were incorporated in the revisions. We also have checked again the manuscript with the help of a native speaker.

References:

Hardy S. 2009. Granizada e inundación del 19 de febrero de 2002. Un modelo de crisis para la aglomeración de La Paz. Bulletin de l'Institut français d'études andines:501–514.

Sicart JE, Espinoza JC, Quéno L, Medina M. 2015. Radiative properties of clouds over a tropical Bolivian glacier: seasonal variations and relationship with regional atmospheric circulation. International Journal of Climatology.

Sulca, J., Vuille, M., Silva, Y. and Takahashi, K., 2016. Teleconnections between the Peruvian central Andes and northeast Brazil during extreme rainfall events in austral summer. Journal of Hydrometeorology, 17(2), pp.499-515.