

Reply to Referee 1 regarding the Manuscript NHESS-2019-27: "Characteristics of a Hailstorm over the Andean La Paz Valley", by M. Zamuriano et al.

OVERVIEW

We thank the referee for the detailed revision of the manuscript that helped us improve its quality. We have taken note of the comments and we would like to follow them with our answers. We have enumerated the comments and accompanied them with our replies marked with R:

TITLE:

1) The title makes reference only to the hailstorm, but throughout the text the authors also mention/discuss the occurrence of a flash flood accompanying the same weather event. Therefore, the title should be modified, perhaps by reading "Characteristics of a Severe Convective Storm over...". The main point is that the authors give equal emphasis to the hail precipitation and to the flash flood (rainfall amount) in the text, but the title, as it is, does not reflect that.

- **R:** It is true that casualties during this hailstorm are related to the flash-flood accompanying this event. While we concentrate our research on the atmospheric characteristics of this hailstorm, we acknowledge the role of the flash-flood in this natural disaster and we therefore agree that the title can better reflect this. 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". This is not final and can be still changed in the revised manuscript.

FIGURES:

2) Most figures are appropriate for describing the results, but they are way too small, making it hard to read and to verify several of the important detailed information discussed in the text. It is true that the digital file allows for the zooming in of the figures, but this is rather cumbersome for the reviewer. If, for example, the authors keep one figure per page (in the submitted version) then the figures can be enlarged.

- **R:** We agree that the figures can be hard to read in its current form and we now include clearer, one per page, figures.

3) Captions can be improved and/or do not provide full information of the contents of the figures: Caption of Fig.1a must inform the horizontal grid spacing for each domain.

Caption of Fig.2 begins with "(a)-(d) Remote Sensing observations assessment" which could be replaced by "(a)-(d) GOES-8 visible imagery (grey shading) and TRMM estimated 3-hour accumulated precipitation (blue shading)..."

Caption of Fig.3: "accumulated" instead of "cumulated".

Caption of Fig.4: Should read: "...maximum simulated radar reflectivity in domain D4..."

Caption of Fig.4: Should read: "...blue contour encloses areas with simulated hailstones equal to or larger than 5 mm in diameter...", and must inform at what vertical level this is valid. Surface level?

Caption of Fig.6: Should read: "...most unstable CAPE..." instead of "...maximum CAPE..."

- **R:** Captions for Fig.1a now contain grid spacing for each domain; the proposed captions for Fig. 2, Fig.3 and Fig. 6 make sense and have been modified; and captions for Fig.4 include now the vertical level of the hailstones simulated by HAILCAST (surface level)

INTRODUCTION:

4) Page 1, line 24: "...between 1420 and 1545 LST [...] a hailstorm affected the city of La Paz." Please, inform the corresponding UTC times as well. Has the hail precipitation lasted for 1 hour and 25 minutes over La Paz? That would be highly unusual; a truly extreme event. Or was the accompanying flash flood that lasted for such a long period?

- **R:** Since our main findings are related to thermal daytime circulation, we only considered to include times in LST format to give an idea of the local thermal context. However, we agree that the use of UTC times can be useful for many readers and we include them in the revised manuscript. We also clarify that the precipitation duration was reported to last 1 hour 25 minutes with a peak including hail precipitation of about 20 minutes duration.

5) Page 2, line 11: "...generated a super-cell over the city (Sorucu, 2012). This explanation might sound trivial for a super-cell formation..." As for a supercell being responsible for the hailstorm, it

surprises me that the authors of this study run a fairly high resolution simulation of the convective storms with WRF but do not verify whether any of the simulated cells developed a mesocyclone. That could provide additional evidences for the supercellular nature of the storm(s). The authors should look for such evidences in the 2 km grid-spacing simulations through the analysis of convective updrafts correlated with (negative) vertical vorticity. More detailed comments on that matter follow below.

- **R:** We realize this paragraph in our manuscript is not very clear and does not convey our main message. We intended to point to the lack of knowledge of this kind of events by citing the super-cell explanation given by the SENAMHI without any formal evidence. This study doesn't aim to study the super-cellular nature of the storm and we purposely left the mesocyclones out of the analysis. However, this comment raises a very interesting question that can be addressed in further investigations.

DATA AND METHODS:

6) Page 2, line 25: "...a temporal resolution of 6 hours and a spatial resolution of around 0.75 x 0.75 lat-lon..." I am not sure that we can state that the temporal resolution of the ERA Interim is of 6 hours since we would need at least 2 "time-steps" (i.e., 12 hours in this case) to minimally resolve any atmospheric feature using this dataset. The same comment holds for the spatial "resolution". I suggest rephrasing by "...the gridded data is available at 6-hour intervals..." and by "...with horizontal grid spacing of 0.75° x 0.75° latitude-longitude..."

- **R:** The referee is right about ERA-interim and the terms used in our manuscript. We have put effort to rephrase this paragraph.

7) Page 2, line 26: "...geopotential fields at 200 hPa, and specific humidity and winds at 500 hPa..." The authors extract 200hPa geopotential fields from ERA Interim but never show these fields explicitly. It must be indicated what is the above-ground height of the 500 hPa pressure level over La Paz. This is important because, at first, it sounds strange to analyze the 500 hPa humidity fields when we should be mostly interested in the analysis of the low-level moisture (below 3000 m AGL). It turns out, however, that La Paz is situated in very high terrain and therefore the 500 hPa fields may represent the (local) low-troposphere, which is unusual for most regions.

- **R:** We use the 200 hPa geopotential field for synoptic conditions analysis and it is included in Fig.1b. However, the referee is right that we do not show any values and we use it only qualitatively for identifying the position and intensity of the Bolivian High; the revised version adds a comment about the intensity of this field with the correspondent values. Concerning the 500hPa pressure level, the referee makes a fair point that this pressure level can look strange without stressing that it reflects low-level circulation in this particular region. We agree it is important and we do better justify the use of this pressure level in the revised manuscript.

8) Page 3, line 8: "...they provide area-wise estimates with a fair temporal resolution..." I would rather state more explicitly that the 3-hr sampling interval from the TRMM satellite, despite not being adequate for monitoring the evolution of a single severe convective storm, is the best available remote sensing data for this specific case study. The authors only utilized the rainfall estimation product from TRMM satellite. Given the severity of the storm, other products could have been analysed, such as the height of the 40dBZ radar reflectivity just as one example. South American hailstorms are known for being very tall, particularly in the La Plata Basin sector. Most readers will be curious about the depth of this cell in Bolivia; has TRMM sampled the storm at its mature stage?

- **R:** We have looked to satellite radar data (product 2A25 from TRMM) and unfortunately the radar missed the event (because of the trajectory offset), so we have added a sentence about this. We also agree that the nature of this event (very high altitude) raises some questions about how it can be compared with other regions, for example in relation of the cell depth; we are happy to add this information (from simulations) in the revised manuscript.

9) Page 3, line 14:

"...resolution network of rain gauges; the network is maintained by SENAMHI."

Is this an automated surface network? Or is it manned? This must be informed for the sake of completeness.

- **R:** This information is now included.

10) Page 3, line 25:

“...over the Bolivian central Andes D1, D2, D3 and D4 of 54, 18, 6 and 2 km of grid size...” I wonder if the D1 domain with 54 km horizontal grid spacing is really necessary when downscaling from ERA Interim. The downscale “leap” from ERA Interim directly to the 18 km grid spacing may have sufficed. Any comments on that? Please, provide the number of gridpoints (matrix size) of the 2 km mesh.

- **R:** It is true that an outer domain of 18 km may have sufficed for the simulations described in our manuscript. However, as a pre-test we contrasted results using ERA-Interim and FNL analysis (GFS based at 1x1 deg resolution) as initial and boundary conditions, and we designed a horizontal grid configuration compatible with both datasets (we considered a leap from 1 deg to 18 km grid size too big). We have kept this configuration because we considered the results were good enough and taking out the outer domain wouldn't change the main findings. If we were to propose an operational forecast configuration, we would take out the outer domain. We added a comment on this alongside with the number of gridpoints of the finer domain.

Page 3, line 30:

11) Misspelling: “The KAIN-Fritsch scheme....”

- **R:** Corrected

Page 4, lines 1-2:

12) “The initialisation time is fixed to 1400 LST on 17 February 2002, allowing enough spin-up time until the event.” First question: 14:00LST = 18:00UTC? I understand the authors' concern with the model's spin-up period but, in my experience and from several other numerical studies on convective storms, initializing the simulations 24-hr before the convective event usually suffices for that matter. Starting 48-hr in advance (as done here) may lead to too long a “forecast range” to produce the best possible simulation. Have the authors tested distinct initialization times for the simulations? If so, was the choice of utilizing the one starting 48-hr before the event justified for being the simulation with best correspondence with observations?

Finally, were all four domains initialized at the same time? These pieces of information should be informed.

- **R:** Yes, 14:00LST = 18:00UTC (we have proceeded as responded to comment 4). Also, we have tested different spin-up times initialized at the same time in all domains. We agree that we could have used a shorter spin-up, but we have found a good correspondence with satellite data with this configuration with respect to the location and timing of the main cells (Fig.2c,g). We do realize Figure 2 did not reflect that, so (as stated in comment 2) we have updated this figure.

Page 4, line 20:

13) The authors have available the output of a fairly high resolution WRF simulation (their domain D4) of the convective storms, but as “hailstorm diagnostics” they follow an ingredients-based approach (“We assess the presence of the main ingredients for a hailstorm to occur...”) for which having a high-resolution simulation is not indispensable. I recognize the importance of the ingredients-based approach, but additional diagnostics should have been chosen that explore the full explicit information made available by the high resolution simulations. Interestingly, in the Results section, the authors do show variables/fields such as simulated reflectivities, updraft strength, surface winds, and areas enclosed by hailstones surpassing a given diameter threshold, but none of these variables/fields is mentioned in the methodology as a diagnostic. The parameter “updraft helicity”, computed around 3 km A.G.L., would be also a natural choice of diagnostic to verify if the simulated storm(s) displayed mesocyclones (i.e., if they behaved as supercells) in any given stage of its(their) development. At least, vertical velocities should be analyzed in tandem with vertical vorticity in order to assess the presence (or the lack thereof) of mesocyclones. Surface winds/outflow produced by the simulated storms are shown in the Results section but could be better utilized by the authors when assessing the storms' severity. Finally, the presence of moderate to strong vertical wind shear is among the typical ingredients for severe convective storms, but the authors do not include any parameter for vertical wind shear in this section, despite discussing this parameter in the Results section.

- **R:** We acknowledge that this section must be updated with the variables used in the Results section and it has been done. The non-use of the updraft helicity parameter was discussed in the reply to comment 5. We agree this parameter could be interesting for future research. In the meantime, we propose to discuss the values that we found in order to assess the severity of the storm, since its location has good spatial correspondence to the updrafts position in Fig.7c. We

also have updated this section for a better introduction of the wind shear and the rest of parameters used in the results section.

RESULTS:

14) Page 5, lines 9-10: "...the well known anticyclone at 200 hPa (also called Bolivian High) was located over the north-east part of Bolivia (Fig. 1b)."

How the Bolivian High was characterized? The authors do not show the 200 hPa geopotential heights in Fig.1b. To a large extent, the Bolivian High is a - R to the intense convective activity (latent heating) observed over central South America during the warm season, so it is as much a consequence from deep convection than the cause for it. The discussion in Section 3.1.1 indicates that the Bolivian High drives/influences the convective activity but does not stress the important feedback from the convection itself.

- **R:** This is correct. But we have decided to focus in our study mainly on the influence of the Bolivian High position and intensity on the enhancement (southern position) or suppression (northern position) of moisture transport towards the Altiplano and less on the feedback. The referee makes a fair point that we could discuss the feedback from the convection itself but that would be outside the scope of this manuscript, which has been updated in the introduction section. Fig.1b is improved in the revised version.

15) Page 5, lines 15-16: "We find a considerable amount of water vapour over the Bolivian Altiplano due to the continuous precipitation episodes registered during precedent weeks." Shouldn't the presence of water vapour over the Bolivian Altiplano be the cause for the precipitation events rather than a consequence from it? If the Amazon Basin was not the moisture source for the Bolivian Altiplano (as stated by the authors in lines 11-12 of page 5), what was the effective moisture source? I know the authors discuss this matter in more details later on in the text, but my point here is that the general perspective provided by Fig.1b alone does not convince the reader that the Amazon Basin was not a moisture source for the Bolivian Altiplano. Fig.1b also suggests the presence of the South Atlantic Convergence Zone; do the 850hPa fields (not shown) also depict that?

- **R:** We understand the referee's point about our text formulation. We concede we can not assert the moisture source merely from Fig.1b. We have reformulated this sentence.

16) Page 5, line 22: "Satellite images from GOES-8 describe the fast development of the hailstorm..." Figs.2a-d per se do not allow the identification of the hailstorm. Maybe an arrow could be superimposed to the image to indicate which cell is the hailstorm; or else the figure caption should inform that.

- **R:** Figure improved.

17) Page 5, lines 26-27: "...the presence of low level water vapour is not well captured in this band but it's corroborated with infra-red image at 12 μm (not shown)." I do not agree with this specific statement. The thermal infrared imagery at 12 μm is useful to detect clouds and storms with tops at distinct heights, but not to detect low-level water vapour. In fact, it is hard to detect low-level water vapour from the geostationary satellite imagery, with the most reasonable choice (with GOES 8) being at mid-levels utilizing the 6.48 μm channel ("water vapour channel"). As for the 12 μm channel, was the hailstorm exceptionally deep for the Altiplano region (as inferred from the brightness temperature)?

- **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 confirm 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 infrared) to the appendix and rewritten this paragraph in order make it clearer. We do keep the visible band results in the manuscript's improved Fig.2

18) Page 5, line 30 and page 6, line 1: "...with two important cells captured by TRMM at the east of lake Titicaca and surrounding La Paz city." Again, it is hard to identify these cells in Fig.2c. The authors should try to superimpose arrows to the satellite imagery to highlight the convective cells being of most interest.

- **R:** This suggestion has been appreciated and applied to Fig.2

19) Page 6, lines 1-2: Here the authors mix two verb tenses “northern cell was” and “southern cell is”. Please, choose one verb tense when describing the event and stick to it throughout the text.

- **R:** Thank you for pointing out this unfortunate mix. We took more care to the manuscript tenses all over the revised version.

20) Page 6, line 3: “At this point the infra-red images are almost the same as the visible channel (not shown).” I cannot understand what the authors mean by this statement. It is best to remove it since it is confusing and does not add relevant information.

- **R:** We realize this is confusing and this is related to the comment 17. Following our reply, we decided to show this in the Appendix and make this sentence clearer.

21) Page 6, lines 3-4: “...the convective cloud development arrives to its term during late afternoon (Fig. 2d).” I would suggest rephrasing to “...the demise of the convective activity occurred during the late afternoon (Fig.2d).”

- **R:** We are glad to use your suggestion, it improves the sentence.

22) Page 6, lines 7-8: “Morning is characterized by high water vapour content and disperse rainfall.” The simulated radiance from WRF does not inform “water vapour content”, but provides a simulated image from the thermal infrared band which is utilized to detect brightness temperatures from distinct surfaces and cloud tops, implying (in the case of clouds) the presence of hydrometeors and not simply water vapour.

- **R:** We are aware the outgoing long-wave radiation contains only the long-wave spectra and does not include exclusively information about water vapour content, but all the thermal radiation to some extent. Nonetheless, recent studies (Sicart et al. 2015 and Sulca et al. 2018, among others) have shown the usability of OLR for cloud cover analysis over this region. We have rephrased this sentence and added the references.

23) Page 6, lines 8-9: “...the model’s rainfall spatial distribution corresponds very well to the clouds locations in Fig. 2a-b over the Altiplano and cordillera, and less over the Amazon.” It seems clear to me that the simulation overestimated the cloud cover/rainfall to the east of Lake Titicaca and over the Altiplano and Serranias. Moreover, the strongest simulated cell at 0800LST (Fig.2e) was located south-southwest of the respective observed cell (Fig.2a). I generally do not expect the model to nail down the exact location and timing of the convective storms, but I do not agree with the statement that “the model’s rainfall spatial distribution corresponds very well to the clouds locations”; in fact, the misplacement of the strongest cell at the early stages of the weather episode may explain some of the surface features displayed in the following figures and should be stressed in the text.

- **R:** The referee makes a fair point regarding Fig.2a and Fig. 2e. We have updated Fig. 2 and rephrased this sentence in order to support better our findings.

24) Page 6, lines 9-10: “Early afternoon (Fig. 2g) shows important water vapour at the northern cordillera...” Again, Fig.2g does not show water vapour. If the authors wish to describe the behaviour of the atmospheric water vapour in the simulation then they must plot the simulated water vapour mixing ratio (or specific humidity), not the simulated outgoing long wave radiation.

- **R:** This comment follows comment 22) and we have updated this section to make it consistent to our reply to comment 22)

25) Page 6, lines 12-15: In this paragraph the authors jump into two conclusions without presenting solid arguments to back them up. First, that the hailstorm was mainly induced by mesoscale features, and, second, that the cordillera blocked the moisture flow from the Amazon. At this point they can only hypothesize these two aspects. The authors should first describe the WRF simulations in more details before presenting these conclusions.

- **R:** We might have been adventurous in our fast conclusions and we accept the referee's comment. We have therefore opted to better develop and explain the reasoning behind this conclusion.

26) Page 6, Section 3.1.3: I think the discussion in this Section is poor. First, there is no figure illustrating the analysis; second, the authors should provide more specific/detailed information instead of just stating “Some places registered no precipitation...” and “...station observations confirms that an important quantity of rainfall fell down close to complex orography...”. Around La Paz

there is more than one important orographic feature, so what exactly “complex orography” are we talking about? How did the WRF simulated rainfall compare to the observed rainfall from the rain gauges? How did the TRMM-estimated rainfall compare with the rain gauges? These are relevant information since TRMM was also used to evaluate the WRF simulations in the previous section. In line 17 it should read “accumulated precipitation”.

- **R:** As for comment 25, we might have considered Fig.3a self-explanatory and we didn't speculate too much about the orographic features' description and rain gauges information. We have taking this comment into account, have reformulated accordingly the section.

27) Page 6, lines 24-25: “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.” Actually, the limitation goes beyond the ingredients-based analysis: the pieces of information analyzed thus far could not provide any insight regarding the internal structure of the convective storms, especially in the absence of a local weather radar. This is a particularly relevant issue considering the study of a severe hailstorm. Therefore, a (good) high resolution numerical simulation is desirable to provide such an important insight.

- **R:** Indeed, the limitations for this kind of study are very well summarized by the referee and we have taken into account this comment by stressing these issues in our revised manuscript.

28) Page 6, line 28: Avoid starting this paragraph with “We therefore....”

- **R:** Yes, we see your point and we have changed this.

29) Page 7, line 3: “A closer look AT the maximum SIMULATED radar reflectivity...”

- **R:** Thank you, we have corrected this.

30) Page 7, lines 3-6: “...reveals late morning convection in places where lake and/or valley breeze encounter complex orography (Fig. 4a).” I do not think the simulated radar reflectivity alone provides this information. Perhaps the simulated surface winds combined with the simulated reflectivity can indicate that, but, still, it is hard to reach a conclusion from Fig.4 alone. To better characterize mesoscale fronts (such as breezes) it would be better to plot the divergence fields at the low levels and look for linearly-oriented convergence features. “Later on, the lake breeze becomes more intense and pushes the rain spots towards 5 the east (Fig. 4b- c).” Given the highly divergent pattern of the simulated winds over the Titicaca Lake associated with earlier convection (Figs. 4a-c), it is quite possible that the early convective activity over the lake produced an outflow that was channelled in between the Cordillera and the Northern Serrania; so it could be that the lake breeze was substantially enhanced by a convectively-induced outflow. It is interesting that the simulated reflectivities are not high for deep convective storms (Fig.4) despite the presence of hail. The authors make no mention to this finding. This result suggests that the heavy hailfall occurred because of high-terrain effect, that is, for the 0°C isotherm being very close to the ground at the elevated terrain of La Plaz. Or else, the WRF simulation underestimated the storms' severity.

- **R:** We were perhaps too intrepid to assert the convection locations from Fig.4 alone. We recognize that further analysis is needed to arrive to this claim and we decided to discuss the convection after analysing Fig.5. The referee also makes good observations regarding the possibly breeze enhancing by early convective activity over the lake, so we have added some comments on that. We also share the referee's interest to the “low” reflectivity values and we have made some comments about the freezing level (0 deg isotherm) in Page 8, line 15 and Page 11, line 14. We nonetheless agree with the referee that more can be said about these findings and we have underlined better this feature in the revisions.

31) Page 7, line 16: “The lake breeze front is accompanied by strong winds at 500hPa...” How was the lake breeze identified? It is unusual to utilize 500 hPa winds in tandem with surface winds to characterize a lake breeze.

- **R:** We realize we have mixed lake breeze (using surface wind) and 500hPa wind circulation. The main point of this is to start to relate surface to low level circulation for the purpose of introducing wind shear and to relate it to the moisture suppression from the Amazon discussed in comment 14. The revised manuscript formulates better this section.

32) Page 7, lines 18-20: “We observe at the same time an intensification of previous convergence zones around complex orography; with a propagation of the convergence areas from the previous

zones towards each other (Fig. 5e).” The magnitude and noisy character of the convergence-divergence patterns indicate that the “convergence zones” are all contaminated by ongoing deep convection in the simulation. So we are basically looking at the environment modified by the storms themselves instead of convergence as a pre-storm lifting mechanism. So I think the above analysis is a bit confusing regarding what exactly the authors wish to discuss. Terrain effects? Lake-induced circulations? The problem is that at this stage (Figs.5e-f) the mesoscale environment is already highly modified by the ongoing convection such that it is difficult to isolate the mesoscale forcing mechanisms based on divergence.

- **R:** The referee is right about the later divergence contamination that makes hard to isolate the forcing mechanisms. We have included additional discussion about these features

33) Page 7, lines 31-32: In the analysis of the skew-T diagrams I think the authors missed a few important features: the temperatures and dew-point temperatures are very low if we consider a typical environment for severe convective storms. Naturally, this is explained by the very high elevation of the local terrain, but, still, this point deserves to be stressed since most readers are not familiar with such environments. On the one hand this aspect does not preclude the generation of CAPE (as shown in Fig.6e), but on the other hand it does reduce significantly the precipitable water, which is not informed in the text. In fact, the simulated 2m-specific humidity was rather low over La Paz (Figs.5a-c). Given these points, it is intriguing that a significant flash flood was observed in La Paz, as confirmed by the videos. So this raises a few questions:

has the rainfall alone accounted for the observed flooding or has the melting of hailstones contributed to that occurrence? Or else, is there any indication that the WRF simulation has underestimated the moisture supply for that region? Perhaps, very steep terrain leading to fast surface runoff also accounted for that event? This is an important result and discussion because most forecasters (at least the ones working in lower terrain areas) probably would not cite flash flood as a main threat if looking at those simulated/forecast soundings.

I probably would not say that the thermodynamic profile in Fig.6a is stable (line 31), but approximately moist neutral. It would not be hard to become unstable even with just some little surface heating. By the way, did WRF underestimate the 2m-temperature?

- **R:** We acknowledge the importance of a low freezing level and we discuss it later in the manuscript. However, as stated in the reply to comment 30, we agree this has to be stressed more and we have modified the manuscript to reflect this. The referee also raises a very interesting point regarding the intensity of the rainfall and flash-flood occurrence. To our knowledge, as discussed by Hardy (2009), a mix of elements favored the flash flood (soil saturation by previous precipitation episodes, very steep terrain, and urban characteristics). The hail cumulation over the city center played a role in blocking the sewage system and directing the flows over the streets. We have added a small discussion about this. We also updated the interpretation of Fig.6. And finally, WRF underestimated the 2m temperature but not by much (15 C in WRF and 18 C in La Paz city)

34) Page 8, lines 6 and 8-9: “The location of this band overlaps the lake-valley breeze convergence zone.” & “...responsible lifting mechanisms identified until now are orography and lake-valley breeze convergence.” As mentioned earlier, I think that a closer analysis of the simulation suggests that the (simulated) lake breeze was enhanced by convectively-induced outflow from previous convection.

- **R:** We present a more insightful analysis following our reply to comment 30)

35) Page 8, lines 6-8: “The evolution of the intensity of vertical velocity at 4000 meters above ground level (magl) and wind shear from surface to 6000 magl (Fig. 7a-c) gives an idea about the severity of afternoon convection and resulting storm.” The authors do not develop the discussion here. Were the simulated vertical velocities intense? They do not appear particularly intense to me. Even at 2 km horizontal grid spacing, simulated severe storms can develop vertical velocities of the order of 10 ms⁻¹. Isn't it possible that WRF has underestimated storm severity in this case? Or maybe it is because large hail accumulation at very high terrain does not require strong updrafts typical of true supercells. Any information available regarding the observed size of the hailstones? Sometimes, at higher elevations, storms produce copious amounts of small hailstones, for which very strong updrafts are not required.

- **R:** The referee is right that we could improve the discussion here. Our results 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 on this subject. The lack of studies of

this nature on the region makes also difficult to relate our values to similar events. However, we think a small discussion can be fruitful and we have added it.

36) Section 3.3: I think that the discussion around the results from the sensitivity analysis was rather superficial and, therefore, mostly inconclusive.

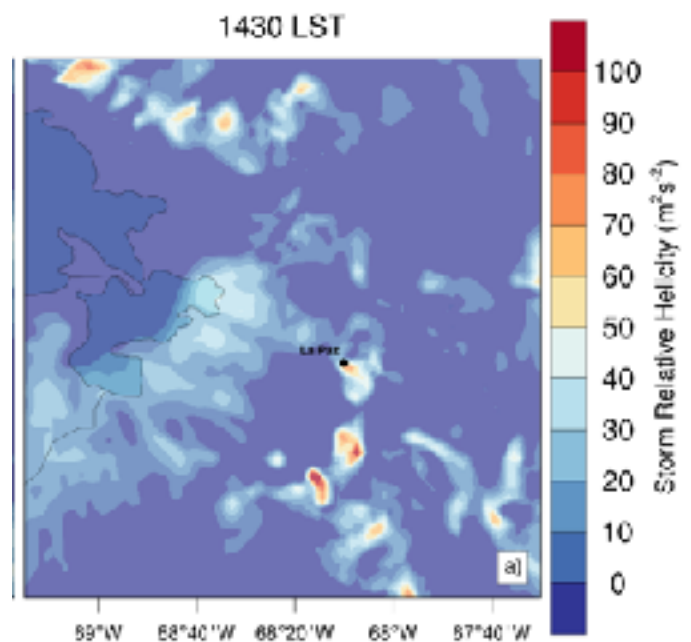
- **R:** As we have realized in previous sections, we agree our discussion can be improved and we hope the revised version reflects it.

37) Page 10, line 3: "On 19 February 2002, surface wind over the altiplano was guided by thermal lake, mountain and valley breeze effects." Again, I would say that the WRF control simulation suggests that outflow from previous convection also played a role by influencing the strength of the lake breeze (i.e., by enhancing it).

- **R:** We have revised this sentence following our reply to comments 30 and 34

38) Page 10, lines 17-18: "The presence of sufficient wind shear extends and supports the organization of convective storms in terms of multicells, supercells or mesoscale convective systems." In other words, no conclusion was reached regarding the convective mode. Unfortunately, the authors did not check for the possible development (or non-development) of mesocyclones within the storm cells simulated in their D4 domain. That would help identifying the convective mode (as for multicells versus supercells).

- **R:** We have addressed this issue in our reply to comment 13. We have also updated this sentence following the supplementary figure of storm relative helicity. It indeed shows several cells spatially consistent with the updrafts regions shown in Fig.7, but with values not high enough to be considered super-cells (more than $150 \text{ m}^2 \text{ s}^{-2}$, according to weather.gov). Further comments on this finding has been added.



39) Page 11, lines 5-6: "...suggests that this severe event was in fact part of a mesoscale convective system." Throughout the text the authors have not presented arguments to sustain such conclusion.

- **R:** We thank the referee for the very useful comments that we take into account and we hope the revised manuscript will be able to present better arguments to sustain our main conclusions.

References:

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