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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Synoptic and Mesoscale atmospheric features associated with an extreme Snowstorm over the Central Andes in August 2013" *by* Marcelo Zamuriano et al.

Anonymous Referee #1

Received and published: 6 November 2019

General Comments

The authors provide a detailed study of a snowstorm case affecting the South America Altiplano during August 23, 2013. The analysis is supported by MODIS, TRMM, and GOES Imagery, ERA-Interim products, WRF simulations (3 km inner domain) and surface observations. With this information, the authors aim to describe the synoptic and mesoscale forcing that led to significant snow accumulation over the Altiplano.

Studies addressing the mechanisms that lead to extreme events are highly relevant in the current context of a changing climate. The numerical experiments performed by the authors are relevant to understand the influence that surface features (mountains and

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lakes) have over the enhancement of snowstorms. However, the present study shows several flaws regarding meteorological concepts and methodology to support the hypotheses presented. Details of these flaws are presented in the specific comments along with some recommendations on how to alleviate them.

Specific Comments

1. Use of WV imagery for cloud cover interpretation (page 4, lines 7-8)

The authors use the water vapor band imagery from the GOES-13 satellite (central wavelength at 6.55 μm according to WMO) to assess the cloud cover during the storm. This band retrieves the brightness temperature associated with the presence of water vapor in the mid to upper troposphere. A direct interpretation of cloud cover is not advised because an undersaturated atmosphere will not show clouds, despite the presence of water vapor. For a direct interpretation of cloud cover, authors should use the Infrared band (10.7 μm) or the Red band (0.65μm). Indeed, the latter is named as "visible" by the authors (section 2.1.2), which is inaccurate since a visible image (not band) is composed by three bands (blue, red, and green).

2. Use of equivalent potential temperature (θe) gradient at 850-hPa to identify frontal zones (page 4, lines 20-24)

This is a common technique to identify frontal zones and seems reasonable to use for the purposes of this study. Figures 2 and 5 show θe fields while the authors expect the reader to identify the frontal zone they argue exist in the study domain. I strongly suggest one of these two approaches:

a) Use a gradient threshold to identify them just as is done in the study referenced by the authors (Schemm et al, 2018).

b) Modify the color map employed for θe such that the temperature gradient concentration (indicative of a frontal zone) is clearly identified (see Figure 1 of Sprenger et al, 2012)

An analysis of θe at 850-hPa will indicate frontal position around the Altiplano and not over it. Please clarify if this level is used to support the idea of cold air advection over the Altiplano.

3. Use of potential vorticity (PV) analysis does not seem enterally justified in the text (page 4, lines 25-26)

I would think that many NHESS readers could be unfamiliar with the PV concept and its interpretation. I strongly suggest expand this two-line paragraph by adding some explanation about the PV analysis, especially about its interpretation (e.g. cold air intrusions from the stratosphere).

4. Use of integrated water vapor transport over the Altiplano (page 4, line 27-28)

The Altiplano surface is at $\sim$600-hPa but the methodology does not mention if this is considered when calculating IVT from surface over the Altiplano. Please state this clearly since it would affect the IVT results.

5. Cold fronts characterized by sea level pressure (page 6 line 20-21)

The statement "cold fronts position (characterized by high sea level pressure)" is wrong. If any, cold fronts are characterized by a strong horizontal temperature gradients and wind shift (Schultz 2005 MWR , Schemm et al 2018). As a result, it is hard to make the connection between the plume of -2 PVU at upper levels and the presence of a cold front near the surface, as the authors suggest.

6. IVT transport over the Altiplano (page 6, lines 25-30)

As mentioned in the comment n°4, a clarification of the IVT methodology is needed. The IVT analysis is suggesting transport from lower (Amazon) to higher (Altiplano) lands. Nevertheless, there is a significant altitude difference and therefore is hard to make a clear interpretation of the moisture transport in this context. As a counter example, IVT analysis have a straightforward interpretation over the ocean because is the same depth of atmosphere that contributes to the flux. Furthermore, it is hard

to distinguish which component is leading to strong moisture flux over the Altiplano: strong high-level winds? Or high-moisture content? The analyses provided in the study does not allow to a clear distinction between these components. I think that providing an analysis of the water vapor column anomaly could alleviate the IVT interpretation ambiguity.

7. WRF simulation timing assessment (page 7, line 21)

Although the authors claim that the WRF simulation was capable of reproducing the timing of the storm, no time series analysis is provided. I suggest either add a time series analysis or remove this statement.

8. Moisture advection along Andean valleys (page 8, lines 18-19)

The authors state “moisture is advected towards the Altiplano along intra-andean valleys”. Although this seems a plausible hypothesis, no evidence is presented to support it. If Figure 8 is intended to be used for this purpose, the moisture flux associated with the valleys should be clearly portrayed, which is not the case in the current Figure 8. Even more, later the authors state that “IVT appears to be unaffected by orography” and that “synoptic transport is dominant”, which seems contradictory with an intra- andean valley transport, where orography would be important. Besides, comment n°4 and 6 also hold for this statement.

9. Atmospheric water vapor content is high (page 8, lines 19-20)

Figure 8b-c shows relative humidity, which is not an indicator of absolute moisture or water vapor content. The atmosphere could have low moisture content and be saturated. Therefore, the statement “the atmospheric water vapor content is high” cannot be supported with Figure 8. In addition, GOES Imagery provides the moisture content at certain level and not over the full atmospheric column. There are other satellite products that provide this information . In addition, authors should clearly distinguish through the paper if they are referring to the moisture content of a specific

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layer or in the atmospheric column.

10. Cloud cover in August 25 (page 8, line 33)

Same as previous comments, GOES water vapor imagery is not directly indicative of cloud cover.

11. Extreme amount IVT resulted in heavy snowfall (page 10, line 31)

From the analyses presented it is not clear that this is the case. Even if the IVT is indicating transport from the north towards the Altiplano, it is unclear if the absolute moisture is anomalously high, or if the Altiplano was anomalously cold.

12. Prediction purposes (page 12, lines 17-18)

If it is unknown the extent to which the current results can be transferred to similar events, what is the goal of transferring the model's configuration to the SENAMHI for prediction?

Technical corrections

Lines 14-15 on page 3 are not necessary.

Page 3 line 20 should read: "dataset produced by".

URL on page 3 line 22 should be moved to a foot note.

Page 3 line 26, the reference of SENAMHI should follow same pattern as the other references: SENAMHI (year).

Page 4 lines 7-8 should belong to the previous paragraph.

Page 4 line 16, please rephrase "Additionally, . . ." to clarify its meaning.

Page 5 lines 8-9, move the two-line paragraph to section 2.2.2 since this correspond to a WRF analysis.

Page 5 lines 16-19, move the extra content to Table 1. In other words, these lines are

not necessary since you have Table 1. This table only needs to be updated with the extra information contained in the abovementioned lines.

Page 5 line 28, replace “lake it by” for “lake for”.

Page 6 line 20, use K instead of deg C.

Page 10 line 5, remove period in “24. August”.

Page 11 lines 1-2, the clause “While. . .” is incomplete.

Page 13 line 28, first author is duplicated.

Page 13 lines 32-33, reference is duplicated.

Page 13 line 35, add title of the reference.

Page 15 line 7, URL is duplicated

Page 15 lines 28-30, authors and URL are duplicated

In all corresponding figures need to mask the Altiplano region for analysis at 850-hPa, as in Sprenger et al. (2012).

Use brighter colors in Figures 2 and 5 to clearly identify contours, arrows, and annotations.

Use larger fonts in all figures.

Provide larger figures (e.g. page width) so it is easier to distinguish thin lines (e.g country limits).

Need to add the meaning of the dashed line in Figure 2 and 4. I presume is the contour of Altiplano at certain altitude.

Indicate the data source in each figure. For example, it is unclear from the caption in Figure 2 if these are GFS or ERA-Interim data.

Figure 4 need larger annotations for A1 and A2. Also, panels e) and f) need an annotation to indicate to which site correspond each.

Figure 5 need date and time in panel c). In addition, need the timestamp in all analyses (review previous and following figures) unless they are daily composites, which should be stated in the methodology and figure's caption.

Figure 11, why using curly vectors? If no physical explanation is provided then authors should use regular vectors.

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