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Climate of the Past Discussions

Interactive comment on "Dust record in an ice core from tropical Andes (Nevado Illimani – Bolivia), potential for climate variability analyses in the Amazon basin" *by* Filipe Gaudie Ley Lindau et al.

Anonymous Referee #3

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This paper seeks to investigate the relationship between coarse dust particles and climate variability. In essence, the authors argue that periods of stronger glacier retreat lead to increased exposure of glacial deposits that provide for increased coarse particle deposition on the ice. The paper is interesting and could eventually make a significant contribution to our understanding of the dust and its climatic significance in tropical ice cores. There are, however, several aspects in the applied methodology that are flawed and need to be changed to make sure that their reported results are indeed robust. I have outlined my major concerns and a few smaller suggested edits below.

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Major concerns

1) I am concerned about the way the correlation with the SST field was performed. This analysis should not be based on annual means (calendar years). Doing this will lead to biased results as it cuts the Illimani wet season artificially into two separate years. Similarly, ENSO is phase-locked to the seasonal cycle and you will lose the strong ENSO signal if the data are averaged over calendar years. The correlations with SST need to be performed separately for wet and dry season or at the very least you need to define a hydrologic year (e.g. July-June).

2) Another problem is that apparently time series were not detrended prior to correlation analysis. Many of the records used, are characterized by strong trends, which must be removed prior to such an analysis. The SST in the tropical N. Atlantic and in the Nino 4 region, for example, have warmed significantly over the 2nd half of the 20th century (Fig. 5 and Fig. S3). The CPPn record also shows a strong upward trend. Hence you will obtain a significant correlation between these records, simply because they show similar long-term trends. But these are spurious correlations that are simply an artifact of the trends themselves; they do not imply that the records are casually connected or significantly correlated on a yearly basis. This analysis therefore needs to be performed on detrended series. The same comment also applies when correlating CPPn with precipitation records from nearby stations, with 500 hPa specific humidity (Fig. 4) or with the FLH (Fig. 7). If there is indeed a mechanistic link between all these records, the correlations will still be significant after removal of the trends.

3) For some variables, a 3-yr running mean filter was applied prior to calculating the correlation coefficient. How was the resulting reduction in the degrees of freedom taken into account? This should be discussed in the methods section.

4) The choice of the Nino 4 index to represent the Pacific connection with Illimani requires better justification. As shown by Francou et al. (2003), the mass balance in the Cordillera Real is much more closely related SSTA in the eastern Pacific (Nino1+2

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or Nino3 region).

4) The discussion of the FLH changes are interesting. But I would encourage the authors to compare the results to other recent studies that have also looked at FLH changes (and related ELA changes) nearby, notably on Quelccaya (Yarleque et al., 2018) and on Zongo glacier (Vuille et al., 2018). This would provide for additional confirmation, as the FLH is a large scale tropospheric change that should be consistent across all three sites, but varies depending on the reanalysis product used.

5) More rigor is required in the analysis of Figures 6. It is not sufficient to simply show a sequence of aerial photographs. Since you digitally analyzed their extent in each image, this change from one time period to the next should be highlighted in color contours to make the changes visible.

6) In addition, the methodology to delineate the glacial extent needs to be discussed in more detail and an error assessment needs to be performed, considering uncertainties associated with the hand digitizing of the glacier extent, the spatial resolution of the digitized aerial photographs and the resolution and vertical accuracy of the underlying DEM. The glacier extent in Figure 7 cannot be indicated by a single point, but needs to include error bars.

7) The retreat of glacier 8 in Figure 7 is pinned on just one data point in 2009. It would be helpful to have a few more data points between 1983 and 2009 to reduce the uncertainty in this response.

8) Figure caption 7: You state "The thicker line represents the linear increasing trend (r2 = 0.5)." What does that mean? Trends are not characterized by coefficients of determination (rˆ2), but by their slope and testing whether the slope is significantly different from zero (e.g. with an F-test).

9) In the discussion of the relationship between temperature on Illimani and glacier retreat, the argument is made that they tend to coincide. This may indeed be the case,

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but in general glacier retreat is considered a delayed response, which, depending on glacier size and dynamics, may not express itself until a decade or two after the warming started. A bit more discussion on the anticipated response time of this particular glacier to such forcings would be helpful in this context.

Minor issues:

Line 17 and throughout entire paper: The Andes are usually considered to be plural, hence it should be 'The tropical Andes have'.

Line 61: down to bedrock

Line 98: from 1941

Line 141 replace 'finishes' with 'ends'

Line 173-174: You write that 'Tropical Pacific SST anomalies are associated with anomalous upper-level westerlies over the southern tropical Andes'. Note that this applies only to periods with anomalously warm tropical Pacific SST anomalies (e.g. see (Garreaud et al. 2003).

References cited in review

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