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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 #2

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In this manuscript, Gaudie and colleagues investigate the link between dust deposition on the Illimani glacier in Bolivia, and climate change and its associated glacier retreat in the past 100 years. They improve on previous studies that measured dust concentrations by also analyzing size distribution. My main criticism is that the authors base all their conclusions on the correlation of the coarse dust particle fraction with various other variables. Science is not about finding correlations between variables, but about explaining dynamical links when correlation occurs. This manuscript lacks source attribution, trajectory modeling, and other supporting evidence that would substantiate

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Discussion paper

the conclusions. In addition, the scientific novelty is poor, as many of the conclusions could be reached (and were reached in other studies) without the dust contribution. For these reasons I suggest to reject this manuscript at this point. I would recommend to the authors to split the dust and satellite sections into two separate papers and improve these by adding more substantial analyses than just correlation studies.

Major comments:

The authors split the size distribution into a CPP (10-20um) fraction and argue that local dust sources will increase the CPPn fraction. This could be the case, but it is not shown. As a counter example, one could have a regional source with mode around 10um and a local source with a mode around 40 um. In that case, all CPPn variability would be due to emission and transport changes for the regional source. I suggest to show the whole range of size distribution measured by the Multisizer 4 up to 60 um and discuss potential local and regional sources based on that total distribution.

Figure 4: I put these remarks in the major comments as the interpretation of this figure is central for the manuscript. In line 154-155 the authors say that the correlation between CPPn and the specific humidity “suggests lower regional precipitation and reduced moisture transport from the Amazon basin to the southern tropical Andes”. That is a very strange way to interpret a correlation. Reduced regional precipitation could easily be shown using the ERA5 data instead of that correlation plot. In addition, there is no justification for the reasoning behind the reduced moisture transport from the Amazon. It could just as well be that both CPPn and specific humidity react similarly to temperature changes. A simple correlation between these two variables is not enough to justify that causal link. Finally, I will note that the paper by Segura et al. that is cited at the end of that sentence only talks about DJF precipitation, while the precipitation over the study cite is “distributed over 9 months” according to line 45.

In line 182 the authors state “From 1983 to 2009, we estimated a height reduction of -13 m and a negative mass balance of -11.8 ± 2.29 m w.e.”. There is no information in the methods about how they estimated that. In addition, this simple phrase thrown casually here should be a paper by itself, with lines 186-194 as part of its introduction.

The authors link the regional warming trends with rising CPPn fractions, arguing that glaciers melted during periods with warmer temperatures, which produced a greater abundance of large particles. However, the CPPn fraction is decreasing during the first warming period established by Gilbert et al., 2010 (1920-1960). This contradicts the interpretation of the authors and is not discussed in the manuscript.

Minor comments:

Line 40: The link between measured dust concentration variability in glacier ice cores and changes in dust aerosol concentration in the air is complex. Please rephrase more conservatively.

Chapter 2.3: The exponential fit regression may give a good first order approximation of the accumulation rate. However, I think the authors should show that sublimation and surface melt are secondary processes and should not affect the accumulation rate estimate significantly.

Figure 1: The location of Glacier 8 and of the drilling site are not clear.

Line 139: Why is the mean only taken from the period 1999-2016 when the figure shows data from 1920?

Lines 155-156: Figure 4 does not "indicates reduced deep convection over the Bolivian Amazon". It's a correlation plot, nothing more.

Figure 5: The caption says the think lines represent 10-year LOWESS smoothed data, while in the text a correlation of 3-year moving average of CPPn with TNA is consistently mentioned. Choose one and use it for both the correlation and the figure. You probably want to correlate CPPn and TNA sst after smoothing both, not just the CPPn.

Line 170: TNA SST was positively correlated. . .

Lines 172-173: Why the Nino 4 region? Air parcels arriving to the Bolivian Plateau are more likely to have originated from Nino 1 or 2 regions.

Line 181: What does it mean that the DEMs have high mean absolute errors? Please expand.

Lines 197-202: It looks like the sections for the piecewise linear function were subjectively chosen to suite the interpretation of the authors. A different choice of sections would lead to a completely different interpretation. Please use a more robust and objective method.

Line 207: Could be. Or not. This statement needs some supporting evidence.

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