Second review of Lindau et al 2020
"Giant dust particles at Nevada Illimani: a proxy…."
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The Cryosphere

This is my second review of this paper, and considers the revised version the authors submitted and their responses to my (and the other reviewer's) comments from the first version.

I have a number of remaining concerns about the manuscript, and the author's responses to the original reviews.

Lines 100-104 - Ice layers in the core. The other reviewer pointed this out in the first round of reviews, and the authors added a sentence or two in response (and a very general figure in the supplementary material). I think their analysis falls far short of what is required to justify interpreting the particle record as a purely climate signal. The author's claim that "these features indicate few events of meltwater percolation" (line 102) is not backed up by any analysis. I would need to see some sort of analysis of the various core proxies vs. the ice layer record to have any confidence in that statement. One obvious question - what is the effect of melting on the particle size distribution? Is the depth variability of the giant particle concentration simply a function of surface concentration during melting?

Lines 125 (and throughout) - The use of GPPnb as a proxy. The authors present a timeseries of particle concentration (Fig. 3) and interpret the seasonal pattern of both total and giant particle deposition. In this figure, and lines 200-201, the authors clearly show that giant particle concentrations are highest in the dry season, and much lower (by at least a factor of 2) during the wet season during convective activity. I would argue that this (giant particle concentration) is the most accurate measure of giant particle deposition at the site (after, of course, the authors answer the post-depositional modification question). Yet, the authors then proceed to move to a relative measure of giant particles (giant particle percentage GPP). Unfortunately, that measure conflates two uncertain measures - both fine and giant particles. What if atmospheric processes were affecting the two differently, such that GPP is being altered primarily by fine grain processes? At the very least, I would have to see both measures (GP concentration and GPP) statistically compared vs. the other core parameters and meteorological variables. I suspect (and could of course be wrong) that the correlations with GPP could be non-existent or even absent when run with GP concentrations.

Figures - Figure 3 is not relevant to the author's argument. The overall correlation through the entire record (which the authors have still not quantified) has no bearing on the wet season convective activity. A much more useful figure would be wet season dD, wet season giant particle concentration AND wet season GPPnb vs. year. Similarly, Figure 6 needs to include giant particle concentration for dry and wet seasons vs. dD (not just GPPnb).

Dust provenance - I'm still confused as to what the dust provenance work is supposed to show. The focus here is on the giant particles, and the question that remains to me is - are these

particles simply local (in the Illimani massif) or from some farther source? The simplest explanation seems to be that these giant particles are simply local transport and thus do not require a complicated convective activity explanation. But that can only be proved if the dust provenance geochemistry clearly shows the giant particles are not of local origin. The sample choice and data do not seem to be able to shed any light on this issue - wet vs. dry season geochemistry on bulk samples provides no specific information on the provenance of the giant particles. The giant particles could be from right next the the drillsite, and deposited with a background, fine fraction dust matrix that is from a remote location.