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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The origins of ice crystals measured in mixed phase clouds at High-Alpine site Jungfraujoch" *by* G. Lloyd et al.

Anonymous Referee #1

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Review of "The origins of ice crystals measured in mixed phase clouds at High-Alpine site Jungfraujoch" by G. Lloyd et al.

This manuscript details observations of mixed phase cloud and aerosol properties taken over a high mountain site in Switzerland in order to further our understanding of what microphysical processes contribute to ice formation in mixed phase clouds. I think this is a great article highlighting important research, but I do have a few ideas that should be considered in order to improve the manuscript. I will list my major comments on the paper here:

1. While the introduction goes into great detail about the various studies that have observed mixed phase clouds using a mountain observatory, there is no material that

leads the reader into what the broad motivation of the study is. Why is it important to study aerosol effects on mixed phase clouds in the first place? Doesn't it have to do with how, in general, how models do not resolve these processes very well? What sort of sensitivity do commonly model out parameters (like radiative forcing, etc.) have to the mixed phase cloud properties? Why should we care about the scales of the transitions between phases? I also would suggest splitting up the two paragraphs into even smaller paragraphs as I found Section 1.1 hard to follow in general.

2. While the discussion go into a great deal of discussion of how much processes that affect changes in the ice crystal concentrations could affect the IMF values, there is little to no discussion of how processes related to changes in the liquid droplet properties could also affect the IMF outside of stating that LWC could increase or decrease. Do the authors have any insight as to what processes could contribute to the changes in LWC?

3. The authors rule out the possibility of the Hallett-Mossop process simply because "However, temperatures at JFJ were generally colder than in the H–M zone, and ice concentrations as a function of temperature (Fig. 5a and b) did not show any 15 significant increase at higher temperatures." However, the authors also mention a peak in IWC at -8 C during 2013, and I do see a peak in ice concentration at about 75/L at this temperature range in Figure 5a. Given these increases in IWC and ice crystal concentration at these temperatures, it does make me wonder if the H-M process is occurring at this temperature range. Did the authors check the CPI imagery for the presence of graupel along with splinters? The decrease in CDP droplet concentration along with the increase in LWC in Figures 6a and c lend me to think that there are large enough droplets to form drizzle and splinters, as well as there being enough LWC present for significant riming for 2013. However, I don't see these same trends for 2014. Have the authors checked the mean droplet spectra from the CDP for T > -10 C from 2013 and 2014 to see if it is the case that there are more large drops in the H-M temperature range? Also, if the H-M process is operating for the clouds in 2013, but not in 2014, do

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the authors have any insight as to why? If there is an active H-M process occurring, it may be worth adding a couple of figures to that effect. This also affects section 6.4 which talks about riming on mountain surfaces.

4. Whenever they can, the authors should replace statements like “several hundred” with more exact values in order to make the paper more quantitative. A good example of this is in the fourth conclusion.

Minor comments:

Section 4. Paragraph 1. The way this is written, it is hard to identify the high and low pressure systems since most of the text in the first paragraph does not refer to Figure 3 until the very last sentence. This should be rewritten to guide the reader through Figure 3, and it should definitely introduce Figure 3 before describing the synoptic features.

Section 5.1. Line 2. Missing period.

Section 5.1. Lines 19-20. Do you think the Hallet-Mossop process could cause this? This is related to major comment 3.

Section 6.1. Lines 8-11. Could you quote Korolev et al.’s exact frequencies they found?

Section 6.1. Second to last paragraph. There are dendrites present in Figure 14, so I do not think you can say that the fragile habits typically associated with mechanical breakup are not present. Does this affect your conclusion that “there is currently no evidence in the literature that this mechanism would be capable of producing the ice crystals concentrations observed.”

Section 6.4. This is related to major comment 3. I don’t think you have adequately shown that there is no link between the concentration of small ice crystals and the number of droplets or liquid water content at temperatures around -5 C, especially since the data during 2013 suggest an enhancement of LWC and ice crystal concentration that corresponds with a decrease in droplet concentration for temperatures around -5 C. Have you checked for the presence of riming in the CIP imagery for 2013 at around

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-5 C?

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