Interactive Comment by E. Herrmann

For clarity, the referee's comments are copied in black and our responses are offset in blue.

Dear authors / Hi guys!

With some interest I have followed the outcome of your latest Jungfraujoch campaign (this and the snow-blow paper). Referee #1 has already made some useful remarks (most of which I would agree with) but there is one more issue I'd like to raise. In figure 6, you show something that you call particle size distributions. These extend from ~250 nm to ~15 micrometer and have a peak between 400 and 800 nm or so. However, there is a bit of a problem with that: The actual size distributions at Jungfrauoch, measured behind the same inlet, do not look at all like the plots in figure 6 (see http://onlinelibrary.wiley.com/doi/10.1002/2015JD023660/full). The size distribution at Jungfraujoch has an accumulation mode at roughly 140 nm, beyond that is merely that mode's "tail" and the occasional dust particle from Africa.

The explanation for this odd result can be found earlier in the manuscript (page 26071 bottom) where you write that the WIBS doesn't provide reliable numbers for particles with diameters below 800 nm. With this in mind it would seem that figure 6 merely shows the WIBS signal. I could get behind that. But it is important to note that the WIBS signal is NOT the particle size distribution. Instead I would say that the WIBS signal is the actual size distribution multiplied with the WIBS detection efficiency curve. And that detection efficiency is significantly smaller than 1 below 800 nm (as the text implies) and appears to be 0 (zero) at 250 nm (as figure 6 suggests). It is therefore a bit misleading to write that figure 6 shows particle size distributions at Jungfraujoch. And I also want to stress that the actual size distribution does not have a mode at 580 nm (as stated in the manuscript). That "mode" is a result of WIBS detection efficiency. It would be nice if you would take this into account in the revision. Considering that not only the counting efficiency decreases but also the fluorescence measurement is unreliable below 800 nm, one of course has to wonder how meaningful the information in figure 7 is. You might want to discuss this in the revised manuscript as well

We thank the E. Herrmann for their helpful comments and recommendations. We agree that the sampling of particles with the WIBS is unreliable under 0.8 µm and we will revise the discussions of the size distributions to clarify these effects of the reduction in counting efficiency and include appropriate caveats.

We now include SMPS data in our revised analysis as part of a comparison to long term data sets which provides accurate size resolved concentrations down to D_p ~ 20 nm which we compare to the WIBS data to highlight the reduction in performance at small sizes. This can be found in our response to referee #1.

Beside this main point I wanted to make, some other things caught my eye. For example, what exactly is the motivation for discussing the SDE events separately? The current manuscript doesn't appear to be all that massive to make this necessary.

During the first SDE we sampled at a different location (Schilthorn) with the WIBS which makes directly linking the cloud microphysics measurements made at the jungfraujoch difficult as the two mountain sites are separated by several kilometres horizontally and approximately 500m vertically. Unfortunately during the second SDE we do not have reliable cloud microphysics data to perform analysis on due to artefacts resulting from the extremely high dust concentrations observed during the SDE.

And why do you call clouds glaciated that are still 50% water? While one certainly needs to define a threshold, 50% feels like an unintuitive choice.

> We have now revised the threshold between mixed phase and glaciated conditions to IMF ≥ 0.9 to be in keeping with Lloyd et al., (2015). A short discussion of the effect this has had on the analysis is provided in our response to referee #2.

Finally, the colours got mixed up in the legend of the top panel of figure 3 (the caption is correct).

> We thank E. Herrmann for bringing this to our attention and this will be corrected in the revised manuscript.

References

Lloyd, G., Choularton, T. W., Bower, K. N., Gallagher, M. W., Connolly, P. J., Flynn, M., Farrington, R., Crosier, J., Schlenczek, O., Fugal, J., and Henneberger, J.: The origins of ice crystals measured in mixed-phase clouds at the high-alpine site Jungfraujoch, Atmos. Chem. Phys., 15, 12953-12969, doi:10.5194/acp-15-12953-2015, 2015.