Reply to RC C1220 of Jeff Stith:

We thank the referee for carefully reading our manuscript and the valuable comments and suggestions. We have included responses to the reviewer's comments below and indicated where changes were made to the text (blue).

Line 69, The FSSP was originally developed by PMS, not SPEC. See, for example Baumgardner (1983) and references therein.

The text was changed to "the Forward Scattering Spectrometer Probe (FSSP, PMS Inc., Boulder USA)".

Line 176. It is not clear where the control box and inlet box are in Fig. 2.

In Fig. 2 only the HOLIMO II inlet is shown. To make this clearer, the references to Fig. 2 was moved a few sentences up and the caption of Fig. 2 was changed to "A horizontal cross-section of the HOLIMO II inlet box...".

Figure 3. Is there supposed to be a blue dashed line and a blue dotted line here? It looks like there are two dashed blue lines.

For every wind speed two dashed lines are shown, which correspond to different simulations. For the red and black lines there are overlapping and therefore hard to distinguish. A dotted line for anisoaxial sampling is only shown for the 5 m/s velocity.

Section 2.3. The analysis presented here assumes that all particles hitting the wall are lost. This seems reasonable for smaller water droplets, but larger ones may hit the wall and splash. Furthermore, ice crystals hitting the wall are likely not lost and may shatter into pieces. Some discussion of the likely impact of these two effects is needed, since they are relevant to the material presented in Section 4.3.

True, we agree that more discussion about these assumptions is needed, although it is hard to quantify these effects. The following sentences were added: "These assumptions have some uncertainties. Not all particles will be lost to the wall, but larger droplets might splash and larger ice crystals might shatter into pieces. The magnitude of the overestimation of the number of smaller particles is hard to quantify. However, because of the low inlet velocities, particularly in comparison to airborne measurements, it is expected to not significantly change the results."

Section 3.1. This section could be improved by adding a bit more detail and perhaps more references. Not many readers will be familiar with terms such as "dilating the binarized voxels", so more explanation would be helpful, such as a reference for "so called connected component labeling" (line 338).

This section was kept comparatively short because the data analysis is described in detail in Fugal et al. (2009). Nevertheless we added some more explanations to increase the comprehensibility for the readers and a reference:
"The volume is partitioned in discrete volume elements, called voxels, defined by the pixel aperture and the distance between the layers."

(so-called connected component labeling, "Haralick and Shapiro, 1992). The grouped voxels represent a probable particle …"

new reference:
Haralick, R. M. and Shapiro, L. G.: Computer and robot vision, vol. I, Addison-Wesley Pub. Co., Reading, Mass., 1992.

Lines 370-371. "Rather, to avoid these false particles, we excluded all smaller particles within a cylindrical volume around the larger particle." How much of the total volume is excluded by this technique? Does this technique introduce a size bias in the sample volume?

Because the cloud particles are quite diluted only a small fraction of the volume is excluded by this technique. In a 30 s-interval typically 0.1 % of the volume was excluded. The maximum excluded volume was 0.44 %. Therefore this does not change the results significantly, and consequently does not introduce a size bias. The sentence was added:
"Because the cloud particles are quite diluted, this excluded less than 0.5 % of the volume."

Section 4.3 The possibility of re-suspension of ice from the surface should be discussed as a possible source of ice particles observed in this study. This is particularly relevant since the air masses likely experienced a moderate ascent over nearby glaciers.

The possibility of re-suspension of ice from the surface was estimated by analyzing a cloud free period where the wind velocity where extraordinary high. We added the paragraph:
"The background caused by re-suspension of snow from the ground was investigated by analyzing a cloud free period. The measured background total particle concentration was 0.1 cm^{-3} and the TWC was 8.3 mg m^{-3}. Because of the extraordinary high wind velocity of 19 m s^{-1} this is an upper estimation and can be expected to be lower for calmer conditions. "

Conclusion, lines 656-659. Although you observed mixed phase cloud for four hours, you have not measured the lifetime of super-cooled liquid water in these clouds, so these conclusions are not fully supported by the analysis presented here.

We measured the clouds at the single point Jungfraujoch from an Eulerian point of view. Therefore different air masses were measured over time, which makes conclusions about the lifetime of the cloud difficult. Nevertheless, the stable measurements of mixed-phase clouds over four hours indicate that the WBF process cannot be active at these conditions, because it would have glaciated the cloud. To clarify this the paragraph was changed to:
"During the 8h period a transition from a liquid to a mixed-phase cloud was observed. The mixed-phase cloud was observed for four hours at Jungfraujoch, suggesting that the WBF process, which would have glaciated the cloud, was not yet dominant. Korolev (2007) showed that updraft velocities of around 2 m s^{-1} are sufficient to exceed supersaturation with respect to water, which will result in a growth of both liquid droplets and ice crystals. Such updraft velocities might have occurred during the lifting of the air masses to the JFJ."