Response to Referee #1

The authors would like to thank the reviewer for carefully reading the manuscript and providing thoughtful remarks that have improved the manuscript. The replies to the comments are given below. The referee comments are highlighted in red with our responses in black fonts.

General comments:

1. Add more discussion about why surface measured aerosol can be used to estimate cloud droplet number concentration in the cloud above. It might not be true if the orographic clouds are formed somewhere else and drift to the observational site, or the cloud is decoupled with the surface, or aerosol from the long-range transport plays a significant role in cloud droplet activation.

Thank you for raising this point! Throughout the observation period, clouds are formed locally at Wolfgang-Pass (WOP), when the low-level flow is forced to ascent due to the local topography (the very good droplet closure supports this). This could not be repeated for Weissfluhjoch (WFJ) owing to a lack of in-situ data, however the airmasses sampled (i.e. those given as input to the parameterization) are often in the free troposphere, so they should contain the same aerosol as the one used to form the clouds. This does not apply under perturbed free tropospheric conditions prevailing at WFJ (i.e. injections from the boundary layer of lower altitudes), which however bring less hygroscopic particles at the mountain top site. This is now discussed in the revised manuscript.

2. There is no in-depth discussion of the properties of the mixed-phase clouds (e.g. INP or ice properties) in the manuscript. Are you sure ALL those clouds are mixed-phase clouds? If not, the title is not accurate. Please consider changing the title and corresponding text in the manuscript or adding evidence to show that those clouds are mixed-phase clouds. In addition, ice processes and collision coalescence process are not considered in the cloud parcel model. The statement "Under such conditions, droplet size tends to be minimal, reducing the likelihood that large drops are present that promote glaciation through rime splintering and droplet shattering" is unfair and misleading in the abstract. Even this statement is true, several crucial steps are needed to prove this statement.

These are all good points. We focus on liquid cloud droplets that can form in these clouds along with their drivers of variability, given the other RACLETS studies that focus on the ice content of the clouds (in the following paragraph). It is well established that for similar liquid water content, more cloud droplets lead to smaller droplets. It is also clear that in such situations, secondary processes like collision-coalescence and riming reduces in probability. To note this possibility and important implications is in our opinion quite appropriate – especially since observational evidence in other studies for clouds with similar dynamical conditions point to such possibilities (e.g., Lance et al., 2011). Even more so, given the discussion on limiting droplet number, N_d^{lim}, which is effectively the upper limit in droplet number and hence the maximum extent of droplet size reduction. We will note, however, that we have not proven these aspects but the statement remains one of the main implications of our study.

Regarding the mixed-phase nature of clouds during RACLETS, there are three studies under review (Ramelli et al., 2020a, b; Lauber et al., 2020) providing extensive descriptions of ice properties during specific cases (22 February, 7 March, and 8 March 2019) within mixed-phase clouds (MPCs). To provide further evidence of the presence of MPCs, we have included a figure in the supplemental material (Fig. S4), showing the estimated degree of riming of 39 dendritic crystals collected at WFJ (in the scope of another analysis) during our period of interest. Characteristic images of the rimed ice particles are provided. All dendrites that were photographed are at least lightly rimed (i.e. riming degree = 1), which is direct evidence for the co-existence of droplets and ice in clouds. A relevant discussion is added under Section 3.2.1 in the revised manuscript.

It is important to mention here that despite the mixed-phase nature of some clouds sampled over the valley site, processes like condensation freezing and the removal of cloud droplets through riming and collision-coalescence are not found to disturb the maximum supersaturation and hence the number of activated droplets predicted by the parameterization. The good degree of droplet closure supports this statement.

<u>Specific comments:</u>

1. Line 162: "11.5 nm and 469.8 nm" are radius or diameter?

Here we are referring to electrical mobility diameter. We have now clarified this in the revision.

2. Line 280: "…being 200 m and 1100 m AGL for WOP and WFJ". This sentence is not consistent with the previous statement. Only WOP has wind lidar, correct? Why choose 200 m and 1100 m as the altitude of interest?

We use vertical velocity data extracted at 200 m for WOP, as the wind lidar deployed at this station has no values closer to the ground, while the wind values at the altitude of WFJ (which is located approximately 1 km above WOP) are selected as a proxy of the vertical velocity prevailing at the mountain top. This is the best we can do with the observations at hand, and we will make sure this is clearly stated in the text.

3. Line 308: Please add pressure and temperature figures to show that "a high pressure system was dominant over Europe with clear skies and elevated temperatures."

A new figure (Figure S3) is now added in the supplemental material showing the air temperature and pressure measured by the MeteoSwiss station at WFJ. The two periods of interest are indicated on this figure with black arrows.

4. Figure 1a: suggest changing the red color of dots for WOP. Red also means high kappa value which is confusing.

Good point! We adopted a blue-to-yellow colormap throughout the paper to avoid any confusion.

5. Line 360 and Figure 2: suggest shading the time period when precipitation occurs in Figure 2, as done in Figure 7. It might be helpful to visualize "a big spike of N_{aer}" before the third precipitation event.

Thanks for this suggestion. The precipitation events depicted in Figure 2 are shaded in the revised manuscript, as per Figure 7.

6. Figure 8: I'm a little bit confused here. So different circles represent calculated N_d for different aerosol size distribution and kappa? The horizontal dashed line represents the limiting droplet number. What about the other dashed line?

We apologize for this confusion. The colormap represents the maximum in-cloud supersaturation (predicted by the parameterization), not the hygroscopicity parameter. The slopped dashed line was used to highlight the upward trend between N_{aer} and N_d, observed within the aerosol-limited regime. We agree that these lines are confusing and hence they are now removed from the revised manuscript.

References:

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