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## **Comment on acp-2022-57**

Anonymous Referee #2

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Referee comment on "The ice-nucleating activity of African mineral dust in the Caribbean boundary layer" by Alexander D. Harrison et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-57-RC2>, 2022

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### **Review of the manuscript „The ice-nucleating activity of African mineral dust in the Caribbean boundary layer” by Harrison et al.**

The manuscript by Harrison et al. (2022) presents the data of a field campaign in Barbados, dealing with aerosol sampling and determination of INP activity of aerosols within the Marine Boundary Layer (MLB). Sampling was performed in Summer 2017 at Ragged point at a 17m sampling tower to reduce the sea salt influence.

The size distributions, derived by APS and SMPS measurements, and size-resolved chemical information, derived by scanning electron microscopy (SEM), show the dominance of mineral dust for larger particles (> 200 nm) and in this way for the surface area (volume/mass).

The INP investigations delivers a low INP concentration and low active site density (in comparison to other mineral dust influenced locations).

Harrison et al. state that the main reason for this finding will be the low detected K-feldspar content (0.1 – 2 wt%) in Barbados, which is assumed to be a key component for the INP activity of mineral dusts.

Second part of the manuscript is characterized of discussion about the possible reasons for the low IN activity - Source region, transport process and mixing with salts are named.

The manuscript is well-written and has an excellent structure. The presented analytical

procedures are state of the art and well explained. Figures are good in quality and appropriate in number. The manuscript definitely fits the scope of the journal. I recommend to accept the manuscript in ACP after minor revisions.

While I have no major critical points to the first part of the manuscript, there are two substantively points which have to be considered in the discussion part.

A - The main message of the manuscript is that a low INP activity of the MLB aerosol particles is detected in Barbados and this is most likely due to the low K-feldspar content.

I accept this thesis, but find that even more convincing data could possibly have been obtained from the samples taken. Above all, it should be clearly shown that the K feldspar was indeed very low throughout the campaign.

For this purpose, a more precise determination of the real K-feldspar content within the individual samples would be desirable. The manuscript mentions XRD measurements, but only one measurement of a rain sample is presented. Were the daily samples also examined with XRD and if so, what was the variability of the K-feldspar values?

Otherwise, the K-feldspar content is derived from the scanning electron microscopic data. This is possible in principle, but it requires several things. On the one hand a very low particle assignment of the filter, so that a single particle analysis remains possible and on the other hand the analysis of at least about 100 particles / analyzed size bin. Figure 3 shows the scanning electron microscopic results of 4 samples indicating significant variability (at least with regard to sea salt). Contrary to the figure legend, only percentage values can be seen, not a number of analyzed particles, which would be desirable. I would have liked to support the assumption of the low K-feldspar content in general throughout the campaign either by the XRD data from more samples or electron microscopic data.

B - The discussion on other factors, influencing IN activity (based on literature/comparative values), is generally very thorough and broad. In some cases, there are even repetitions here, which could be avoided by some cuts.

I am not entirely convinced by the discussion about the assumed low influence of internal mixing with sea salt.

In the 4 compositions shown in Figure 3, a larger proportion of external sea salt particles in coarse mode is recognizable in 2 samples. However, the extent to which internal mixing with sea salt was found in the "Al-Si-rich" particles (should be visible in SEM) is not used for the discussion. Is there SEM data for more samples? This would also help to get an idea of the variability of aerosol particle composition.

These data could significantly strengthen the discussion and conclusions.

Minor points:

A - Figure 2 shows size distributions. In red those that were determined in the SEM. Size distribution determinations with the help of SEM are difficult and a factor of 2 as an error and a flatter course is quite typical in this type of determination.

The size distribution from SMPS/APS should still have the higher accuracy. Therefore, it would be advisable not to use the size distribution determined with the help of the SEM data but to combine the relative group abundance (SEM data) with the SMPS/APS size distribution in order to achieve the most reliable results.

B- Line 72 Reischel, 1987 used ammonium iodide. These results are difficult to compare with atmospheric IN measurements.