Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC2
https://doi.org/10.5194/acp-2021-741-RC2, 2022


# Comment on acp-2021-741

Anonymous Referee #1

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Referee comment on "Mexican agricultural soil dust as a source of ice nucleating particles" by Diana L. Pereira et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-741-RC2, 2022

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## General Comments

This work evaluates the ice nucleating ability of Mexican agricultural soil dust and discuss the possible factors influencing its ice nucleating ability. Since "the ice nucleating abilities of agricultural dust particles from the Mexican territory have not been reported up to date (Lines 84-85)", the dataset in this area will be potentially helpful to improve our knowledge of the possible impact of agricultural soil dust on ice nucleation in mixed-phase clouds. Unfortunately, however, there are some problems regarding the experimental approaches. For example, it is not clear when and how the soil and aerosol samples were obtained. The use of sized-resolved INP data are unique, but it is disappointing that only limited INP data in a narrow size range are reported in this manuscript. Although this work tries to evaluate the possible importance of organic and mineral components in the agricultural soil dusts using several approaches, the conclusions are obscure and not well summarized. In addition, it is difficult to compare the results presented in this manuscript with those from other previous studies which evaluated the ice nucleating ability of agricultural soil dusts in other locations, because this study uses $T_{50}$ values while most studies have used other parameters like the ice-nucleation active site density (INAS). For these reasons, it is almost impossible to evaluate the quality of the data and whether the key conclusions presented here are scientifically appropriate. I think that the quality of this manuscript should be significantly improved before considering publication.

## Specific Comments

1) It is not clear when the soil and aerosol samples were collected. As for the soil samples, please clarify when the samples were collected in Table 1. As for the aerosol samples, please clearly explain when and where each sampling was started and finished (this

information should be summarized in a table).

2) When did you collect the soil samples used for microbiological analysis and then when these were analyzed? If the authors would like to measure the colony forming units (CFUs) per gram, the analysis should be performed just after sampling, because the microorganisms in soils would be significantly changed once these were collected and stored at room temperatures. If the samples were not analyzed immediately, the data would not be scientifically valuable, and all the data and description about the CFU data (Lines 179-185; Lines 264-275; Figure S4) should be removed.

3) I could not understand why only the INP data obtained from the MOUDI stages 3 to 6 (Figures 3 and 5) are reported, despite the facts that the samples were likely to be collected on the eight stages of the MOUDI (Lines 130-133). Although the authors explain that "the present results focus on particles >0.56 μm as it has been shown that particles >0.5 μm have a higher potential to act as INPs (e.g., DeMott et al., 2010) (Lines 190-191)", I think that they could evaluate whether the particles larger than 0.5 μm indeed have a higher potential to act as INPs than those smaller than 0.5 μm based on their data collected on the MOUDI stages 7 and 8. It is also unclear why the INP data larger than 5.6 μm (MOUDI stages 1 and 2) are excluded.

4) In addition, I would like to suggest showing the total INP data (i.e., the sum of INP data obtained from all the MOUDI stages).

5) A fatal flaw of this work is a lack of the reliability of the field INP data. As I already point out in Comment 1, it is not clear when and how the field samples were collected. However, if only 4 field INP data (only 1 field sample at each location) are available as shown in Figure 3, the numbers of the data are too small. Furthermore, it is also unclear whether the INP population in the field data were indeed well characterized by local agricultural soils, because their source and composition are not evaluated. Given the location of the sampling sites (Figure 1), there is the possibly that the aerosol population in the field INP data were characterized not only by local agricultural soils, but also by various types of aerosols from oceanic and urban/rural sources. If the authors would like to discuss the difference between the field and laboratory INP data based on only 4 field data, they should provide strong evidence that the available field data were mostly characterized by local agricultural soils. Otherwise, the discussion and conclusion about the comparison of the field and laboratory data presented in this manuscript (e.g., Lines 28-30; Lines 189-210; Lines 369-372) would not be scientifically valuable and hence should be removed.

6) Although the authors describe that "the differences between laboratory and field environments are also reflected in different PSD observed during the aerosolization process (Fig. S1) (Lines 205-206)", I doubt if the particle size distribution (PSD) of the filed (F) samples would be totally characterized by local agricultural soils. Please show evidence that all the sizes (0.3-10 μm) of the field samples were entirely characterized by local agricultural soils.

7) If the authors consider that "the highest particle concentration for the L samples was found for particles between 1.0 μm and 5.0 μm (Fig. S1a), while the F samples are enriched in smaller particles, i.e., 0.3 μm (Fig. S2b) (Lines 208-209)" and "the larger particles present in the L samples likely promoted ice nucleation at warmer temperatures (line 210)", the authors would need to evaluate the ice nucleating ability of the laboratory (L) samples in the size range smaller than 0.56 μm (see also Comment 3).

8) I cannot understand how Figures S2 and S3 are prepared. In addition, I cannot understand why the results of the agricultural dust particles in the laboratory (before heat treatment) are different between Figures S2 and S3. Please explain the details of these figures.

9) Previous studies (Tobo et al., 2014; Steinke et al., 2016) have reported the INAS of agricultural soil dust, instead of $T_{50}$ values. If the authors think that "the ice nucleation temperatures observed in the present study are on the same order as those reported for agricultural dust in Wyoming (USA), from -18ºC to -36ºC for dp=0.6 μm (Tobo et al., 2014), and Argentina, China, and Germany from -11ºC to -26ºC for dp <5 μm (Steinke et al., 2016) (Lines 231-233)", the INAS of agricultural soil dust presented in the manuscript should be calculated.

10) Although this work tries to discuss the possible influence of organic matter (Section 3.2) and mineralogy, especially K-feldspar (Section 3.3) using several approaches, it is hard to understand the key conclusion of this study. Do you think which component would have more impact on the ice nucleating ability of the agricultural soils? Please discuss this point and explain in the Abstract and Conclusion sections.

11) Although the correlation map is shown in Figure S5, it is still hard to imagine the relationship of $T_{50}$ values with OC and mineral compositions for each sample. I would like to suggest preparing some scatter plots that compare these parameters (e.g., INPs vs. OC, K-feldspar, etc.).

12) In addition, I would like to suggest comparing the total INP data (see also Comment 4) with OC and mineral compositions, because the OC and mineral contents reported here would be based on the analysis of the bulk samples smaller than 10 μm. This result might be helpful to answer Comment 10 (which component would have more impact on the ice nucleating ability of the agricultural soils?).

**Technical Corrections**

13) Line 22: ice crystals formation => ice crystal formation

14) Line 62: improving => enhancing or influencing (or something like this)

15) Lines 68-69: Garcia et al. (2012) reported the results from aerosol sampling in the air and not in soils.

16) Lines 95 and 104: The terminology like Morelos (MOR) and Morelos (ZAC) are confusing.

17) Line 193: solid lines => solid curves?

18) Line 194: dotted lines => dashed curves?

19) Line 209: Fig. S2b => Fig. S1b?

20) Line 243: dp <0.6 μm => dp = 0.6 μm

21) Line 301: What is “Hunucmá sample”?

22) Table 1: Please clearly explain why the samples are labeled as “Nopal”, “Corn”, “Bean”, “Chili”, “Wheat”, “Onion”, and “Corn 2” (please add some explanations in the Methods section). In addition, what do you mean by “°O” and “N°”?

23) Figure 1: It is hard to see the location of each sampling point, because the same symbols with similar colors are used.

24) Figure 2: It is hard to understand the experimental setup. More detail schematic images (particularly, around aerosol collector) should be presented.

25) Figure 3: It is hard to see the difference of curves, because similar curves and colors are used. Please prepare more eye-friendly figures.

26) Figures 3 and 5 caption: d) 0.56.1.0 μm => 0.56-1.0 μm

27) Figure 4 caption: Organic carbon, elemental carbon, and mineral contribution ~.

28) Figure 5: It is hard to see the difference of curves, because similar curves and colors are used. Please prepare more eye-friendly figures.

29) Figure 7: Please explain how the red-colored range of "Field samples (this study)" is defined. In addition, the authors would need to prepare a supplementary figure showing the comparison of the INP data with this red-colored range. In this figure, the INP data should be the total INP number concentration (see also Comment 4).

30) Figure S1: I would suggest the use of $dN/dlogD_p$ instead of aerosol concentrations (N). In addition, please clearly explain when and how the data in Figure S1b were measured.

31) Figures S2 and S3: Please explain what the box-and-whisker plots indicate.