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Comment on acp-2022-356

Anonymous Referee #2

Referee comment on "Measurement report: The Urmia playa as a source of airborne dust and ice-nucleating particles – Part 1: Correlation between soils and airborne samples" by Nikou Hamzehpour et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-356-RC2, 2022

This study analyses both soil samples and (airborne) dust samples collected in the drying region of the Lake Urmia (LU) in Iran. Besides for an analysis of parameters such as size distribution and composition, also ice activity was determined from differential calorimetry. While the characterization of the samples was well done, the use of differential calorimetry may not have given the full extent of information possible from off-line IN measurements. Particularly the freezing onset is not a very useful parameter, and it was neither defined how this nor the frozen fraction were defined and derived. Therefore, some unexpected results concerning the ice nucleation may arise from the applied methods, and this is difficult to judge from the present material.

Related to studies cited in the manuscript (lines 422-423), it also already has been known that similarity between mineralogy of soil and dust in a region is an indicator of the regional origin of aerosols, and dust mineralogy has been used for the identification of the different source regions worldwide. Therefore, the here presented study corroborates these findings but does not add much to it. It is also said clearly, that (line 520) "Abandoning agricultural lands in arid and semiarid regions due to climate change and lack of water is becoming a serious problem." This is an important statement which could have been stressed more.

However, overall, the arising impression is, that while this study is a thorough characterization of dust in the region of LU, it does not extend far beyond that. As such, I advise the editor and the authors to discuss if this work is rather a measurement report and not a research article.

Besides for the above voiced criticism concerning the ice nucleation measurements, the study overall is a thorough study which merits publication, once the below addressed (smaller) issues will have been tackled.

Specific comments:

Lines 27-28: Onset-temperatures are not very telling for judging an IN activity, and the onset temperatures you report here are rather low, compared to data from a range of other studies using filter samples and PCR-tray based off-line data evaluation (where an "onset" this is often found above ~260K (but typically not reported)). Therefore, claiming "high potential of dust blown from Urmia playa surfaces to affect cloud properties and precipitation" is exaggerative. Moreover, atmospheric INP number concentrations would be interesting, in this context, and it is not clear if they can be derived from your measurments.

(Publications I refer to here, to name only a few, are: Schneider et al. (2021), Testa et al. (2021), Gong et al. (2019) and even McCluskey et al. (2018) in clean marine regions.)

Line 164 ff: It did not become clear if the particle size distribution was only measured for soil samples or also for dust samples.

Line 188: Please add if the surface collected soils that were compared with nearby air-sampled dust samples were among the original 130 samples mentioned in section 2.1.1? Or how was the location for the selection of these soil dust samples chosen? Please also add if the collection was done at the same time as the dust sample.

Lines 251-252: More description is needed on the parameters introduced here – the reader should not have to look up another publication to obtain at least the needed basic information. Specifically: How was T_het determined, and how was F_het determined and what does it express? F_het often is a temperature dependent variable. Is it, in your case? Did you count separate droplets and frozen droplets and determined F_het from these? Or did you use the area under the thermograms? Could you add a figure showing an example, or showing F_het for all samples?

Lines 319-321: These two sentences are a bit contradictory, as you say in the first sentences, that soil organic matter is present in relatively low amounts, but then you say that these are typical values. So what does "relatively low amount" refer to? (The two publications you cite here give values in a similar range.)

Line 426 ff: Is it fair to assume that mixing of dusts while they are airborne explains your finding that there is an overall lower correlation coefficient between soils than dusts? If so, maybe add this line of thought to the text.

Line 551: Again: How is F_het defined? It is difficult to judge your results if it not clear how this parameter was derived.

Figure 10: What is indicated by the temperatures given in the plots? Are these onset temperatures? Again: How are they derived, anyway?

Line 565 ff: Could these observations also originate from peculiarities of the DSC-technique? Is there a chance to repeat these measurements with other off-line INP analysis techniques of close by befriended groups? This is not too much work and could clarify if you are really onto something here.

And, as mentioned above, onset temperatures are not a very informative parameter, anyway, and F_het was not defined. Much could be gained by additional measurements.

Lines 583-585: If organic substances and ions would mask the ice nucleation by K-feldspar and quartz, as you say, higher onset temperatures may be expected. Have you tried if heating the samples changes the results on ice nucleation? If you did, but this is part of the second paper, maybe at least point towards this here.

Lines 599-601: Is there a chance to determine the particle surface area of both soil and dust samples to tackle the surface area dependence of ice nucleation and therewith to make these two groups of samples comparable?

Minor and editorial comments:

Line 263: In your manuscript it is sometimes “sa-sheet”, sometimes “Sa-sheet”. As this is some kind of a parameter, it should not change but be consistently the same at all occurrences. As you capitalize most of the other abbreviations, it would be best to also do it for that one.

Table 1: It is confusing that you mix long and abbreviated sample names, i.e., giving both

for some and either one or the other for others. Preferentially, both would be given here for all samples, so one could refer to this table and would not go back to the text where this is defined if one wanted to look that up again.

Also: Fan delta is not included in Table 1. It is also not included in Table 2. Why is that? Is it, because (line 291) "No soil samples were taken from these locations due to waterlogging."? If so, why is it mentioned and included at all, in your text? Explain this when you introduce "Fan delta" in the text.

Figure 3: According to Fig. 1, black circles should be the dust sampling sites. Here it says it's the white circles?

Lines 526-528: Please add already here that the size classes for clay, silt and sand can be found in Table 6.

Table 6: "Clay" is capitalized, "silt" and "sand" are not. Unify.

Line 543: As you cite Froyd et al., 2022 here, make clear that they refer to the Middle Eastern region as a whole, not to the LU region in particular.

Line 544 ff: "… it has been demonstrated that on a regional scale, the direct dust-climate feedback is enhanced by an order of magnitude near major dust source regions (Kok et al., 2018)." This enhancement is compared to what? Please add.

Literature:

Gong, X., H. Wex, T. Müller, A. Wiedensohler, K. Höhler, K. Kandler, N. Ma, B. Dietel, T. Schiebel, O. Möhler, and F. Stratmann (2019), Characterization of aerosol properties at Cyprus, focusing on cloud condensation nuclei and ice nucleating particles, Atmos. Chem. Phys., 19, 10883-10900, doi:10.5194/acp-19-10883-2019.

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