Reply to Reviewer 1

We would like to thank the anonymous reviewer 1 for his helpful comments and suggestions. In line with the comments and suggestions we have revised the manuscript and made significant additions and changes. Below are all the comments (in bold) followed by the replies. The parts that are in italic are corrections that are included in the revised version of the paper:

General comments:

The introduction is very general and far from being complete. Important information regarding heterogeneous ice nucleation (especially immersion freezing) and the atmospheric relevance of this study is missing. Additionally, literature data is not properly presented. A detailed error analysis should be added to the Methods. The uncertainties in calculating FN and hence AF are necessary. Furthermore, the uncertainties in the reported temperatures are essential.

We modified the introduction based on the reviewers' comments and suggestions. Explanations on the different nucleation mechanism were added. Errors bar has been added to all the figures with explanations in the text.

There are many imprecise statements in the results section. The authors move back and forth between frozen fractions, onset freezing temperatures, IN concentrations and activated fractions. This section can be divided into subsections with a better structure. Figures 4, 6 and 7 are not necessary. The same results are clearly reflected in Figures 9 and 10.

Figures with freezing fraction (Figures 4 and 9 from the original manuscript) have been removed from the new revised manuscript. Instead, this information will be presented in table 2 in the new revised manuscript. However we don't agree with the reviewer regarding figures 6 and 7 (in the original manuscript). These figures present the results of IN concentrations and activated fraction for all the sampling days (19 in total) and thus are valuable for comparison with the days we identified as dust storm days and as clean days. The relatively small spread in the results helps to strengthen our argument that dust particles are always present in the atmosphere in this region.

Lastly, in the abstract, introduction, methods and results sections it is mentioned that the ambient particles were studied under different meteorological conditions. However, these conditions (besides the wind direction presented in the back trajectories) and their influence on the IN abilities of the sampled particles are unclear. I fully agree with the key points (i.e., the transfer efficiency, the use of median freezing temperatures, the comparison of the current results with literature data and the spread of the spectra at high temperatures) highlighted by Dr. Vali.

We decided not to use the term meteorological conditions but rather to specify that the measurements were done during days which were classified as dust storm days and those without dust storms. As was mentioned above, the median freezing temperatures will only be presented in a table. Our reply to the question about the removal efficiency is given further down in our reply.

Specific comments

Abstract

P. 472, l. 10-11: It is unclear to which droplets are the authors referring to. I suggest changing it to: Droplets containing aerosol particles from dusty days froze at warmer temperatures than droplets containing aerosol particles from clean days.

Thanks. The sentence has been changed

Introduction

The paragraphs are not clearly structured. The heterogeneous ice nucleation modes are mentioned but they are not explained/defined in the text. Especially, immersion freezing, which is the ice nucleation mechanism of this study, was never defined. Where and under what conditions is immersion freezing important? The motivation of this study is very general. The importance of the IN concentration and its atmospheric relevance is not clearly stated in this section. Why is it important to measure the IN concentration at the ground-level? What is the contribution of the present study to our current knowledge?

Literature data regarding IN concentrations is cited/discussed; however, a distinction between ground-based and aircraft-based measurements is not provided. Without this information, readers who are not familiar with ice nucleation will easily get confused.

We have modified the whole introduction based on the comments and have added an explanation about the different nucleation mechanisms.
Ground measurements have the advantage of allowing to monitor aerosol and ice nuclei characteristics on a regular basis. In fact we have monitored ice nuclei concentrations by condensation freezing on a daily basis for over two years (to be submitted), something that is impossible to do using airborne platforms. Furthermore, it is important to note that during dust storms the atmosphere is well mixed almost all the way up to cloud levels. And indeed our ice nuclei measurements are in general agreement with measurements of ice crystals concentrations in clouds in Israel (Levin et al., 1996) and with LIDAR measurements. This does not mean that the ice nuclei measurements have one to one relations to ice crystals in clouds but it does indicate that the ground measurements are valid as indicators of the potential for ice formation.

It is interesting to point out that Kanitz et al. (2011) and Seifert et al. (2010) observed a relatively high fraction of ice in mid-level startiform clouds with cloud top temperatures as warm as -10^{0}C or even warmer when dust particles were present. This is very similar to the results of Levin et al. (1996) who reported on ice concentrations in Eastern Mediterranean convective clouds. It is also in good agreement with the present results showing the effectiveness of the mineral dust particles as freezing nuclei at such warm temperatures.

P. 472, l. 14 to 18: There are several sentences that should be cited. Add the corresponding references.

We have changed the sentences and added the citations.

P. 472, l. 23 to 26: I think the authors can replace the cited references by the review done by Hoose and Möhler (2012).

We have changed the sentences.

In the last decade much attention has been given to laboratory studies on heterogeneous ice nucleation (e.g. Hoose and Möhler, 2012 and references therein)

P. 473, l. 9: Why is it dust important? Do the authors mean that its atmospheric relevance is greater than other types of aerosols?

Dust is important because it contributes to the atmospheric aerosol loading more than any other type of particles. It is especially important in the Mediterranean region due to the proximity to the deserts of North Africa (Lelieveld et al., 2002). It is also important because along their trajectory from the deserts, many of the particles undergo changes due to chemical processes (e.g. sulfate coating; Levin et al., 1996) and/or attachment to other particles (e.g. sea salt; Levin et al., 2005).

P. 473, l. 9: Is it true that dust is the most effective IN? What about bioaerosols (e.g., bacteria). Recent reviews have shown that bioaerosols are more efficient IN than mineral dust particles via different heterogeneous ice nucleation mechanisms (e.g., Hoose and Möhler (2012), Murray et al. (2012) and Ladino et al. (2013))

Although biological particles are the most efficient IN, their concentrations in the atmosphere are relatively low as compared to dust aerosols.

Although biological particles have been found to be among the most efficient IN (e.g. Schnell and Vali, 1976; Levin and Yankofsky, 1983; Levin et al., 1987; Diehl et al., 2002), their concentrations in the atmosphere are relatively low. This makes them less likely to dominate the ice processes in clouds (Hoose et al., 2010). On the other hand, mineral dust aerosols are among the largest contributors to atmospheric aerosols (Goudie and Middleton, 2006). The presence of dust particles inside many ice crystals suggests that ice nucleation is often initiated by mineral dust aerosols in the atmosphere (Isono, 1955; Isono et al., 1971; Kumai, 1961, 1976; Twohy and Poellot, 2005; Cziczo et al., 2013).

P. 473, l. 13: The ice nucleating efficiency of an aerosol particle via deposition nucleation is not directly correlated with temperature. The relative humidity with respect to ice (RHice) at which ice nucleation is observed is commonly used to infer particle's efficiency. I suggest separating the literature studies and the conditions at which mineral dust was found to nucleate ice as function of the different nucleation modes (e.g., immersion freezing, condensation freezing, contact freezing and deposition nucleation).

We added a list with different examples of mineral dust that were found to nucleate ice as a function of the different nucleation modes

Dust particles have been observed to nucleate ice at different heterogeneous nucleation modes: deposition freezing (e.g. Möhler et al., 2006; Kulkarni and Dobbie, 2010; Kanji et al., 2013), condensation freezing (e.g. Roberts and Hallett, 1968; Levi and Rosenfeld, 1996; Zimmermann et al., 2008; DeMott et al., 2011), contact freezing (e.g. Pitter and Pruppacher, 1973; Ladino et al., 2011) and immersion freezing modes (e.g. Pitter and Pruppacher, 1973; Marcolli et al., 2007; Lüönd et al., 2010; Broadley et al., 2012; Pinti et al., 2012; Welti et al., 2012; Kanji et al., 2013).

P. 473, l. 26-28: These sentences are grammatically incorrect. Re-phrase it.

We corrected the sentences

Ground measurements of IN concentration by Levi and Rosenfeld (1996) using a thermal diffusion chamber at -15°C reported similar IN concentrations to those reported by Gagin (1975). Levi and Rosenfeld (1996) found that the concentration of IN during dusty periods was more than double than those found during non-dusty periods.

P. 473, l. 29: Condensation freezing is not defined.

We added a definition for condensation in the introduction

P. 474, l. 3-4: Re-phrase it: "…to characterize the efficiency of the eastern Mediterranean aerosol particles to act as IN via immersion-freezing under different meteorological…"

We Re-phrased the sentence

The aim of the present research is to characterize the efficiency of IN in the Eastern Mediterranean area in immersion freezing mode in dust storm days and during days without dust storms.

P. 474, l. 4-5: Add the meteorological conditions that were tested.

We decided not to use the term meteorological conditions but rather to specify that the measurements were done during days which were classified as dust storm days and days in which dust storms did not occur.

The characteristics of the research area

P. 474, l. 6: Remove "the": Characteristics of the research area.

We removed the word.

P. 474, l. 7: Add months. "…during 01.2009 and 12.2010".

We added Jan 2009 to Dec 2010

P. 474, l. 13-16: This part is confusing. It reads as if the aerosol particles from the Sahara desert and from marine environments were anthropogenic, even though they are clearly biogenic sources of aerosol particles. Please re-phrase it.

We Re-phrased the sentence.

The Eastern Mediterranean region is characterized by air masses arriving from different sources (Lelieveld et al., 2002). Many of these air masses often contain aerosols from distant and local anthropogenic sources (Levin and Lindberg, 1979; Graham et al., 2004). Some contain dust particles from the Sahara desert (Ganor, 1994; Levin et al., 2005) while others contain marine and biogenic aerosols from the Mediterranean Sea (Levin et al., 1990) and from land sources (Ganor et al., 2000).

P. 474, l. 19: "…and anthropogenic aerosols with a relatively…"

We corrected this sentence.

P. 474, l. 24: What do the authors mean by intense?

This was changed to: *more common with much lower visibility and much higher aerosol loading*

P. 475, l. 3: What do the authors mean by episodes? Can the authors be more explicit? (e.g., "dust storms or days with dust concentration larger than…")

The term episode was replaced with dust storms.
According to Ganor (1994) the word episode represents events with high mass loading of suspended dust. In the text we define what we mean by this in terms of the PM_{10} values and the back trajectory.

P. 475, l. 4: Is a dust-depositing storm the aforementioned episode of dust? If yes, be consistent with nomenclature.

We replaced the dust depositing storms with dust storms

P. 475, l. 4-7: This long sentence can be divided in two sentences.

We divided the sentence

Dust storms are most common between December and April (Katznelson, 1970) with maxima occurrences in spring time, mainly during April (Ganor, 1994). During the summer very few dust storms occur (Ganor et al., 1991).

P. 475, l. 7-9: Is it possible to re-phrase this sentence. It is a bit confusing.

We Re-phrased the sentence

Although during dust storms mineral dust particles are present in high concentrations, such dust aerosols are almost always present in the atmosphere in this region (Levin and Lindberg, 1979).

Method of analysis

I suggest adding a subsection about the uncertainties. The uncertainties should be added to the corresponding figures.

A standard deviation values were added to all the figures

P. 475, l. 14: Why is the sample flow 20 LPM and not 8 LPM as in Ardon-Dryer et al. (2011)?

In the Antarctica campaign reported by Ardon-Dryer et al. (2011), we were required to limit the electrical power and to minimize the weight, thus a smaller pump was used.

Previous work from our group (not published) found that a collection of 400 liters is needed to make a proper analysis. This is why we used a pump of 20LPM for 20 min for each sample.

P. 475, l. 18: Is it possible to add to Table 1 such conditions (i.e., polluted, clean, and the most relevant meteorological conditions for each sample)?

A column was added to table 1 which describes the conditions under which the samples were collected.

P. 475, l. 19: The sampling time is not clearly mentioned. Was it constant for each sample? If not, it can be added to Table 1.

The sampling time was 20 min for all the filters. We clarified it in the revised manuscript.

P. 475, l. 20: Is it the size range in radius or diameter? Please clarify it.

The particle size is the diameter. This was clarified in the revised manuscript

P. 475 and 480, l. 20: The lower limit detection of the 3010 CPC is 10 nm which means 0.01 μm and not 0.1 μm.

We thank the reviewer for this comment. We forgot to mention that on the CPC we used a Particle Size Selector (TSI, Model 376060) with a few screens that selectively remove small particles while passing through particles larger than 0.112 microns.

The aerosol total concentration was measured by TSI Condensation Particle Counter (CPC) Model 3010, which was located next to the filter sampler. In order to measure the concentration of particles in the size range of 0.11-3μm, a TSI Particle Size Selector Model 376060 with a number of screens placed in the front, were used to remove particles smaller than 0.112 microns.

P. 475, l. 21: "operating" is not appropriate.

Instead of operating we changed the word to located.

P. 475, l. 22: "…concentration (Nt) of the aerosol particles…"

We corrected it.

P. 475, l. 22: Was it Nt determined or measured?

Nt was measured.

P. 476, l. 7: I suggest to re-phrase "for measuring ice nucleation". I think that it the following would be more clear: "to investigate/study ice formation by deposition nucleation and by condensation freezing".

We re-phrased it

P. 476, l. 7-10: Which modifications were needed to study Immersion freezing with the FRIDGE-TAU?

We had to change the program controlling the temperature in order to modify the cooling rate to allow the temperature to decrease at 1°C per minute. In addition, the program controlling the camera had to be modified to allow an increased rate of picture taking.

P. 476, l. 13: Provide the revolutions per minute used in the shaker.

The ultrasonic cleaner was Sonicor SC-52T, which operates at 60Hz.

P. 476, l. 13: How accurate is this assumption? Did the authors further shake the same filter for another 30 minutes (or longer) to measure the resulting particle's concentration? Was it zero? Or, did the droplets from the new solution (i.e., the solution resulting of extra 30 minutes of shaking) freeze at the same temperature as pure water drops?

The method used in this paper assumes that the efficiency of removal of particles from the filter is close to unity. It is based on experiments (not published) that were carried out about 15 years ago. Following the comment by Vali, we decided to repeat this experiment in order to re-check our assumption. Unfortunately, since the laboratory of Prof. Levin at Tel Aviv University has been shut down a few years ago we used instead the facility made available to us at Dr. Bingemer lab at the University of Frankfurt. Although the facility is not identical to the one used

in this paper, the general characteristics are similar, namely, the stage in the FRIDGE, the temperature controller and the camera are similar. On the other hand the double distilled and deionized (DDI) water is of slightly poorer quality than the one used at Tel Aviv University. Nevertheless, we decided to run a number of tests as described below with each test composed of more than 130 drops.

A volume of 384L of air containing Arizona Test Dust (ATD) particles were deposited on Nitrocellulose Membrane Black filters of 47mm diameter with 0.45μm pore size (the same one used in the paper). The filter was put into the DDI water (resistivity of 15.87 MΩ·cm) and placed for 15 min in an ultrasonic shaker, thus exposing the samples to similar conditions used in the paper (this was done following consultation with the manufacturer of the ultrasonic shaker).

Drops containing aerosols from the water were placed on the FRIDGE's temperature controlled stage. The temperature was lowered at a similar rate to the one we used before and the temperature at which the drops froze was recorded (named ATD - after 1 ultrasonic shaker cycle). Then the filter was placed in a new test tube with fresh water and put in the ultrasonic shaker, repeating the procedure above. The freezing temperature of the drops was recorded (named ATD - after 2 ultrasonic shaker cycles). The experiment was repeated again for a third time (named ATD - after 3 ultrasonic shaker cycles). In addition to the above tests, similar procedure was carried out using clean water with no filter and a clean filter without aerosols.
In these experiments we assume that the nucleation is deterministic, namely it is enough that one nucleus is active at a certain temperature for the drop to freeze. The results of the freezing fraction after the three shaking procedures are presented in Fig.1.

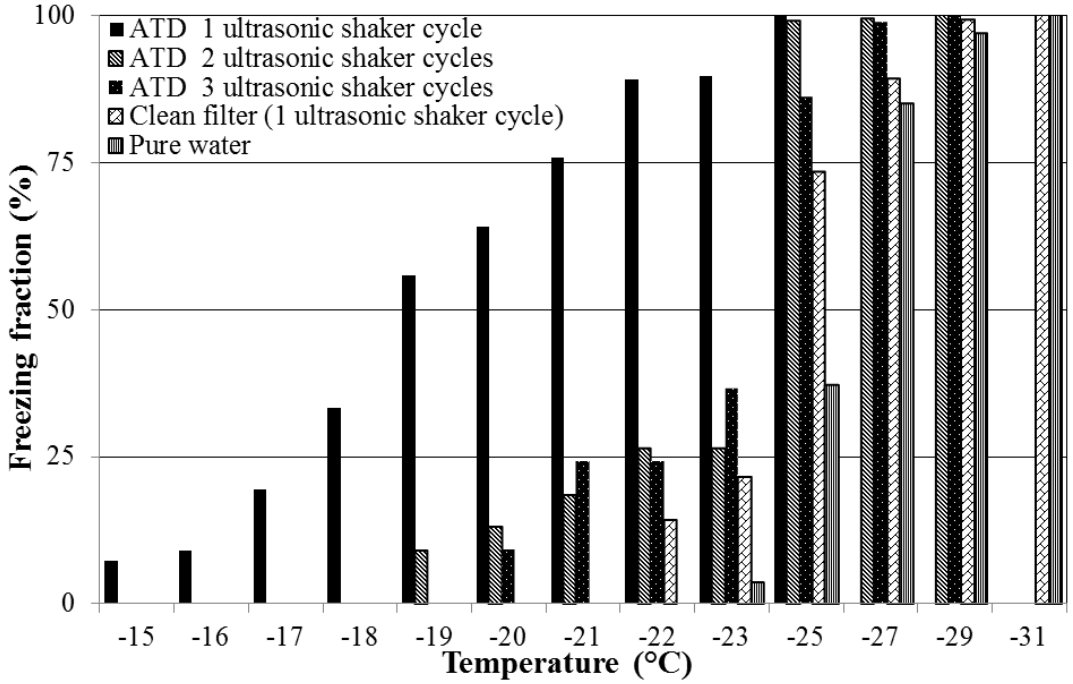

Fig 1: The freezing fraction as a function of temperature in the experiment testing the removal efficiency of the particles from the filter. In black, ATD particles after 1 ultrasonic shaker cycle, in dark downward diagonals ATD particles after 2 ultrasonic shaker cycles, ATD particles after 3 ultrasonic shaker cycle white points on the black background. Clean filter experiment (after 1 ultrasonic shaker cycle) in diagonal bricks and pure water in the narrow vertical lines.

From Fig 1 one can see that down to a temperature of about -19°C there were no freezing drops after the second and third shaking. At around -20°C about 10% of the drops froze after the second shaking and this number increased to almost 25% at -22 to -23°C. However, we can also see that the clean filter and the clean water started freezing also at the latter temperatures. This indicates that it is very likely that some of the drops frozen after the second and third procedure at these lower temperatures were actually nucleated due to the contamination in the water. This was not the case in the original experiment reported in the paper where drops started to freeze at lower temperatures (see Fig 4 in the original paper).Thus, based on the present tests it is difficult to evaluate the efficiency of removing the aerosols active at these relatively low temperatures. However, it seems that most particles active at the higher temperatures have been removed in the first run. Of course more careful experiments using different aerosols, different ultrasonic

shaking times and different ultrasonic power are needed. But this will have to be done as a separate experiment.

In the revised manuscript we added a few sentences addressing this issue:

Each filter containing the collected aerosols was placed in 10ml of double distilled water (resistivity of 18.2 MΩ·cm). The aerosols were then removed from the filter by an ultrasonic shaker. The use of the ultrasonic shaker was found to be effective for particle removal into the water solution. This method which is more aggressive then the removal method used by Vali (1968) was found to be effective in removing all of the most effective particles after only one cycle of shaking in the ultrasonic bath.

P. 476, l. 16: How were the droplets placed on the stage? Did the authors use a syringe? Please clarify it and provide the needed missing information.

The droplets were put on the stage using a pipette; we have added this information to the method section.

P. 477, l. 25-26: What do the authors mean by freezing mode? There are several freezing modes, please clarify it.

We added the word immersion-freezing.

P. 477, l. 26: (total of 10 mL without extra shaking)...

We added it to the sentence.

P. 478, l. 6: "…because of the decrease in the number of immersed particles per droplet, and hence a decrease in the available surface area (Pruppacher and Klett, 1997)".

We modified the sentence as suggested by the reviewer. We do not think that the reference of Pruppacher and Klett is appropriate here.

Results and discussion

Why is there not any data for the 2009 and 2010 summer seasons? Although the frequency of dust storms is very low during the summer season, it may be important as a background measurement. The sampling time for each filter needs to be provided (Add to Table 1).

The filters were sampled during conditions of dust storms, clean and polluted days (e.g. Lag BaOmer) or just randomly to represent other "regular-background" conditions. Five samples were collected on days that do not fit our strict definitions of dust storms, clean or polluted days. Thus, the results from the dusty conditions could be compared to days without dust, which resemble summer time conditions.

It is worth mentioning that another paper is in preparation in which a two year campaign of daily IN measurements in the deposition and condensation modes using the FRIFDGE-TAU will be presented. Together with the present paper it will allow us to compare different seasons and the ice nucleating efficiency in different nucleation mode.

Regarding the sampling time, all filters were sampled for 20minutes; we added this information in the revised manuscript.

Using Equation 1 and assuming a sampling time of 2 hours, a droplet volume of 1µL and a pump flow of 20 lpm, resulted in FN values which are one order of magnitude smaller than the reported values in Figure 6. A FN concentration of 0.03/L was found when 1 drop freezes and FN is 20/L when 139 drops freeze. Is there anything wrong in this calculation?

In our experiments each sample was collected for 20min, not for 2 hours. Using the correct sampling time, if one drop freezes at -15°C, K' in equation (1) is $0.175L^{-1}$. If 139 drops freeze at about -26°C than K' is $120L^{-1}$. Both values are similar to the numbers one gets from Fig 6 (in the original manuscript). If, on the other hand, the sampling time is longer and thus the total

collected aerosol mass is larger, each drop in the sample will contain a larger number of particles capable of freezing at the higher temperature. This means that when the temperature is lowered to say -15°C, not one but many drops will freeze almost simultaneously. Of course in such a case, the sample should be diluted in more than 10ml of water and probably many more drops would be needed to improve the resolution.

This section can be divided into subsections (e.g., Onset freezing temperatures, Clean versus polluted days, Median freezing temperature, Ambient IN concentrations). I think that a new Figure where the IN concentrations are plotted as a function of time can be added. It will be interesting to see how the IN concentrations change every month, by seasons, and between the 2009 and 2010.

We made many changes in the revised manuscript. There will not be a section that describes in detail the onset freezing temperatures and median freezing temperature. Instead, this information will be presented in table 2 in the revised manuscript. In addition, following the comments by the reviewers we have added a new section that covers the Lag Ba Omer event. Furthermore, an additional explanation on the connection between ice nucleation and particle's surface area has been added

Since we had only 19 days, we do not think that having a plot of IN concentrations as a function of time is informative enough.

P. 479, l. 3: "The drops containing the collected ambient particles began to…"

The sentence was changed.

P. 479, l. 4: Bioaerosols is too broad. It is better to be specific." …some bioaerosols such as bacteria and leaf litters (e.g…"

We accept the change and added it.

P. 479, l. 3-9: The authors move back and forth between onsets and spectra. Along the paper, the authors mentioned/discussed three variables to address the IN efficiencies of their ambient particles: onset freezing temperatures, freezing at which 50% of the droplets freeze (It could be called "median freezing temperature") and also the number of immersion freezing IN. In the discussion, the above mentioned variables are combined, even in the same paragraph, making the manuscripts confusing to read.

We have changed the new revised manuscript, the onset freezing temperatures and median freezing temperature will only be presented in table 2 in the revised manuscript. We focus our attention on the IN concentrations and the activated fraction.

P. 479, l. 5-6: Do the authors expect that the chemical composition of the particles measured by DeMott et al. (2006) in the Arctic are similar to the aerosol particles measured in this study? What could be the reason of the similarity between the freezing spectra from both studies?

We have taken this part out of the paper based on the comment and suggestion in the review of Paul DeMott (reviewer 2).

P. 479, l. 5-12: The elemental composition of your samples will fit nicely here.

A section with elemental composition analysis that we had for two days, was added to the revised manuscript.

P. 479, l. 8: What is the author's definition of onset?

Onset represents the temperature at which the first drop freezes. We will clarify it in the new revised manuscript.

P. 479, l. 10: Replace “the immersion-freezing” with “ambient aerosols”. The authors are investigating ambient particles from Israel and they are compared them with soot. Re-phrase it

The sentence was changed.

P. 479, l. 19-21: This is confusing. Prenni et al. (2009b) did not observed biomass burning on their TEM grids.

We agree with the reviewer, this citation of Prenni et al. (2009b) was taken out of the new revised manuscript.

P. 480, l. 19 (and throughout the paper): I think it is better to use “activated fraction” instead of “activation fraction”.

We agree with the reviewer. This correction will be made in the new revised manuscript.

P. 480, l. 19-27: Why an activated fraction of one was not reach?

In laboratory experiments using known particle composition and size, one may be able to get activated fraction close to unity. However, ambient aerosol samples that are composed of particles of different composition and size, with only a few that are very good IN, cannot give an activated fraction of one.

P. 480, l. 29: 0.6? Are the authors referring to FN>0.6. Be explicit.

The FN concentration and AF values were found to increase with decreasing temperatures at a relatively high correlation coefficient of >0.6.

P. 481, l. 4: "…role in ice formation".

The word “the” was deleted.

P. 481, l. 15: “these” refers to clean days? It needs to be clarified.

The sentence was changed.

Samples were defined as clean days when PM_{10} daily average values and the value measured during the aerosol sampling were below 50 µg m^{-3} (Ganor et al., 2009) and the air mass trajectory in the previous 72 hours did not pass over a source of dust.

P. 481, l. 15-16 and 21-22: This is redundant. The same point is repeated in these two paragraphs.

These paragraphs were changed.

The filter samples were separated into dust storms and clean conditions based on PM_{10} values and the air mass back trajectory. The Back Trajectories (BT) were calculated for each measurement using the HYSPLIT method (Hybrid Single Particle Lagrangian Integrated Trajectory Model). Dust storm days were defined as days when the PM_{10} daily average values and the value measured during the aerosol sampling time exceeded 100µg m^{-3} (Ganor et al., 2009). In addition, the air mass trajectory in the previous 72 hours had to have originated over a dust source or passed over one. Samples were defined as clean days when PM_{10} daily average values and the value measured during the aerosol sampling were below 50 µg m^{-3} (Ganor et al., 2009) and the air mass trajectory in the previous 72 hours did not pass over a source of dust. It should be noted that in the research area the yearly average standard values of PM_{10} is 60µg m^{-3} (Israel Ministry of Environmental Protection, 2013).

Out of all the days that were sampled, eight days were defined as dust storm days with daily average values of PM_{10} from 254 to 867µg m^{-3}, with an overall average of 527±236. Five days were defined as clean days, with PM_{10} daily averages ranging from 30 to 39µg m^{-3} with an overall average of 34±3.8, as can be seen in Table 3. In the clean cases the air mass arrived

from the west or northwest, while on the dust storms days the air mass arrived from the south or southwest, as can be seen in Fig. 6.

P. 481, l. 17-24: What happened with the other six days? Can the authors comment on it?

The other days were not considered clean or dust storm days because they did not fit the strict criteria of PM values with air trajectory.

P. 482, l. 5: Is it 1.8C within the temperature uncertainty?

The difference of 1.8°C is larger than the uncertainly in temperature. In fact T test calculations show that the onset of freezing and the median freezing temperature between the clean days and dust storms days were significantly different from one another.

P. 482, l. 6-7: Is there any experimental evidence that montmorillonite was present in the measured ambient particles during the dusty days?

This is based on measurements of Ganor (et al., 2009), who found a high frequency of montmorillonite particles in dust storms in our area.

P. 482, l. 11: "that some". Is it possible to be more quantitative?

The whole paragraph will be deleted in the new revised manuscript.

P. 482, l. 12-13: Why is it the elemental composition not shown? This is very important. The elemental composition could help the authors to interpret their data. Which was the measured NaCl mass compared to the total mass?

Although this paragraph will be deleted in the revised manuscript, we added a paragraph discussing the elemental composition analysis that was carried out.

P. 482, l. 16-19: This is not completely true and needs to be corrected. For example, Gallavardin et al. (2008) and Wex et el. (2013) did not use neither ammonia nor ammonium sulfate. Additionally, the reduction of the IN efficiency discussed in the aforementioned studies is more related to deposition nucleation and not to immersion freezing. IN deactivation cannot be generalized. It strongly depends on the particles' composition, coating material, coating thickness, and the ice nucleation mode.

We decided to take out this paragraph from the new revised manuscript.

P. 483, l. 14-16: This is a very strong conclusion to make. I am not sure about the accuracy of this assumption. The IN concentrations from DeMott et al. (2010) were obtained using a continuous flow diffusion chamber (CFDC) which operates on a single particle basis. This is in contrast with the FRIDGETAU chamber which measures the IN concentrations from the bulk. It could be that the observed difference in the IN counts between the CFDC and the FRIDGE-TAU are due to different sensitivities in both instruments for detecting ice. It is notable that DeMott's data from the AMAZE-08 campaign (Prenni et al. (2009b)) are one order or magnitude smaller than the present observations. The IN measured during the AMAZE-08 were highly influenced by biological aerosols which are known to be very efficient ice nuclei.

Paul DeMott also requested that we change this paragraph. In the revised manuscript we removed figure 11 (in the original manuscript) and thus removed the comparison with some of the references mentioned in this paragraph.

<u>Conclusions</u>

P. 483, l. 19: "For the entire sampling period the ambient aerosol particles were found…"

We changed the sentence.

P. 483, l. 19: what do the authors mean by effective?

By effective we mean that the FN froze the drops in this temperature range.

Drops containing ambient aerosols were found to freeze between -11.8ºC down to -28.9ºC, with median freezing temperature that varied from -17.8ºC down to -24.4ºC.

P. 483, l. 19-20: "…-29C, with an average temperature at which 50% of the drops froze of -21C."

See our correction in the comment above.

P. 483, l. 20-21: "FN concentrations of…"

We changed the sentence.

P. 483, l. 23: what are the authors referring to by "case"?

We changed the sentence.

P. 483, l. 26: "…more effective as FN. This is…"

We changed the sentence

P. 484, l. 4: "Droplet containing ambient particles from dusty…"

We changed the sentence.

P. 484, l. 5: "...warmer temperatures than droplet containing particles from clean days."

We changed the sentence.

P. 484, l. 10: What is the meaning of background level in this context?

The sentence has been modified:

This observation agrees with previous studies showing that some dust particles are almost always present in the atmosphere in this region.

Table 1: Indicate that the CPC counts are the average.

Add the standard deviations of CPC, PM10 and PM2.5.

We added standard deviations for CPC, PM_{10} and $PM_{2.5}$ values.

Add a column where the sampling time is indicated.

The sampling time was the same for all filters. We added this information in the Method section of the new revised manuscript.

Add a column where each samples is categorized as polluted or clean.

A column that described the classification of each filter samples was added to table 1.

Figure 2: This figure was already published in Ardon-Dryer (2011). It must be cited. Is it possible to provide more details about this figure? A figure should be self explanatory. What is in the left and right part or the figure?

Following the recommendation of the reviewers, we removed this figure and only made reference to Ardon-Dryer et al. (2011).

Figure 3: Can the authors add error bars in both axis of the right Figure? Figure caption needs to be rephrased because it does not read well. It must be indicated that the spectra vary as a function of temperature.

Error bars have been added to the figure with an explanation in the text.

Figure 4: I think that this figure is not necessary. I suggest replacing it with Figure 9.

The figure was deleted from the new revised manuscript.

Figure 5: Based on my suggestion for Figure 4, I think that it makes more sense to plot the PM10, PM2.5 and PM10-2.5 average values under clean and dusty conditions and not for the whole data set as shown in Figure 5.

Since not all the filter samples were classified as clean or dust storm days, we believe that showing this figure could be important. This figure shows that when PM_{10} increases there is a higher chance of finding more effective IN particles.

In addition, the average values of PM_{10}, $PM_{2.5}$ and $PM_{10-2.5}$ under clean and dusty conditions were added to table 3 in the new revised manuscript.

Figure 6: I think that this figure is unnecessary. I suggest replacing it with Figure 10a.

Not all the days were dust storms or clean days, therefore we thought it would be interesting to show the range of IN concentration and activated fraction on the different days.

Figure 7: I think that this figure is unnecessary. I suggest replacing it with Figure 10b.

See our reply to the comment above.

Figure 9: Add error bars in the x-axis.

The figure was deleted from the new revised manuscript but error bars were added to all the other figures.

Figure 10: Add error bars in both axes.

Error bars were added to the figure.

Technical corrections

All the technical corrections have been adapted and inserted into the new revised paper.

REFERENCES

All the references mentioned in this reply have been added to the revised version of the paper.