Responses to Zamin Kanji (Reviewer 1)

First of all, we would like to thank the reviewer for the useful elaborated comments and suggestions which help to improve the new version of the manuscript.

All comments have been treated carefully by the authors, rewordings and deletions as suggested by the reviewer have been conducted, mentioned references have been added, and critical formulations have been reformulated or removed.

Where it is required, replies to some points are listed here:

Introduction: *This maybe true, but if you are going to mention it, you must provide a reason as to why is it important to have the freezing process observed while the droplets are already at their terminal velocity.*
That is not true for Ladino et al 2011 - drops are at their terminal velocity.
These critical statements have been removed from the revised manuscript.

Section 2.1.1: *This is unclear what the authors are trying to say, elaborate more?*
The ceramic surface of the Pt100 sensor does not show the same emissivity as an ice surface, this part has been elaborated in the revised manuscript.

Section 2.1.1: *Comment on at the atmospheric relevance of this stationary drop?*
The fact that the drops are very stationary in the acoustic levitator was described as an advantage because this allows to directly measure the surface temperature of the freezing drops. This is not possible with drops floating in the wind tunnel because of their movements in the air stream. Of course drops in the atmosphere are not stationary but freezing as a hydrodynamical process should not be affected. A comment has been added to the paper.

Section 2.1.2: *you should report how long it took the pure water drops to freeze above -35°C*
We never observed pure drops freezing above -35°C for time periods as long as 60 min.

Section 2.2: *Why choose 4s, when from your figures 5, it is clear that the time is closer to 5 seconds rather than 4?*
The reviewer is right, this has been changed in the revised manuscript and corresponding Figures 6a and 6b have been modified accordingly.

Section 2.3: *Mont K10 from sigma-aldrich are known to be acid treated and have different properties from untreated montmorillonite. This sample is not representative of any atmospheric*

dust. Something to this effect is mentioned in Pinti et al (2012), why was this not taken into consideration?

These experiments were performed first in the very beginning to use montmorillonite K10 as a test substance to prove the function of the levitator. At that time we were not aware that it is not representative of atmospheric dust.

Section 2.3: *I think we know that sometimes the manufacturer's quote inaccurate compositions for various reasons. I am certain upon talking to various people from the INUIT community that there is more than just a trace of Feldspar. I suspect there is more Feldspar in this sample and this should be confirmed with XRF or XRD, should be easy to get a sample analysed.*

Based on analyses from an INUIT group, the numbers have been changed in the revised paper and the references are corrected.

Section 2.3: *Shouldn't this be (Hiranuma et al., 2014, in Prep) I think his publication is almost ready to submit.*

In the mentioned paper, illite IMt1 is not included but only illite NX.

Section 3.1.3: *This is very vague - the lines intersect at one point, but do not overlap, the slope is completely different therefore the temperature T50 dependency on SA is different too. You have to comment on this and offer some explanations as to why this may be.*

According to another reviewer's suggestion, the comparison with the median freezing temperatures of Broadley et al. and previous Figure 12 have been omitted and replaced by comparisons with the results of Knopf and Alpert (2013).

Section 3.2.2: *Why third order regression curve? Why not use the exponential function for Ns(T) that previous studies have used before, like Niemand et al., or Tobo* et al (2014) ACP)

We had the impression that the data points are represented best by a third order regression curve and followed Broadley et al.

Section 3.2.3: *Shouldn't you discuss on the suitability of which model is better used to represent your results, the time dependent or independent model?*

Some remarks about this issue have been included in the revised paper, in Sections 3.2.1 and 3.2.2.