This study is a laboratory experiment measuring the ice-nucleating properties of mineral dusts coated with SOA, as determined by 3 separate types of instruments, the PINC, CFDCs, and the AIDA cloud chamber. The experiments appear to be well designed and meticulously carried out, and the redundancy of multiple ice nucleation instruments, while not necessary, strengthens one's confidence in the results. There are some differences between the analyses of data from the separate instruments, and also the interpretation of these results in light of previous work which require further clarifications, as discussed below. Once resolved, this paper will make a good contribution to the literature.

The topic of deactivation of ice nucleation activity by coatings has been reported in a number of previous manuscripts. The main conclusion from this paper is that SOA-type coatings do not deactivate the INP activity of 2 types of representative mineral dusts. This is opposite to the previous conclusions of many papers that report that deactivation does occur. However, this study is specific to measurements in the mixed phase cloud regime.

Major Comments:

1. The most interesting question in the manuscript is WHY does deactivation occur in some studies and not this one? My major comment is that this question warrants more attention and more organized discussion than is included in the current manuscript. References are made to other studies here and there throughout the text, but these are not summarized or reported in the context of all other studies making it different to draw any overarching conclusions. A systematic analysis or at least discussion of what does and does not lead to activation is needed.

A. A thorough discussion on the thicknesses of coating here compared to those in papers which did observe deactivation would greatly strengthen this paper. (Of course, such a discussion is dependent on availability of coating thickness (or aerosol mass/density changes such as those obtainable with a particle mass analyzer (PAM)) observations. The manuscript does conclude that no effect of coating thickness was observed over the range of 3-60 nm. However, it is possible that other studies were conducted on even thicker coatings. Is this known?

B. The multiple measurements (PINC, CFDC, AIDA) are in general agreement, which suggests that the reported fractions frozen and ice nucleation active site densities are relatively accurate. It follows that the reasons that deactivation of INP efficiency was not observed is related to sample generation. Are the substrate aerosols similar in size distributions to previous measurements? How does coating method compare to previous studies?

C. It may well be that some aerosols are more receptive to coatings that others, for either chemical or physical reasons. Chemically, acidity will vary with atmospheric coating compositions. Also, the sticking coefficient on a Teflon aerosol (for example) would be much lower than dust (note that this would not explain why the dust doesn't deactivate. Physically, it is possibly that highly irregular shaped particles and/or highly porous particles may be more difficult to coat and therefore less likely to deactivate as INP.

D. Are there any other reports of coatings NOT activating mineral dust INP?

E. Might deactivation of heterogeneous nucleation occur only for certain ice nucleation mechanisms not explored here? That too, would be interesting. For example, Sullivan (2010) observed very different effects of coating for super and subsaturated conditions, as mentioned on page. 3. The Sullivan immersion freezing results do show deactivation, which is different than the results here. How do the temperatures of the 2 studies compare?

The manuscript currently takes a broad brush on activation vs. deactivation (pg. 3 ln 15…" the effects of inorganic acid and organic coatings on a variety of mineral dust particles" are all reported in one lump statement. It would be interesting to more carefully consider how variations in substrate aerosol result in more/less deactivation and also how differences in coating compositions lead to different results.

2. Pg 4 ln 28 Why does the CFDC require such a high supersaturation (105%) to simulate immersion freezing? Also, for reference, the PINC operating supersaturation should be reported at the same point in the text. Later, it is reported that the PINC's droplet survival region is at 107% ss and higher. Was 107% the ss chosen for immersion measurements?

Further, I am confused about this survival statement- if droplets only survive at 107% and wetter, then how is all the Figure 6 and Figure 7 PINC data (at 105%) obtained?

3. Figure 8 and text page 13: The text says there is only one outlier below the 1:1 region on the figure. I see at least 2 outliers, one PINC and one CFDC.

4. pg 15. The conclusion that "observations of scatter between the 3 INP chambers can be attributed to differences in the evaluation of immersion…" appears to have been added as an afterthought. This is an important point and should be made and elaborated on earlier in the text.

Minor Comments

1. page 1 line 30. The last sentence in the abstract is grammatically incorrect. Revise.

2. page 4, ln 17. "There are no other studies in the MPC regime" I had to look back to find MPC defined. It should be written out. Also, since this statement is so central to the paper, the regime should be specified here, add "…that is, over the temperature range of …., and above water supersaturation range of …"(these values are currently provided later in the manuscript.)

2. page 15, ln 7, there is a misplaced phrase, (with T in C). Please revise sentence.

3. page 16, ln 6, "We note.." This is a run-on sentence that needs to be revised.