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Interactive comment on “Ice nucleating particles over the Eastern Mediterranean measured by unmanned aircraft systems” *by* Jann Schrod et al.

Anonymous Referee #2

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This manuscript reports on a new technique, determining ice nucleating particle concentrations at various altitudes from drones. The technique clearly shows promise, and the measurements are needed in this area. I have a few questions/concerns, which the authors need to address prior to publication. Then, the manuscript will make a solid contribution to the field.

Major Comments: 1. The abstract ends with a bold and interesting conclusion, that ground level INP measurements are of limited use in understanding INP aloft and their role in cloud formation. This may be true, but it is not substantiated by the data as presented. More could be done with the collected data set, as I describe below.

2. Pg. 5. The cruiser has a 2-stroke engine. 2 stroke engines are notoriously dirty running, producing significant pollutants, including NOX and likely particulates as well.

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Discussion paper

There is a serious concern that the emissions from the engine will be active INP and thus will contaminate the INP sample and bias the reported concentrations. What has been done to check, correct and/or avoid this?

3. pg. 6 ln 27. Sampling times and therefore volumes vary by a factor of 3. Why wasn't the sample time kept uniform? What impact does this have on INP concentration results? This needs to be addressed.

4. pg 10, ln 30- pg 11 ln 7 and Figures 11&12: The manuscript states the INP concentration is highly correlated to the concentration of large particle measured by the OPC and with the vertically integrated aerosol optical depth. Unfortunately, not all the data presented supports this conclusion. - This is the major issue with the manuscript.

In Figure 11, we see that INP correlations were essentially unchanged over the vertical altitudes sampled. (Particle concentrations are not included in the figure, but it is highly unlikely that they are equally invariable). When all the data is combined into one plot, interesting details are often lost, and I suspect that that is the problem here. Aren't there cases in which a dust layer aloft was sampled? Did INP concentrations increase with the dust layer? Yes or no? Perhaps dust layers were too thin to have an impact on the concentration of INP in any given sample? Or, did the drone miss the layers? Alternatively, what about contamination from the drone exhaust? Is that somehow causing falsely high values at lower altitudes (i.e. take-off and lands)

Rather than look at overall campaign correlations (i.e. Figure 12), it should be much more telling to look at cases. Likewise, correlations with particle counts are expected to vary vertically, especially when dust layers aloft are an order of magnitude higher than at ground level. The lidar is evidence that that as occured in some cases here, but the connection to INP concentration is not demonstrated.

In Figure 14, where a case study is presented, peaks in INP concentrations do not coincide with peaks in backscatter coefficient, especially the point at 10:00 UTC of 180 INP/std l. Any idea what happened at this point.

Backscattering coefficient can be complicated by many particle characteristics. - It would be nice to also include the OPC concentrations along with the INP concentrations. Also, in 14C depolarization ration and INP are not correlated in any way.

In summary, the vertical profile of INP may be contaminated by the drone's engine exhaust? And INP concentrations may or may not be sensitive to cases of dust events. Given the wealth of data collected here including all the key elements to really look at dust, dust size, and INP at multiple altitudes, I urge the authors to consider additional cases are available which support their statement that INP is highly correlated to large particles. If the cases do not support that correlation, other interpretations of the data should be considered.

5. On figure 12, it is impressive that the predicted numbers are lower than measurements in 12a and 12b, but well correlated in 12c&d. It would be nice to expand the discussion of how these were parameterized differently, as it seems that this result is a key finding of the manuscript.

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