

## ***Interactive comment on “Concentration and variability of ice nuclei in the subtropic, maritime boundary layer” by André Welti et al.***

### **Anonymous Referee #2**

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First of all, this is a very impressive set of measurements. The field of atmospheric ice nucleation is lacking long term data sets with which to compare models and test our understanding. Hence, a dataset comprised of 500 individual measurements over the course of 5 years is extremely valuable. Hence, I support its publication.

However, I think there are some aspects of the paper which need to be improved prior to publication. I go through these in detail below:

Specific comments:

1) P1. Ln 11. Why does an exponential change in INP concentration suggest that several groups of particles with different ice nucleating properties are contributing to INP populations? The exponential dependency on T could also just be explained by a

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distribution of sites across a surface, rather than particles of different composition or size.

2) Consider using the term ‘ice nucleating particles; INPs’ rather than ice nuclei (IN). Vali et al. [2015] in their recent definitions paper came up with some compelling reasons why this is a better and less confusing term.

3) P1 Ln 24. Provide a citation for 100 L-1 INP leading to diamond dust. My understanding was that diamond dust was a relatively low concentration of ice crystals of relatively large size, i.e. in contradiction to the statement made here. In addition, I understand diamond dust tends to form in clear air, without the presence of a liquid cloud.

4) P2, Ln 1. When discussing data like that of Ansmann et al. and making statements such as ‘above -10 C ice containing clouds are rare’, make sure it is stated what sort of clouds are being referred to. For example, in convective clouds ice formation above -10oC is common. Ansmann et al. deal with shallow clouds.

5) P2. Ln 3. This paragraph is very confused. Parts of it seem to be referring to ice formation in shallow cloud types (e.g. stratus), whereas it then morphs into a discussion about secondary production which is more relevant for deep clouds.

6) P2. Ln 17-25. This discussion of marine INP is lacking reference to some more recent literature on the subject, e.g.: [Burrows et al., 2013; McCluskey et al., 2017; Vergara-Temprado et al., 2017; Wilson et al., 2015; Yun and Penner, 2013]. I appreciate the effort made to go back to much older studies, but the new work also needs to be discussed.

7) P2. Ln 25. The statement that ‘From laboratory experiments it is established that dust particles tend to nucleate ice below -20C whereas biological particles can initiate immersion freezing at temperatures up to -5 C’ is wrong. I can point to numerous studies showing dust can nucleate ice at much warmer temperatures; e.g. [Atkinson et al., 2013; Niemand et al., 2012; Ullrich et al., 2017]. Modelling suggests that dust

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is important in many locations at much warmer temperatures than  $-20\text{ C}$  [Vergara-Temprado et al., 2017].

8) P2-3. It is not possible to distinguish between dust and bio INP on the basis of an inflection at  $-16\text{C}$ . It is false to claim that such an inflection would give you information about biological INP. In making this statement the authors are assuming they know what the ice nucleating spectrum of dust is and also that they know that biological INP nucleate around  $-16\text{C}$ . Neither can be assumed or are correct. Biological material has a huge diversity in its nucleating ability. There are exceptional ice nucleating materials from specific fungal and bacterial species and much less active materials associated with marine biology. Also, the work of DeMott et al. [2016] and Wilson et al. [2015] suggest that the slope of INP vs T for marine INP materials is quite shallow, in contrast to what is stated here.

9) P4. Ln 5. It would be helpful to see the control fraction frozen curves as well as the fraction frozen curves for the samples. These control experiments look better than those reported by Conen et al., why is this? What has been done differently?

10) Figure 1. Also show other INP parameterisations that are used in models in addition to Fletcher, e.g. Meyers et al, Cooper et al.

11) P10, In 15-23. In this discussion of the conclusion that the authors see no evidence for marine INP, they need to cite other papers with similar conclusions. For example, Fig 5 of Vergara-Temprado et al. [2017] clearly shows that desert dust is much more important than marine organic INP in the Eastern Atlantic region. Similarly to the final statement referring to Burrows, Wilson et al. [2015] also conclude that marine INP might be important in the southern ocean. They do not make this conclusion on the basis that marine organics are particularly good at nucleating ice, they conclude this because the southern ocean atmosphere has very little desert dust in it and marine organics therefore define INP population.

12) P11, In 15, Why are INP above  $-10$  biogenic? This statement needs to be expanded

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upon or altered. As mentioned above, mineral dust can nucleate ice in this temperature regime. 13) P12. In this discussion of Ansmann et al., make it clear that this -20C number is for shallow clouds only, not deep convective clouds. In contrast the OSCIP from Rango and Hobbs is for cumulus clouds. Consequently I think the link between these ground level measurements and mid-level clouds is not as clear as the authors suggest.

14) Conclusions: Some of these paragraphs are very short and there is a single sentence paragraph which seems to be floating and not connected to other statements. Hence, it reads more like a list of bullet points than a well-crafted conclusions section. This could be improved.

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