

## Review of manuscript acp 2017-783, Concentration and variability of ice nuclei in the subtropic maritime boundary layer. A. Welti et al.

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This manuscript provides a potentially valuable long series of IN data for an area from which there have been no previous IN measurements that I am aware of. The topic is suitable for ACP and the methods used appear to be satisfactory, although there is a suspicion that concentrations may be underestimated. The language and presentation are clear and the few changes that seem desirable will be listed at the end of this review. This is a good paper and I hope that some of the following comments might be helpful. Where the authors disagree with those comments, I would be happy to enter into a debate

Summarising, I recommend publication after the authors have made any changes that appear to be appropriate.

- 1. Abstract:** The paragraph that shows no relation between bulk chemical composition and IN concentrations is not very useful in the main text and certainly not in the Abstract. At a site where sea salt particles will be dominant by mass and IN active at temperatures relevant to cloud formation represent about one particle in a million, no other result would be credible.
- 2. Introduction:** *p1., line 21.* Is there any firm evidence for concentrations as low as  $0.01\text{m}^{-3}$  except in polar regions? In addition to the listed factors influencing the formation of precipitation, updraft velocities and liquid water content could be added. Cloud extent suggests horizontal extent – cloud depth would be better.
- 3. *p2 line 1:*** What concentration of ice crystals is necessary for ground-based detection? The presence of concentrations of IN in clouds at  $-10\text{C}$  will usually be similar to that of the concentrations in the air feeding the cloud of all biological IN capable of nucleation at that temperature. The list of potential IN from those sources continues to grow and concentrations must often exceed  $100\text{m}^{-3}$ .
- 4. *P2. line 5.*** Insert reference here: Hallett and Mossop, Nature 249, 26-28, 1974 who were first to investigate the effect experimentally and define the conditions necessary for it to operate. *Line 15* Hallett and Mossop defined that condition in 1974!
- 5. *P2. Line 19-22.*** In Bigg's 1973 paper, 3 years of continuous measurements in the Southern Ocean, south Indian Ocean and south Pacific were summarised. The measurements were later used by Schnell and Vali (J. Atmos.Sci., 33(8), 1554-1564, 1976) to show that the measurements revealed a strong dependence on biological productivity. Their interpretation of biological IN rather than dust as the main factors in the measurements is much preferable to Bigg's. This work brings up an important point in relation to your manuscript. The ocean measurements were made with membrane filters that are known to undercount the concentration of IN in a salt-laden atmosphere, yet concentrations in the biologically productive zones were considerably higher than those reported in your manuscript. Chlorophyll measurements in the vicinity of the Cape Verde Is reported by Ramos et al. to indicate strong biological activity and significant biological IN should become airborne by bubble bursting. Am I right in assuming that your method only gives a "yes-no" answer

for the presence of an active IN at temperature T? If so it needs to be pointed out that actual concentrations may be higher.

5. *p.3 lines 3-4.* A spectacular example of the changes in IN concentrations that can occur was published in J. Meteorol. 15, 561-562, 1958. This was later interpreted to be due to related to enhanced biological populations resulting from heavy rain, with a proportion becoming airborne. (Atmos. Chem. Phys., 15, 2313-2326, 2015).

6. *p.3 line 11-12.* Long sampling times aren't necessary. Membrane filters with pore sizes 0.45µm or larger can be sampled at >10l/min but sampling >300l leads to serious undercounts. For long-term measurements or simultaneous measurements at many sites, sampling at 300l/day avoided logistic difficulties but averaged out any short-term fluctuations.

7. *p.3. Line 14.* Reduction of the RH in the vicinity of a hygroscopic particle is a major factor. Allowing hygroscopic material from a 1m<sup>3</sup> sample on a membrane filter to diffuse into an underlying wet filter, then drying the top filter and processing it, results in an IN count more than a factor of 2 higher than on a simultaneously sampled filter kept dry. (*This work has not been published – use the information if you want to*).

8. *p.5, line2.* At first I didn't understand this as all particles capable of forming IN at temperatures warmer than the test temperature will be activated. Does the answer lie in comment 5?

9. *Figure 2.* According to Ramos et al. chlorophyll is a maximum at the end of the year at Cape Verde. I don't see much evidence of a corresponding change in the -8C figure. As it is an important point in determining whether biological IN are effective at the site, running means of about 9 measurements at -8C shown on a diagram with a more extended scale might help. This procedure would be useful in reinforcing the surprising statement in lines 6 and 7 on p.8.

10. *Bulk chemical composition.* See comment 1.

11. *Air mass origin.* How many cases were involved in each of the 7 categories of figure 4? The sporadic rainy season from August-September would probably lead to much deeper atmospheric mixing at times and occasional scavenging of aerosol. How reliable are the 10-day trajectories to the site during that period?

12. *Frequency distribution, p.9 lines 7-9.* Size distribution of particles produced by a common method frequently have a log-normal size distribution and this can be expected from a local source. An alternative to Ott's random dilution hypothesis might simply be preservation of the original distribution during transport.

13. *p.10, line 6.* What is the minimum concentration of ice crystals needed for the lidar observations to detect them? It might be better to replace "to start" with "to be detected".

14. *Figure B1, p.14.* It might be interesting to have a separate diagram for the "rainy" season of August and September and for the period of maximum productivity, October-December.

#### **Minor typographical and construction errors.**

p.1 line 22 : change "cloud extend" to "cloud depth".

p.2 line 26. Change “consist to” to “consist of”.

p.5 line 14 “exemplary shown”. Change to “exemplified by”. Change “year” to “years”.

p.8. line 6. Change “effected” to “affected”.

p.9 line 20. Change “to identify” to “identification of”.

p.10. line 10. Change “to identify” to “in identifying”. Line 19: change “must not” to “need not”.