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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Ice nuclei in marine air: bioparticles or dust?" *by* S. M. Burrows et al.

Anonymous Referee #2

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In this work the authors propose a new parameterization of biological ice nuclei (IN) emissions from marine biological sources, and use it to assess the importance of biological IN against dust IN in marine environments. The authors conclude that biological sources would dominate. Although somehow simplified, I find this work interesting and the issues it raises relevant for the scientific community. Therefore I recommend its publication in ACP after some comments have been addressed.

1 General Comments

1- The authors use retrieved POC and in-situ measurements of IN concentration to develop a parameterization of marine IN emissions as a function of POC. Then the simulated POC is used to estimate the IN concentration, which by design is in agree-

ment with the in situ data. Instead of going throught this circular procedure wouldn't it be easier just to directly compare the estimated dust-IN concentration against the in-situ data for marine IN? If the idea is to simulate the global distribution of marine IN where in situ data is not available then this should be validated using an independent data set. Is the simulation able to reproduce at least qualitatively the distributions of Figure 1? Why the other data sets are not used to validate the simulation?

2- The fact that marine IN concentration could be as high as dust IN concentration over the Southern ocean does not neccesarily mean that they would be as important for cloud formation. Biological particles can be easily incorporated into cloud droplets and be scavenged out before they can reach low temperature. On the other hand, free tropospheric IN may be more likely to influence cloud formation at low T. This needs to be more clearly stated in the paper. It also seems that the supplemental material is quite relevant for the discussion. The authors may consider including it into the main text.

3- The simplifications behind Equations 2 and 3 should be clearly stated. I would think that a general expression $\alpha = f(POM, POC, EF, \mathrm{Seasalt}, \mathrm{T}, \mathrm{RH}\ldots)$ is more appropiate as there is no reason to believe that the relations are linear. Then some assumptions may be introduced to arrive to simple expressions like Eqs. 2 and 3, but the procedure must be explicitly shown. I also find puzzling that the authors didn't consider temperature and relative humidity as determinant factors to calculate α . If only immersion freezing is considered then it must be clearly stated (as condesation and deposition would depend on RH). Still the temperature dependency is missing, which is clearly significant, as shown in Figure A1.

2 Specific comments

Page 4374. Line 13. I think the analysis shows that IN emissions from biological sources can be as high as from dust sources, however whether IN concentrations are actually determined by them is a different issue (See comment 1 above).

Page 4378. Line 15. Why is this particular temperature selected?

Page 4378. Line 24. Typically, the IN concentration changes several orders of magnitude with temperature, using $T = -15^{\circ}$ may overestimate IN at higher T by much more than a factor of 10 (as for example shown in Figure A1).

Page 4380. Line 4. The rationale behind these equations is not clear, and must be clearly stated.

Page 4383. Line 8. I am not sure what T42L90 means, but if it is 4° by 5° it may be too coarse. This must be justified.

Page 4385. Lines 14-18. This statement is confusing. Please rephrase for clarity.

Page 4386. Lines 6-9. This is agreement by design. B73 data were used to generate the IN emissions parameterization. Is there an independent data set that can be used for validation?

Page 4386. Line 13. This is confusing. First, I am not sure what the authors mean by the absolute value of the IN concentration. Second I don't understand how it can be consistent with a large uncertainty. Please rephrase.

Page 4386. Lines 14-16. A plot with the spatial distribution of IN concentration would better support this statement. Also Figure A1 shows a T dependency of IN concentration which the authors largely neglect in this work.

Page 4387. Lines 7-8. It must be clearly stated that this refers to the marine contribution to IN total emissions, regardless of whether they are actually incorporated in a

cloud. The actual effect can be different since marine IN are emitted from below (as oppose to dust IN which come from above) and therefore more easily scavenged by liquid activation processes.

Page 4387. Line 27. Why 1%? Is there an study to support this?

Page 4388. Line 9. What do you mean by the main text?

Page 4389. Line 9. These statement implies several assumptions that must be clearly stated. The impact of marine IN emissions would be significant only during the Austral winter when the temperature is low enough to render significant ice nucleation in the 800 m boundary layer. Also mixing with sulfate would make these particles good CCN and limit their effect as IN. The latter process could also deactivate the IN active sites.

Page 4390. Line 12. Is there a reason to believe that contact nucleation with biological IN may be effective in this case? Actually I would imagine that if the biological IN are hygroscopic (which they tend to be) then at the moment of contact they would be coated with water. Thus, the surface tension difference between the droplet and the IN, which is believed to enhance ice nucleation activity in contact as compared to immersion freezing [e.g., Tabazadeh et al., 2002], would be small and marine IN may not be more effective contact IN than immersion freezing IN.

3 References

A. Tabazadeh, Y. S. Djikaev, and H. Reiss. Surface crystallization of supercooled water in clouds. 2002. PNAS 99 (25) 15873-15878; doi:10.1073/pnas.252640699

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 4373, 2012.