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

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Of the three main components of this paper - global chemistry/climate model, aerosol emission rates and ice nucleating activity – these comments address only the last. That is probably the major source of uncertainty in this work due to the paucity of data.

The first difficulty when discussing heterogeneous ice nucleation is that the term covers a phenomenon that may take place, as is well known, over a wide range of temperatures (and to a lesser extent supersaturations). The number of potential sources of ice nuclei is significantly greater at lower temperatures than close to the melting point. Therefore, it leads to a great lack of clarity when ice nucleation is described and various substances or aerosols are compared without regard to that fact. The review of ice nucleating activity in this paper suffers from this. Only in one place (page 4378, line 15) is the temperature specified for the activity that is being modeled.

Accepting the choice of -15°C (to be re-examined later) as the target for assessing IN populations from marine sources filters the potential sources that are considered. This in turn requires that the results and the methods of measurement in earlier work be examined in more detail than has been done here. Several of the biogenic sources considered in the paper do not show significant levels of activity at -15°C. The diatom and phytoplankton samples of Knopf (2011) and Alpert et al. (2011) yielded measurable activity only at temperatures lower than that. Leaving those sources aside, the only identified sources are those reported by Schnell and Vali(1975), Schnell (1975) and Fall and Schnell (1985). It is also significant that Fall and Schnell found little IN acitivity in several sea water samples and that the they considered it uncertain whether the one species of bacteria found to be highly active was truly marine or terrestrial origin. Parker et al. (1985 Antarctic J. 126-128) found one bacterial species active above -10°C. In all, the evidence is very sketchy for what marine microorganisms produce IN activity. Other data come from measurements of ice nuclei in the air; with inferences about possible connection to biological activity or chemical analyses of similar aerosol. On the other hand, the impacts of IN on precipitation and climate also depend strongly on the temperature at which their activity becomes appreciable. This is where some of the biogenic sources are unique and differ from dusts in general. Therefore, the potential climate impacts of these IN should be considered with their regime of activity taken into account. This paper oversimplifies the issue by focusing on one temperature of activity only, and one that is lower than the domain of greatest potential impact of the biogenic IN. Consequently, this paper misses the main point.

The results of Junge and Swanson (2008) do not show inhibition, as this paper states. Junge and Swanson found little activity for the several strains of bacteria and one virus isolated from Arctic ice-core and Antarctic ice samples raised nucleation temperatures above that of homogeneous nucleation but by only a few degrees Celsius. These results prove that the species tested do not make a significant contribution to atmospheric IN, and the discussion provided in their paper assesses the importance of these findings very well. The paper deserves to be considered more carefully in the work here

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reported if only to better bound the range of possible sources of IN.

It would be helpful to more emphatically differentiate the two lines of evidence pointing to the possible role of biogenic IN, namely samplings of source material and IN counts in the atmosphere. The latter line of study relies on measurements taken at low temperatures due to limitations of instrumentation, it uses correlations or chemical analyses to examine the biogenic link. Those are important weaknesses, yet these measurements constitute more direct data on what is in the atmosphere than source identifications. The paper would benefit from a discussion of the pro and cons of the two approaches. The limitations of atmospheric IN measurements are widely documented specially when referring to methods used several decades ago. Section 3.6 addresses this issue reasonably well but can hardly do justice to such a complex problem. In any case, it would seem prudent to acknowledge these large uncertainties early on and not allow readers to be misled by the relatively narrow ranges specified in Table 1 for the scaling factors.

pg 4374 ln 4 and pg 4375 ln1: While 'anecdotal evidence' appears ever more frequently in the scientific literature, it is basically a self-contradictory expression and is certainly out of place in referring to previous data on marine IN. Those publications present data, not hearsay, and albeit incomplete, they are based on measurements.

pg 4376, ln 12: Contrary to what is said in the manuscript, Schnell (1975) states that some of the species tested were specific cultures.

pg. 4376, ln 20: While Rosinski et al. (1987) indicate that some of the IN evaporated in vacuum they also say that the this does not exclude the possibility of biogenic origin of some component. The Rosinski et al. references are mistakenly cited as J. Atmos. Sci. whereas they should be J. Aerosol Sci.

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