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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A comprehensive characterization of ice nucleation by three different types of cellulose particles immersed in water: lessons learned and future research directions" *by* Naruki Hiranuma et al.

Anonymous Referee #2

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The authors present laboratory results on heterogeneous ice nucleation triggered by three different types of cellulose (MCC, FC, and NCC). They use 20 different methods to measure the ice nucleation activity (INA) including nine dry dispersion and eleven aqueous suspension techniques. The manuscript is well written and the topic fits well into the journal ACP. However, the authors should carry out some revisions before the paper is published in ACP. In general, I consider this paper very important, since cellulose is so common in the atmosphere and could be a very common ice nucleation particle (INP).

My main concern regarding this paper is that only three types of cellulose have been investigated. However, cellulose is the most common organic compound on Earth and it is the most common polysaccharide. Of course, there are many, many cellulose types and MCC, FC and NCC are only a very few representatives. It comes not clear from the manuscript how and why these three have been chosen. In general, I miss a more elaborated introduction (1.1 background) where the sources of cellulose in the biosphere and finally in the atmosphere are discussed. Also relevant literature should be discussed (regarding marine aerosols, bio-aerosols (fungi, pollen, bacteria, plant fragments, leaf litter etc.)), e.g. the fact that water extractable INPs consist of polysaccharides should be mentioned (Dreischmeier 2017,Pummer 2012). So there are many sources of cellulose but most cellulose is not ice nucleation active. Then it is important to understand what makes the difference in terms of INA. Why are some cellulose samples so much more ice nucleation active than others? The authors might at least try to find an answer on this question in order to enhance the scientific value of the manuscript.

In principle, the physical and chemical properties of cellulose depend a lot on the history of the respective sample: water uptake, swelling, drying, shrinking, are inherently important for the INA. Even a freeze-thawing cycle of the same cellulose-water system could change the INA from one experiment to the other. These are just some points which should be discussed in more detail and might also help to understand the results of the paper. From my point of view, cellulose is not the ideal candidate for an intercomparison program due to its unstable INA. On the other hand, this study gives good proof that it is not so much the influence of the different instruments which are responsible for the differing results, but much more the cellulose sample, since it properties are not sufficiently constant.

Another important point is the specific surface area of cellulose, since the calculation of the ice active site number inherently depends on it. However, the specific surface area of dry cellulose is not the same as the surface area in aqueous solution after swelling.

Much more area becomes available and also the surface chemistry exhibited to the water interface might be changed. The authors should explain how they include this into their parametrization.

Minor comment Fig. 3, y-axis: “relative intensity (a.u.)”

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