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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A Two-Component Parameterization of Marine Ice Nucleating Particles Based on Seawater Biology and Sea Spray Aerosol Measurements in the Mediterranean Sea" *by* Jonathan V. Trueblood et al.

Anonymous Referee #2

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Review of "A Two-Component Parameterization of Marine Ice Nucleating Particles Based on Seawater Biology and Sea Spray Aerosol Measurements in the Mediterranean Sea" by Trueblood et al.

General comment

This study investigated the ice-nucleating abilities of surface microlayer (SML), surface seawaters (SSW), and sea spray aerosol (SSA) particles collected/generated in the Mediterranean Sea. In parallel to the evaluation of the ice-nucleating abilities of

the different samples, a large set of biogeochemical analysis were performed on the samples to understand the relationship between ocean biology and marine ice nucleating particles (INP). While the ice nucleation analysis of the SML and SSW samples was performed with a LED based Ice Nuclei Detection Apparatus (LINDA), the analysis for the SSA samples was performed using a Dynamic Filter Processing Chamber (DFPC). Taking into account the collected information, the authors developed a new two-component parametrization. Although the information collected/derived by the authors is very rich and valuable, in addition that they were collected in a poorly explored region on Earth, the manuscript is not easy to follows, it lacks important information, and the conclusions are not clearly supported by the provided data. The manuscript fits with the ACP scope, but the current version cannot be accepted for its publication.

Major Comment 1. Two different techniques were used to analyze the ice nucleating abilities of the samples, i.e., the LINDA and the DFPC. While the LINDA determines the INP concentrations via the immersion freezing mode, the DFPC does it via condensation freezing. Given that both data sets were used to develop the parametrization, I am wondering if the INP concentration delivered by both instruments are directly comparable. For example, I am wondering about the very low concentration of INPs reported for the SSA samples in comparison to literature data. It is a true number or is it an artifact related to the used method? 2. I was unable to fully understand how the INP concentrations for the SSA samples were obtained as the DFPC was not properly described. Is this a new custom-made instrument? Is this the first data delivered/published by this instrument? If this is the case, a much deeper description needs to be provided. If this is not the case, how good is the agreement of the data delivered by the DFPC against other well-known ice nucleation instruments? 3. I am surprised that the INPs concentrations of the SML, SSW, and SSA are not compared to literature data. Actually, a recent study by Gong et al. (2020) who also studied the SML, SSW, and SSA is not cited/discussed here. There are also other studies in marine environment performed at subtropical and tropical latitudes that deserved to be discussed in the context of the present study. 4. Given that the chemical composition of the SSA is linked to the size

of the aerosol particles (e.g., O′Dowd et al. 2004; Prather et al. 2013), I am wondering how well the used apparatus to generate the SSA, reproduces the proper size distribution of the natural SSA. Also, the authors only provided the particle size distribution and the chemical characterization for particles smaller than 500 nm and 1000 nm, respectively. There is a big problem here because while the chemical analysis was performed for submicron particles only, but the INP concentration took into account total suspended particles. It has been shown that the aerosol particles with the highest potential to act as INP are those larger than 500 nm (DeMott et al. 2010), and especially the super-micron particles (Mason et al. 2015; Gong et al. 2020) ignored in the present study. 5. INPSSA was normalized by the particle surface area. However, the NPSSA were derived from samples with total suspended particles, but the particle surface area was derived from particles ranging between 10 and 500 nm only. There is a big big mismatch here that can hide important information or can even conduct the authors to deliver wrong conclusion. That is why the following was found: “no statistically significant correlations were seen between total submicron particle counts or total SSA surface area and INPSSA at all three temperatures”. Actually, it would have been more appropriate to use the size distribution of super-micron particles to calculate the particles surface area. 6. It is unclear if the SSW samples are really superficial waters (as defined by the authors), bulk waters, or deep waters. I could not find the depth at which those samples were collected. Also, the SSA was generated from waters collected at 5 m depth. Would not have been more relevant to use superficial waters instead? How comparable are the ice-nucleating abilities of the SSA particles (from “deep” waters, 5 m) with those from the superficial SML and SSW samples? 7. Section 4, the most important, is extremely short and too general without the required information to follow it. Two parametrizations were developed, one for temperatures above -22°C and one for temperatures below -22°C. Therefore, this means that the INPSSA were included in both parametrizations as the INP concentration for temperatures between -18°C and -25°C were obtained for the SSA samples. However, as mentioned above the chemical analysis for the SSA samples was performed for submicron particles only.

Therefore, this parametrizations may be valid for submicron SSA particles only and are not representative for marine aerosol particles.

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