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Comment on acp-2022-529

Anonymous Referee #2

Referee comment on "A global climatology of ice-nucleating particles under cirrus conditions derived from model simulations with MADE3 in EMAC" by Christof G. Beer et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-529-RC2, 2022

In this manuscript the distribution of ice nucleating particles (INPs) originating from different sources is investigated using a sophisticated aerosol module for a GCM (EMAC-MADE3). In addition to the usual candidates of INPs, glassy organic aerosols and crystalline ammonium sulfate are also taken into account. The authors find that ammonium sulfate surprisingly plays a major role in terms of providing the most INPs. The INPs are also coupled to a cirrus cloud module in the GCM for investigating the competition of heterogeneous ice nucleation and homogeneous freezing of solution droplets in the upper troposphere.

Generally, this is a very solid and well written manuscript, the implementation of the different species into the model is explained in a comprehensive way and the results are well described. Therefore, this is a suitable contribution to ACP, and I recommend to accept the manuscript subject to minor revisions (see below).

(1) The two major outcomes of the study are not really prominently stated. It was claimed for a long time that glassy organic aerosols are an important class of INPs and they must have an impact. It is quite clear from this study that the impact is rather weak, if not negligible. The dominant role of ammonium sulfate is also new. I would suggest to emphasize these interesting results more prominently.

(2) For aerosols, the large scale transport is the most important pathway, thus the distribution of aerosols can be simulated well with GCMs, even in this coarse resolution of T63. However, for treating (ice) clouds in GCMs, the variability of thermodynamic variables plays an important role. The ice cloud model relies on former work for the EMAC model, which only marginally treats subgrid scale variability, e.g., using TKE or gravity wave drags for determining the variability of vertical velocity; however, the horizontal/vertical variability is only taken into account by crude cloud cover schemes. From observations (satellite, surface observations, and many others) we know quite well that also ice clouds have internal structures, leading to heterogeneous cloud layers, which

provide additional spatial and temporal time scales. The use of microphysics schemes for the whole grid box, driven by a large-scale motion, without these scales in between might lead to an overestimation of the impact of nucleation pathways. A similar issue might occur for the removing of INPs (as included in ice crystals) by sedimentation of ice crystals, since sedimentation of ice particles in quite coarse vertical resolutions is highly tuned. It is quite clear that the authors cannot change the coupled ice cloud scheme. However, I would suggest to add some comments on these issues, since the competition of heterogeneous and homogeneous nucleation might be affected by small/meso scale motions.

(3) How sensitive are these results if the nucleation thresholds for the different INPs are changed (within their uncertainties)? Have you checked this in sensitivity analyses?