The paper 'Gaps in our understanding of ice-nucleating particle sources exposed by global simulation of the UK climate model', by Herbert et al attempts to identify the gaps in the understanding of the ice-nucleating particles (INPs) by simulating the global distribution of dust and marine-sourced INPs over an annual cycle. The authors have put an appreciable effort into this objective. Different parameterization schemes by previous investigators are employed for this purpose and modifications are made to reduce the bias and errors. Since the science behind the ice nucleation in the atmosphere is not fully understood and ice clouds are highly relevant for their radiative and hydrologic impacts, the study topic is very appropriate within the scope of the Atmospheric Chemistry and Physics journal. However, a few concerns remain.

General Comments

- Does the title "UK Climate model" rightly indicate the UKESM discussed in the manuscript?
- I could not find a dedicated "Results and Discussion" in the manuscript. Rather, discussions are included in the "Conclusions". Why so? Conclusions can be separated from discussions with precise conclusions alone.
- The current study presents a global INP model relevant to immersion-mode freezing of liquid water droplets (L70). If it only considers only one nucleation process out of many complicated proposals for atmospheric ice nucleation then how the study can claim that it can unveil the gap areas in the understanding of ice-nucleating particle sources? How much the immersion-mode freezing contribute to the total observed global ice nuclei concentration? How much it varies with region? This calls for highlighting the issue in the manuscript or modifying the manuscript tile to avoid confusion to the readers. Also, how much bias/underestimation in the presented results is due to this approach?
- Another concern is related to the anthropogenic contribution or effects in the INP concentration which are not considered anywhere nor discussed in section 5. Many regional studies have highlighted the role of anthropogenic dust and even the role of soot and BC as INPs. However, those discussions are missing in the current study except for a few references in section 4.2.2. The authors shall mention the studies from the Indian sub-continent which indicate the influence of pollution/anthropogenic in ice nuclei concentrations. Only a few or NO measurements from the most populated regions of the globe especially in the tropics are used in the current study (Fig 6). In this regard, how well the current study can justify the claim as a global evaluation? Table A1 does not fit for a global evaluation.
- Connected to the above concern, the statement made in the conclusions (L671) is misleading. INP concentration is maximum along the tropics especially where the human population is also high (Fig 4c) and the associated influence in hydrological cycle is also relevant. Considering this, how the statement in L671 can be generalized? This is a significant scientific aspect especially in the context of the dehydration of the tropical tropopause layer due to ice nucleation (Ref: works of Eric J. Jensen from NASA). Thus, the INP studies are as relevant in the tropics as much in mid-latitudes or polar circles.

Other Comments

- Please specify the references for equations 1 and 2
- Please re-write the sentence in L187 (..dust concentrations so poor dust optical depth..). Could not understand
- Please give the references for the statement in L434-436.
- Please re-write the sentence in L653-655. Could not understand