

Atmos. Chem. Phys. Discuss., referee comment RC2  
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## Comment on acp-2021-621

Anonymous Referee #2

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Referee comment on "Relative importance of high-latitude local and long-range-transported dust for Arctic ice-nucleating particles and impacts on Arctic mixed-phase clouds" by Yang Shi et al., Atmos. Chem. Phys. Discuss.,  
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Shi et al. present a global modeling study to estimate the contribution of dust emitted at high latitudes as source of ice nucleating particles in the Arctic. They added a source tagging technique for dust from different regions to accomplish this. The role of dust in the climate system is an important topic, especially in high latitudes, which are experiencing a rapid change in climate. The paper makes an important contribution to this topic, since high-latitude dust contributions are largely unknown. At the same time the paper highlights several challenges -- simulating the global distribution of dust itself, but also estimating the concentration of ice nucleating particles based on the simulated dust distribution. The paper fits well within the scope of ACP, and I recommend the paper to be published after the following comments are addressed:

- Line 124: More detail is needed to describe the ice-nucleation parameterization that is used in the simulations (I believe this is the soccer-ball model?). How is dust represented using this parameterization? Are the same model parameters applied not matter from which region the dust comes from (i.e. different mineralogical composition is ignored)?
- Related to this, in section 3.3, different ice-nucleation parameterizations are used for the comparison with measurements. It seems that the default parameterization should be part of this comparison. I suggest adding this to this section.
- Line 134: What was the rationale for choosing the time period 2006 to 2011? (and not for example a more recent time period for example)
- Line 150: Similar to comment 1, I recommend explaining more detail (i.e. equations) about the dust emission parameterization as well as the source tagging procedure. INP parameterization, dust emission parameterization and source tagging are central to this paper, so even though they are described in other papers, it will be helpful for the reader to have the information easily available. This could go in an Appendix or even the SI.
- Line 155-157: Are the differences between the mass assignments to modes in Z03 and Kok (2011) significant?
- Explanation of quantities shown in the figures: Please add information in the caption

over what time period the model results were averaged. For example, in Fig 3, is this the average over the entire simulated period (2006-2011)? If so, what is the year-to-year variability? And is it the same time period for the observations? What does the grey band represent?

- Fig 4: The caption mentions that the model results were averaged w.r.t time (2007-2011) and space. I suggest showing some measure of variability, with respect to time and space, to be better able to judge the agreement with the observations.
- Figs 5, 6, 7, 9, 10, 11, 12, 13: Include the time averaging interval in the captions.
- Line 257: I would argue that also the MAM case is consistently underestimated by the model and that DJF is underestimated near the surface. However, this is difficult to judge because neither observations nor model results contain any measure of uncertainty. It may well be that the two actually agree within the level of uncertainty. Please include some discussion about this.
- Figure 6: It would be instructive to additionally represent this figure with percentage contributions rather with absolute values for the column burden. This would make it easier to convey the information how much each region contributes to the burden in a given location.
- Figure 8: Suggest adding quantity and unit (INP conc. /  $L^{-1}$ ) as axes labels. How are the temperatures chosen – are they determined by what was used in the observations? How exactly were the time intervals of observations and measurements matched up? Line 346 mentions “monthly averaged aerosol populations” while Table 3 only lists Spring/Summer/Autumn of various years.
- Section 3.3: there are various INP parameterizations out there for various kinds of dust. While I don’t expect the authors to compare all of them, I suggest discussing the reason for the choices made for this paper since the choice of the parameterization can have a big impact on the result. (However, I do recommend including the default parameterization, see comment 2 above).
- Code availability: The github link only points to the general E3SM repo. The authors should include the code that was actually used to run the simulations (including the tagging), for example using zenodo archiving:  
<https://guides.github.com/activities/citable-code/>

Typographical errors:

Line 126: Explain WBF

Line 133: Should read “are shown”

General comment: Axes labels: I recommend to not use the format 1E-6 etc. to represent numbers, but use  $10^{-6}$  etc. instead.

