

Atmos. Chem. Phys. Discuss., referee comment RC3
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Comment on acp-2022-487

Anonymous Referee #3

Referee comment on "Particle number concentrations and size distributions in the stratosphere: implications of nucleation mechanisms and particle microphysics" by Fangqun Yu et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-487-RC3>, 2022

Very good work! The paper compares the nucleation process, between model results with two different nucleation schemes and CLOUD/ATom measurements, under the stratospheric condition. It's nice to see the authors relate the study to the SAI simulation, which could help to improve the SAI modeling accuracy. The overall reasoning in the paper is solid and well-justified.

I have some minor comments and corrections:

Line 81-83: Providing only two publication examples (i.e., Weisenstein et al., 2022, Laakso et al., 2022) seems not enough to prove that "BHN_V2002 has been used in most SAI modeling studies". It would be more convincing if the authors can tell us how many models use BHN_V2002. For example, there are many models involved in GeoMIP (Kravitz et al., 2013), it would be helpful if the authors can tell the GeoMIP community how many GeoMIP simulations use BHN_V2002 for nucleation simulation.

Line 136: $4^{\circ} \times 5^{\circ}$ is too coarse. If possible, please repeat the simulations in $2^{\circ} \times 2.5^{\circ}$. If not,

the authors should discuss how much the grid resolution may influence the difference between the model results and observations, especially for the comparison between model results and ATom observations at one site in Figure 6.

Line 274: why the tropics are selected as "(0°S-30°S)" instead of "(30°S-30°N)"?

Line 296: Figure 3 needs to be optimized:

(1) Set shared x or y axis label among figures (a) to (f).

(2) Adjust the location/size of figures (g) and (h).

(3) There is a horizontal dashed line on the top of the figure (h), which should be deleted.

Line 296: for Figure 3 (g),

(1) is it a coincidence that three solid lines end up with a similar nucleation rate (about $0.02 \text{ std. cm}^{-3} \text{ s}^{-1}$) at approximately 17.5 km?

(2) why there is an elbow point (at around 20 km) in the red solid line? In another word, why does the nucleation rate from BHN_V2002 has a much larger changing rate with height above 20 km, compared to below 20 km?

Line 319: "BHNV_2002" should be "BHN_V2002".

Line 390-391: I think that the competition between nucleation and condensation mentioned by Laakso et al. (2022) might be a complement to the "nonlinear process" (Line 390-391) mentioned by the authors.

Line 409: I don't understand the sentence: "Finally, the observed PNSDs show a clear AccuM2 in all seasons except Fall but the model does not predict the existence of the mode at all."

Based on Figure 6, the model may underpredict the AccuM2, especially in summer. But we cannot say "the model does not predict the existence of the AccuM2 mode at all".

What's more, the authors say "the model-simulated AccuM2 standard deviations are larger in SH Winter and Spring but are smaller in SH Summer and Fall" in Line 505. If "the model does not predict the existence of the AccuM2 mode at all", there would be no "model-simulated AccuM2 standard deviations".

Line 437: The citation (Clement and Harrison, 1992) is missed in the References. Please check and make sure all the citations in the main text are correspondingly listed in the References.

Line 475: Suggest changing "SAI efficiency" to "SAI radiative efficacy". Radiative efficacy refers to the radiative forcing normalized by the aerosol injection rate, which is widely used in SAI studies (e.g., Dai et al., 2018).

In the discussion part, I think the authors can highlight the importance of model development for reducing model uncertainties of SAI simulations. Some other SAI-related model development work (e.g., Golja et al., 2021, Sun et al., 2022) is worth mentioning.

For the next step, I hope the authors could consider comparing the modeled aerosol radiative forcing based on the two different nucleation schemes, which could help the Solar Geoengineering community to have a clear feeling about how much can different nucleation schemes influence the SAI radiative efficacy.

The papers mentioned above:

Dai et al., 2018: <https://doi.org/10.1002/2017GL076472>

Golja et al., 2021: <https://doi.org/10.1029/2020JD033438>

Kravitz et al., 2013: <https://doi.org/10.1002/2013JD020569>

Laakso et al., 2022: <https://doi.org/10.5194/acp-22-93-2022>

Sun et al., 2022: <https://doi.org/10.1029/2021MS002816>