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Discussions

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Interactive comment on "Radiative forcing of anthropogenic aerosols on cirrus clouds using a hybrid ice nucleation scheme" *by* Jialei Zhu and Joyce E. Penner

Anonymous Referee #2

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In this work, Zhu and Penner implement a hybrid ice nucleation scheme in the CESM/IMPACT global climate model and perform simulations to quantify the impact of anthropogenic aerosol on cirrus clouds. The new scheme combines the best features of two existing cirrus parameterizations, in order to reduce their drawbacks and improve the resulting estimates of climate impacts.

The paper provides an important contribution to a research field that is still affected by large uncertainties and a relatively low level of scientific understanding. It also puts the results into the context of previous studies (although mostly citing works from the same group) and updates the estimates of the anthropogenic impacts on cirrus cloud

properties.

There are, however, some parts of the paper that needs improvement, also for the sake of scientific reproducibility, and in general the presentation quality should be better structured and more accurate.

The comments given below should be addressed before the paper can be recommended for publication in ACP.

GENERAL COMMENTS:

1. The model description needs to be extended, as the limited amount of information provided may question the scientific reproducibility.

2. Several types of INPs are considered by the HYBRID scheme, but their properties are only briefly mentioned in the text and it is hard to get an overview of what is assumed. Adding a table of all relevant INPs and their corresponding properties could be useful.

3. The results section is very hard to read. A lot of maps and panels are being mentioned, but not in the order they appear. Moreover some of the maps shown in the figures are not discussed. This part needs to be revised and restructured (like in different subsections). The number of figures could also be reduced, by moving the less relevant ones to a Supplement. The current presentation is overwhelming for the reader, who needs to browse through a large number of plots and maps (more than 80!), while only a very short text is given for each of them and no structured discussion is provided.

4. It is not clear whether the given changes in ice number concentration (Ni) and radiative forcing (RF) are statistically significant, as no statistical tests are applied or discussed. This is critically important for the difference map plots (Figs. 5-12 and 14-17), where some of the patterns depicted may be below the noise level.

5. All reported impacts on Ni from the various effects are given only as absolute values, while the relative changes would be useful to understand their relevance (especially for non experts).

SPECIFIC COMMENTS:

L23: is this value for Ni calculated over the whole column, or only at a specific altitude/temperature range?

L23: here and in the rest of the manuscript, it would be useful to see the relative numbers (see also general comment 5).

L27: this is quite low: did you check whether it is statistically significant?

L129: the turbulent kinetic energy as a proxy for subgrid scale vertical velocity was used in previous studies too, for example Lohmann et al. (J. Geophys. Res., 1999), Lohmann (J. Atmos. Sci, 2002), and Kärcher and Lohmann (J. Geophys. Res., 2002).

L130: you could also mention Kuebbeler et al. (2014), who considered the contribution of orographic waves to the vertical velocity.

L169: please provide references for CESM and for IMPACT.

L171: please specify the vertical resolution as well.

L173: *"fourteen species"*: which ones?

L178: what is the hygroscopicity of bSoot?

L179: *"with <1 monolayers of sulfate"*: does this mean no monolayers, hence purely hydrophobic soot? If yes, please rephrase.

L182–187: this sentence is unclear. It looks like there are 2 types of pre-activated soot. One freezes at 145% RHi, but it is not clear how the other type is treated in terms of ice nucleating ability.

L188–189: *"Dust with fewer than 3 monolayers of sulfate coating is used to form heterogeneous INP in the model"*. At which RHi?

L206: are you considering the role of pre-existing ice crystal which may decrease the available supersaturation? The KL parameterization should include this possibility.

L215–217: could you please be more specify on how this is technically realized in the model?

L226: in the CEDS dataset the historical series ends at 2014, so why not using a more recent year for PD instead of 2000? And what is meant by ≈2000? Is it the year 2000 or are the emissions varied transiently around 2000?

L227: you may want to add that this is the same dataset used for the CMIP6 simulations.

L228–229: CEDS already provides aviation emissions, so apparently you are replacing them with the AEDT dataset in this work. Is there a reason for this choice? Is AEDT more accurate? Please elaborate on this, since emission data might be an important source of uncertainties in this kind of studies.

L231–233: I would suggest rephrasing this sentence as: *"In a sensitivity experiment (PI_cSoot), the emission of cSoot..."*. Note that cSoot is not defined and it appears again later on in the manuscript.

L240–242: which SOA precursors are considered? From which sources (natural/anthropogenic)? How do they change between PI and PD?

L246–248: are you using prescribed SSTs? Please specify.

L254: I would state here that this is a box model, as you are writing later in this section.

L259–260: are you using this 2.2 min update frequency also when applying the scheme in the GCM? If yes, how do you realize that, given that the global model uses a 30-min time-step?

C4

L302: for the sake of readability, you could consider splitting Sect. 3.1 in two parts: on the parcel model comparison (i.e., until L302) and on the GCM results (afterwards), respectively.

L304: the Krämer dataset includes other interesting quantities, like ice water content and relative humidity. Why not comparing them as well?

L309: I would remove *"somewhat"* from this sentence: the simulated concentrations are about one order of magnitude higher than the observations around 205 K.

L310–324: related to major comment 3 above: the figures should be discussed in the order they appear in the paper. Also some of the figures are not discussed at all (e.g., Figure 3d and 4d). In general, I find this paragraph quite hard to read and too short for the amount of results that it should describe (two figures, with 5 lat-lon maps and 5 zonal plots). Please consider restructuring and expanding this part and the rest of the section.

L311: the Ni spot over the tropics of Eastern Hemisphere in Figure 3a is remarkably high: could you please elaborate more about its possible causes?

L323: how is the occurrence frequency calculated?

L325–339: same as for L310–324 and also related to major comment 3.

L338: related to major comment 4, is this statistically significant?

L341: *"the increase in the sulfur emissions from PI to PD leads to a significant increase in Ni"*. Do you mean *"statistically significant"* or just *"large"*? If the first, a statistical analysis needs to be provided. For example, how much is the 90 or 95% confidence level on this quantity?

L380 and following: it is difficult to follow the discussion about these feedbacks without knowing how the model is set up. As I understood from Sect. 2, winds are nudged, but temperature is not. What about SST? How do you initialize the model in PI and PD?

Please provide more details, either here or in Sect. 2.

L385–387: could you please provide some numbers? Shipping is a large contributor to sulfur emissions over the oceans, and emission regulations in this sector have been introduced later than for other land-based sources (e.g., power plants).

L400–402: evaluating the model for IWC would be important for this discussion. See comment above about Figure 2: the Krämer dataset provides RHi and IWC, in addition to Ni. How are sedimentation of ice crystal and snow formation treated in the model? Are there differences between KL and LP? And if yes, how are they handled? I guess this is explained in the respective references, but it should be briefly mentioned in Sect. 2.

L431: the acronym FSNT does not corresponds to what it describes (all-sky shortwave forcing).

L432: see previous comment.

L446–449: the acronyms FNT, FSNT, FLNT are not very useful, I would suggest replacing them with NET, SW, LW or something more intuitive.

L447–449: *"The radiative forcing in cirrus clouds is mostly dominated by FLNT because of the larger longwave radiative effects of cirrus cloud than their shortwave radiative effects."* This sounds like a circular argument: longwave radiative forcing dominates because longwave radiative effects are more important. I would suggest rephrasing this sentence.

L476: what does the uncertainty range refer to? 1-sigma model variability? Or confidence level? Please specify. Statistical tests should also be performed.

L532: see my comment at L341.

L563: see my comment at L309.

Figure 2: please state that the red solid line shows the median (does it?) and also add

a line for the median value of the observations.

TECHNICAL CORRECTIONS:

L19: *"observations"* → *"with observations"*.

L39: the acronym PI is already defined in the abstract.

L53: the acronym *"GCMs"* usually indicates *"general"* circulation models.

L106: *"in limited studies"* → *"in a limited number of studies"*.

L114: *"observations now indicate that"*, this sounds like it is a new finding, but the fact that only a subsection of aerosols can act as INPs is well established (you also refer to studies from 2009, 2011 and 2012 to support this statement). I would rephrase this as *"observations later indicated that"* or similar.

L163: please use a consistent number of decimal places for the RF results given throughout the paper. Even better would be to use $mW\,m^{-2}$ instead of $W\,m^{-2}$, given the small numbers involved (< 1 in absolute terms).

L163: there is a typo in the units (*"W"* is missing).

L179: *"with 1-3 monolayers"* → *"coated with 1-3 monolayers"*.

L235: *"(PI_ALL)"* → *"PI_ALL"* (remove the brackets).

L272–273: I would suggest using consistent units for the concentrations.

L278 and L279: *"with of the order"* → *"with concentrations of the order"*.

L408: *"in the Figure"* → *"in Figure"*.

L421: *"IWP changes from"* → *"IWP switches from"* (to avoid repetition).

L459: *"the global average FNT due to sulfur emissions is a small negative, -0.025±0.064 W m^{-2}"*. This result is given as *"-0.02±0.06 W m^{-2}"* in the abstract (L27). Please use a consistent number of decimal places for all numerical results in

the paper.

L565: *"were used in the global model"* → *"were used in the CESM global model"*.

L573: *"in the Table 2"* → *"in Table 2"*.

L577-580: please consider adding some punctuation in this long sentence.

L582-585: see previous comment.

L622: *"observation"* → *"observations"*.

L902: it looks like *"900"* does not belong here.