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Atmospheric Chemistry and Physics
Discussions



# *Interactive comment on* "Effects of atmospheric dynamics and aerosols on the thermodynamic phase of cold clouds" *by* Jiming Li et al.

**Anonymous Referee #2**

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article

## 1 General comments

This study presents statistical relationships between cloud-top cloud thermodynamic phase and aerosols (dust, polluted dust and smoke) as well as meteorological variables (vertical velocity at 500 hPa, lower tropospheric static stability (LTSS) and surface temperature) to infer the influence of atmospheric dynamics on cloud thermodynamic phase using a combination of global satellite observations and reanalysis data over a 4-year period (2007-2010). The authors first evaluate the cloud thermodynamic phase partitioning schemes in a handful of models against observations. They find that out of



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the models, the cloud thermodynamic phase partitioning schemes in CAM3 and CAM5 compare best to the observations used in their study. The authors then proceed to show that vertical motions can explain the seasonal cycle of supercooled water cloud fraction (SCF) in regions where aerosols cannot explain the seasonal cycle of SCF. They find that strong vertical motions appear to be correlated with regions of low SCF likely through an enhanced precipitation rate, and that higher LTSS appears to be correlated with regions of high SCF. This work presents interesting results that could be useful for near-future model development, however, substantial revisions pertaining to the content, quality and writing style of the manuscript should be undertaken. Specific comments are provided below.

## 2 Specific Comments

1. **Title:** The study could be separated into two parts, the first part evaluating the temperature ramp schemes used in climate models against observations and the second part examining statistical relationships between dynamical variables and SCF. The title only reflects the latter part. Please change the title to better reflect the content of the manuscript.

2. **Introduction:** The logical flow can be improved to enhance clarity. Cold cloud schemes in models are discussed in the first paragraph before the existence of supercooled liquid clouds in the second paragraph. Also, on lines 107-109: the Clausius-Clapeyron equation simply relates the saturation vapour pressure and the temperature. If the authors wish to cite theoretical support for the existence of liquid, they should refer to the free energy barrier of pure water droplets and classical nucleation theory.

3. **Datasets and Methods:**





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- Lines 177-185: Please include indicate that the ERA-Interim reanalysis dataset was used to obtain the aerosol and cloud-top temperatures.
- Line 182: why was a resolution of $2^\circ \times 6^\circ$ chosen? The longitude dimension is quite wide. Please clarify.
- Line 184: It's not clear to me why only daytime observations were used. Wouldn?t it be better to use nighttime observations, especially for the CALIOP observations since sunlight decreases with the signal to noise ratio?

4. **Results**: Section 3.1 is not a result. This section is more appropriate for Section 2 (Datasets and Methods). Also, much of the beginning paragraph in this section that describes the lack of dependency of cloud thermodynamic phase on ice nucleation in model schemes is redundant with what was already written in the introduction and does not need to be repeated.

- Fig. 1: It would be more helpful to distinguish no data regions from regions where SCF is not unity at temperatures below 0°C.
- Fig. 5: The authors use panels a and b in this figure to demonstrate that scheme 1 is more accurate than scheme 2 in terms of simulating SCF compared to the observed values, but could it not be interpreted from panels c and d, which use scheme 1, that scheme 1 can do even more poorly than scheme 2 depending on what the dynamical thresholds of $T_{ice}$ , $T_w$ and n are? The authors have mentioned that $T_{ice}$ is "unreasonably" low ($-40^\circ$C) in CAM3, which I assume implies that this could explain why CAM3 does poorly even with scheme 1, but scheme 2 also has the same $T_{ice}$ ($-40^\circ$C) and predicts smaller absolute differences than CAM3. Please clarify.
- Equations 3 and 4 (and lines 384-385): If each bin is 1°C and there are bins from 0°C to $-40^\circ$C then shouldn?t there be 40 bins (not 41)?

- Figures 3 and 4 do not contain any information about the vertical distribution of n. Please consider including this information in an additional plot, as it may be useful to provide this information to the readers.
- Why have the global distributions of the vertical velocity at 700 hPa, LTSS and surface temperature have not been plotted? It may help to plot these since Figures 12, 13 and 14 do not contain any information about the distribution of these variables. Also, have pattern correlation coefficients between the variables been calculated?
- Fig. 6: Please consider using a more intuitive colour bar, i.e. positive values in a red gradient, negative values in a blue gradient, and zero values in white (or grey as in Fig. 7).
- Figure 14b: Why does greater the case when LTSS is less than or equal to 14 K not result in a higher n value for the bins with higher relative aerosol occurrence frequency?

5. Probably my biggest concern about the manuscript is that the model cloud thermodynamic phase partitioning schemes in Table 1 may not be directly comparable to the cloud-top observations made by CloudSat and CALIPSO in this study. The CAM3 and CAM5 schemes, at least are not, since the temperatures do not refer to the cloud-top temperatures and these limitations should be discussed in the text. Having said that, the conclusions that the authors have drawn regarding the realism of the cloud thermodynamic phase partitioning schemes would only be true if these schemes are fully consistent with how a satellite would observe the clouds, i.e. from the perspective of a satellite simulator. The authors have not run any model simulations in this study, and may find that even though the general formula of the schemes in Table 1 agree well with observations, that the actual model-computed SCFs may not agree very well with the observations after all since they are not comparing apples to apples in a strict sense. Furthermore, the authors should note that the temperature ramp used in CAM5 given in Table





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1 is specifically for detrained convective condensate. Liquid and ice mass and number concentrations for stratiform clouds are computed from prognostic equations in CAM5, which has a very different cloud microphysics scheme from that in previous version (e.g. CAM3/CAM4). This may also be the case for the other models. Please discuss these points.

6. The goal of many climate models is to move away from temperature ramp schemes in general, such as those for cloud thermodynamic phase partitioning listed in Table 1. This is unlikely to be accomplished any time soon, though, and the work of the authors in this respect is useful for the modelling community. However, the authors should discuss the move toward prognostic schemes in climate models, which many have already adopted.

7. Lines 740-741: This sentence is a bit ambiguous. There is evidence suggesting that a cloud phase feedback occurs, causing more shortwave to be reflected back out to space relative to the state prior to global warming. This finding can be briefly discussed here. A few references relating to the cloud phase feedback and cloud thermodynamic phase repartitioning are listed below:

   - Mitchell, J.F.B., Senior, C. A., and Ingram, W. J. $CO_2$ and climate: a missing feedback? Nature, 341, 132-134, 1989.
   - McCoy, D. T., Hartmann, D. L., Grosvenor, D. P. Observed Southern Ocean Cloud Properties and Shortwave Reflection. Part II: Phase Changes and Low Cloud Feedback. Journal of Climate, 27, 8858-8868, 2014.
   - McCoy, D. T., Hartmann, D. T., Zelinka, M. D., Ceppi, P., Grosvenor, D. P., Mixed?phase cloud physics and Southern Ocean cloud feedback in climate models. Journal of Geophysical Research: Atmospheres, 120, 9539-9554, 2015.
   - Storelvmo, T., Tan, I., Korolev, A. V. Cloud phase changes induced by $CO_2$ warming — a powerful yet poorly constrained cloud-climate feedback. Curr.





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Clim. Change Rep., 1(4), 288–296, 2015.

- Tsushima et al. (already in the references).
- Finally, I very strongly recommend that the authors ask a native English speaker to proofread for grammatical errors.

## 3 Technical Corrections

- Abstract: Please indicate that the aerosols refer specifically to dust, polluted dust and smoke aerosols here.
- Please enlarge the fonts in all figures and include only high-resolution plots created using vector graphics.
- Lines 177: The word “current" is preferred over “following".
- Line 190: CPR was already defined on line 144.
- Fig. 3 caption: Technically, what’s plotted is the mean supercooled water cloud fraction vs. the parameter n (what’s on the abscissa), not the other way around. However, n is the dependent variable here since it is fitted based on what f is, so it would be better to have it on the ordinate.
- Line 474: Please clearly define the relative aerosol occurrence frequency.
- Line 486: The sentence is missing a punctuation mark at the end of it.
- Fig. 10: Please change the title of the ordinate to “Supercooled Water Cloud Fraction" in panel a (not to be confused with the total cloud fraction or any other type of cloud fraction). Also, please specify that these seasons refer to the northern hemisphere.





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- Line 499: I think what is meant is “consistency", not “inconsistency".
- Line 612: Insert “that" between “everywhere" and “the".





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