

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

Anonymous Referee #1

Received and published: 22 March 2016

In this paper, the authors mostly analyze the geographical and seasonal variation of the relation between temperature and supercooled-liquid cloud frequency based on CloudSat 2B-CLDCLASS-LIDAR product. To this end, they utilize different type of equation and sets of parameters and find which one better represents the observations. Then, they investigate the effect of aerosols, vertical velocity at 500 hPa, Lower Tropospheric stability and sea surface temperature on the transition temperature between liquid-dominated and ice-dominated clouds in mixed-phase clouds ($0\text{degC} < T < -40\text{degC}$). Although the last part of the study may contain some interesting results if further developed and better presented, the first part of the study - which represents half of the results - is not representative of the title of the study and does provide any new insights on the topic of supercooled-liquid clouds and/or mixed-phase clouds that has not already been published (Cesana and Chepfer, 2013 JGR; Cesana et al., 2015

C1

JGR; McCoy et al., 2015 JGR; Tan and Storelvmo, 2015 JAS; Yoshida et al., 2010 JGR). Indeed, the authors missed a lot of recent publications on that field, which has received more and more attention lately. However, the authors, by using an independent method based on CloudSat, confirm previous results obtained by using CALIPSO observations (mentioned previously). But in its actual format, it is not sufficient to be published in a scientific journal and may be more relevant for technical study/report or validation method paper by comparison to CALIPSO products. This is why I strongly encourage the authors to remove the comparison with “model relation” part and to focus their study on the observational part in case they want to resubmit their work.

Overall, a lot of statements are vague and/or confused and are not supported by any references. Later on, numbers come from nowhere and seem to be guessed more than calculated. I strongly encourage the authors to clearly state whether they computed numbers or just guessed. Several results and conclusions only rely on visual inspections and hypothesis (qualitative analysis) rather than actual quantitative evidences (such as correlation or regression etc), particularly in the last part. In conclusion, In addition, the authors should show more numerical results in terms of means/correlations to strengthen our confidence in the results rather than just showing map and doing qualitative analysis based on those maps.

Other major flaws stand out in the paper. For example, the authors can't compare modeled Temperature-Phase relationship directly with that observed for several reasons (e.g. Cesana and Chepfer 2013, Fig. 11; Cesana et al., 2015, Fig. 1). It does not take into account: i) The bias of the instrument: the lidar cannot pass through optically thick clouds making the relation valid only for certain clouds. The lidar is more sensitive to liquid droplets than ice crystals in mixed phase clouds, which affects the shape of the T-Phase relation. ii) The sampling (spatio-temporal) effects iii) The different cloud/cloud phase definitions: The observations of SCF are liquid/ice frequency of occurrence ratio whereas the modeled SCF are based on ice/liquid water content mass ratio. Besides, you only select the upper part of the cloud whereas the relation

C2

is used in the whole column in the models. For all the above reasons, the modeled T-Phase relation cannot be compared directly to observations like it is done in the paper. If the authors really want to compare with observations, they'll have to either use a simulator of the instruments on the model (e.g. Cesana and Chepfer 2013) or ensure the comparison is possible and consistent by choosing conditions that reduce the differences cited above (e.g. Cesana et al., 2015). The figure 7a is quite different from what published in Yoshida et al (2010, Fig. 6), Hu et al. (2010, Fig. 7) and Cesana et al (2015, Fig. 5) showing the importance of using comparable datasets to evaluate the models. They also show that the T-Phase relation varies depending on the latitude and thus regionally by extension. Besides, Cesana and Chepfer (2013, fig. 11) showed specifically the regional variation of the T-Phase relation – while existing – was small.

Moreover, the authors insist in the fact that most models only use temperature-dependent relation to determine their cloud phase. This is clearly not the case anymore. Cesana et al. (2015) have shown that 5 out of 16 models of their study used the temperature as unique criterion to determine the cloud phase. And the ones using the temperature only are currently working on new schemes. Finally, CAM5 model is only using a T-Phase relation for the convective detrainment and not everywhere as stated in the paper. Besides, the relation mentioned in the paper for CAM5 is not correct, the parameters are $T_w = -10\text{degC}$ and $T_{ice} = -40\text{degC}$.

Regarding the previous general comments, I don't recommend this paper for publication in ACP. However, I strongly encourage the authors to work on the later part (relation of the cloud phase transition with the aerosols) of the paper and to resubmit another more focused manuscript including more quantitative results.

Specific comments:

Line 54: -30degC

Line 86: "However... climate models." This sentence is too vague and the 2 parts are not really connected. The authors should reformulate and specify what kind of

C3

observations (satellite insitu? All?), what kind of processes? (macro,micro?). Besides, Klein et al., 2013 and Zhang et al., 2005 do not refer to climate change/future climate but to present/past climate simulations. The authors should remove these 2 references.

Line 90: "One of... in GCMs." I don't know where the authors can find a list of the primary challenges but these study are quite old and do not represent the current primary challenges. I strongly recommend changing this sentence. Yet, I believe the supercooled liquid clouds and mixed-phase clouds are crucial to reduce the climate feedbacks uncertainties, as shown in McCoy et al., 2015.

Line 95: The authors use the term currently and refer to 2 studies that use old models. Cesana et al., 2015 and McCoy et al., 2015 (the list is not exhaustive) are more recent papers that illustrate this statement.

Line 104: Can the authors reference studies here? (e.g. Forbes et al., 2014 MWR)

Line 107: References are missing for CC theory and the laboratory results.

Line 110: The authors should mention in situ studies that are the most "trustable" observations (e.g. Heymsfield and Miloshevich, 1993, JAS)

Line 126: This "exponent" has not been defined. Please define it or remove the sentence.

Line 130: This sentence is difficult to understand and most likely not grammatically correct. Please reformulate.

Line 153: They only defined a relation between the cloud top temperature and the supercooled liquid fraction based on a best fit of the observations, which is very different from a model "parameterization". Please, change the last part of the sentence as well as the next 2 sentences.

Line 215: I'm assuming the authors are talking about the high-latitude mixed-phase clouds with a supercooled-liquid layer on top and precipitating ice below. However, the

C4

other way around may also happen, with an ice-topped layer. So please clarify.

Line 285: The model used in Doutriaux-Boucher and Quaas (2004) is obsolete and Hu et al. (2010) is not a model-based study. Please, use more recent references.

Eq (1) is wrong; T at the denominator should be T_{ice} (also in Table 1)

Line 296: It is not between -40 and 0degC but between T_{ice} and T_w

Although CAM5 partially uses temperature “ramp” (in convective detrainment), it uses most of the time prognostic equations to calculate liquid and ice mixing ratios. This T-phase equation is therefore not representative of the cloud phase in CAM5. Moreover, the standard version uses $T_{ice} = -40\text{degC}$ and $T_w = -10\text{degC}$ rather than -35degC and -5degC used in the modified version of Song et al (2012, journal of climate). The authors should mention this somewhere in the manuscript.

In addition, the new ERA and LMDZ models use slightly different T-phase relations now. Finally, is there a reason to choose these specific relations out of the Choi paper?

Line 297-302: This sentence is too long and the statement is not really supported by any references.

Line 302-305: Same comment, no references to support these facts that could be the topic of a whole paper (e.g. Tan and Storelvmo, 2015; McCoy et al., 2015).

Line 320: Actually, strong subsidence may contribute to dissipate stratocumuli. The weak subsidence favors stratocumulus formation (Wood et al., 2012, MWR).

Eq 3 and 4 are the same. I guess you forgot to remove the /41 in eq 3.

Line 409: Reference?

Line 427: I strongly encourage the authors to be more rigorous when they mention numbers. For example, T_{ice} does not seem to be -35degC judging from the figure 7a.

Line 441: It is not CAM3 or CAM5 but the T-phase relation that shows over or under-

C5

estimation of SLF.

Line 457-459: The analysis does not demonstrate this at all. It just shows that the T-Phase relation based on the 2B-CLDCLASS-LIDAR product is different from those used in some models, which was very much expected. However, the inability of GCMs to reproduce observed features of the cloud phase is not new and has been already demonstrated in previous studies using actual GCMs output rather than just the temperature-cloud phase relation (Chen et al., 2012; Cesana and Chepfer, 2013; Komurcu et al., 2014 JGR; Cesana et al., 2015; Tan and Storelvmo, 2015; the list is not exhaustive).

Line 460-462: Reference?

Why did the authors choose -20degC . If there is a special reason, please explain, otherwise it would be worth to check the sensitivity of other temperature isotherms.

Line 467: Is it a guess based on visual inspection or did the authors actually calculate the numbers?

This part is unclear and confusing. I don't see how the Fig. 8 verifies the later statement that changes in the SCF are correlated to dust. The following sentence is also unclear.

Line 477: The authors can't conclude this just based on 2 maps at -20degC without even looking for a statistical correlation between SLF and aerosols. A better way would be to focus on a specific region and study the SLF depending on the aerosol load.

The last part is very confusing and could be squeezed easily. Also I don't understand the absolute value for the vertical velocity, which is very confusing because we expect different results from positive or negative vertical velocity. Besides, the authors should define what positive vertical velocity means somewhere because in GCM studies, positive generally mean subsidence.

Finally, in fig. 12, the difference between T_{50} at 0 and 0.0001 (% ???) of aerosol frequency seems to be an artifact rather than a real observation and does not mean

C6

than aerosol have more influence than vertical velocity.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-147, 2016.