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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A 10-year climatology of globally distributed ice cloud properties inferred from the CALIPSO observations" *by* Honglin Pan et al.

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Response to reviewers comments

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Title: "Comparison of CALIPSO level 3 cloud products and application of diurnal vertical variation of global ice cloud properties" by Honglin Pan et al. Journal: Atmospheric Chemistry and Physics (ACP)

Anonymous Reviewer # 2 Answer to major comments: First of all, we really appreciate your instructive and scientific suggestions to our paper! And we have revised them

thoroughly below. Answer to Specific comments Though the calculations are useful, I do not know a) How the calculations compare to previous published calculations, and b) What is "new and innovative" in the results presented in the paper. For these two reasons, the paper is problematic. The authors need to address these issues prior to acceptance of the paper. Answer: Thank you for your suggestion very much. In order to more concrete study objective of our paper, we changed the title to "Comparison of CALIPSO level 3 cloud product and application to diurnal vertical variation of global ice cloud properties". That is, the detailed study content is also corresponding to change that we first compare the ICF from CALIPSO with MODIS, and then we used the CALIPSO ice cloud product to study the vertical distribution of diurnal variability of ice cloud properties. Accordingly, the present version of manuscript has changed a lot than the previous version. In the end, we have discussed the previous study result to our results in the last section. At present, the new manuscript is more paid attention to the vertical variation of ice cloud characteristics. However, some caveats exist when using CALIPSO level 3 version 1.0 profile product in the study. For example, polar orbiting satellite data with a repeat cycle of 16 days and a local equator-crossing times (EXTs) of 1:30 a.m./p.m. which is limited to conduct analysis of diurnal variability of ice cloud properties, and sun light noise caused the bad data quality in the daytime. Therefore, in this paper, we cannot analyze the real physical process of diurnal variation of ice clouds due to some artificial variation, but we can describe the observational phenomenon of ice cloud, which can pave the way for further study of ice clouds, such as cloud model. Further, as current climate models can not accurately depict the vertical distribution of ice clouds, especially on the diurnal difference on the vertical resolving scale, CALIOP do provide a valuable opportunity to study the diurnal variations of global ice cloud properties. In summary, we think our results are favorable to improve the modeling of ice clouds as the observational facts. In the Introduction (line 92, page 4) the authors state that previous studies (6 studies, see lines 89-90) "are not sufficient". Why are those papers "not sufficient"? What does the current paper achieve that was not achieved by previous papers? Please

answer these questions without stating an unsubstantiated negative value judgement. Answer: Because many of these studies have been limited to small spatial regions and short temporal periods, as well as certain classifications of ice clouds. Therefore, long-term and large-scale studies of ice clouds resolved in the vertical are necessary, to delineate some specific physical processes, which can be regarded as the cause of the biggest errors in cloud modeling. In the Summary and Conclusions section, there are no references to the previous literature. What are the commonalities (and differences) between the current calculations and the previous literature? Add a paragraph or two, with references, to address this concern in the Summary and Conclusions section. Answer: Thank you for your suggestion. We have added a paragraph to compare the previous study with our results in the Summary and Conclusions section. The detailed contents are as follows: Our results can be compared with early studies. Huang et al. (2015) used the CALIPSO and CloudSat cloud data to investigate the climatology of cloud water content with different cloud type. Figure 2 of their study reveal that each type of cloud shows an asymmetric distribution between NH and SH. And over both middle and high latitudes, clouds present lager seasonal variation in the NH than SH. The vertical distributions of ICF and IWC in Fig.4 and 5 show the similar characteristics. However, we also find that the PSCs occur in the SH. Figure A3 of Sassen et al. (2008) illustrated that maximum difference of cirrus occurrence over the tropics during diurnal cycle. This point of their results is confirmed by our analysis of Fig.7. Hong and Liu. (2015) suggested that thin ice clouds frequently occur in the tropics at high altitudes, and infrequent occurrence of sub-visual ice clouds. Our study of Fig.8 indicates that sub-visual ice clouds are more frequent at high altitudes (above 15km) than low altitudes, and thin ice clouds frequent occurrence in the low and middle latitudes at high altitudes. In addition, Fig.5 of Martins et al. (2011) revealed the sub-visual ice cloud showed the maximum occurrence frequency at $\sim$15 km over the tropics, but at $\sim$10km over mid latitudes. Our results of Fig.8 demonstrated that sub-visual ice clouds with the maximum concurrency at $\sim$15km over the mid-latitudes by CALIPSO data. Moreover, their results of Fig.3 showed the sub-visual ice clouds

with about 30%-40% of ICF in the strong convective areas, which are consisted with our results of Fig.2. Answer to technical comments Abstract, line 42: Change to “The latitude-and-altitude mean distributions of ICF and IWC were found to be unimodal in all seasons”. I am not sure what unimodal refers to, either on line 42 or later in the text at lines 206 and 242. Please clarify. See comments below on lines 206-209. Answer: Thank you for your suggestion. We have re-written them. The use of the phrase “On the other hand” is confusing, since it is commonly used to make a contrast, and the sentence if it is used in (line 42) does not make a contrast to the previous sentence. Answer: Thank you for your suggestion. We have dropped them. Though the English in the text is generally good, there are several lines in the text which should be revised: Line 47, page 2: change “strong convective activities” to “strong convective activity” This comment also applies to lines 192 and 213. Answer: Thank you for your suggestion. We have corrected them. Line 111, page 5: change to the “A-train” constellation of satellites Answer: Thank you for your suggestion. We have re-written them. Line 131-136, page 6: change to The Level 1 CALIOP data file contains 532 nm parallel polarized and 532 nm perpendicular-polarized attenuated backscatter coefficients. The attenuated backscatter coefficient at 1064 nm is also used to produce the level 2 data file products, given the CALIOP measurements and several algorithms. Answer: Thank you for your suggestion. But we have dropped them, because we revised content of whole paper to better make the study objective more concrete. In equation 2, line 165, on page 8: Why is the summation from 43 to 19 (with 43 below the $\sum symbol, instead of 19 to 43 with the 19 under the \sum symbol?) Answer$: $Thank you for your suggestion. We made a mistake about it, and we have re-written the equation from bin 19 to bin 43. Line 167, page 8$: $What are the numerical ranges of IWC BB and IWC H? Answer$: $The histogram bin range of IWC can be seen in Table 1 below. We only use the bin 2-16 and bin 19-43 to calculate IWC, and the bin 1 and 44, as well as 17 and 18 are neglected.$

Line 189, page 9: change to storm activity Answer: Thank you for your suggestion. We have re-written them. Lines 206-209, page 10: change to coverage of ICF generally ex-

hibited a vertical profile with a single peak, with the peak under the latitude-independent tropical tropopause altitude, followed by a peak decreasing in altitude steadily towards both the SH and NH polar regions. Answer: Thank you for your suggestion. We have re-written them. When I first looked at Fig. 3, I asked myself the question: “Why are the nighttime ICF larger than the daytime values?” Though there is discussion later in the text (lines 248 and 373), it would be good to tell the reader that a discussion of this matter is discussed later in the text. Are the nighttime and daytime differences a measurement artifact or a cloud microphysics issue? Are nighttime temperatures less than daytime temperatures? Some reference to the previous literature would be helpful. Answer: Thank you for your suggestion. We have added them in the manuscript. Line 228, Page 11: change to asymmetrical distributions. Answer: Thank you for your suggestion. We have changed. For Figure 1, page 31, the color scale goes up to 0.5, while the data is mainly from 0 to 0.3. The authors should consider redoing the graph with a color scale from 0 to 0.3 Answer: Thank you for your suggestion. Figure 1 comes from the 3D gridded level 3 data. We make an integral of altitude samples at each latitude and longitude bin for the CALIOP L3 Ice Cloud product, transforming ICF from 3D into 2D. And we deleted the Fig.1 and 2 because we changed the study content and title, now paid mainly attention to vertical distribution of diurnal variation of ice cloud properties. For Figure 2, page 31, the color scale goes up to 0.01, while the data is mainly from 0 to 0005. The authors should consider redoing the graph with a color scale from 0 to 0.005 Answer: Thank you for your suggestion. Figure 2 comes from the 3D gridded level 3 data. We make an integral of altitude samples at each latitude and longitude bin for the CALIOP L3 Ice Cloud product, transforming ICF from 3D into 2D. And we deleted the Fig.1 and 2 because we changed the study content and title, now paid mainly attention to vertical distribution of diurnal variation of ice cloud properties. Line 232, page 11: Use the same g/m3 units in the text as in Figure 5. Answer: Thank you for your suggestion. We have corrected it. Line 235, page 11: I did not understand what the “spike-shaped structure” refers to. From previous CALIPSO papers, this structure is likely identified with a physical feature. Refer to the literature to

make the structure less mysterious. (Is it related to the melting-band lidar backscatter feature, or something else?) Answer: Thank you for your suggestion. We used the quality flags to re-plot the results and adjust the color range. We find the results still occur the "spike-shaped structures" but the spikes became weaker. So, we also think the spikes possibly are the artifacts based on the retrieved algorithm of IWC. And we have emailed the contact the creator of this product, but without the response. Now, we only point out the spikes are artifact in the paper. Line 238-240, page 11: The sentence is not clear. Please revise. The term “we excluded the maximum” is not clear. Answer: We excluded the maximum of the IWC (in Fig.5 that we re-plot, the small red rectangle contains one IWC data over the SH polar region in summer, the big red rectangle zooms in on the small red rectangle) because the value lager than 0.01g/m3. Line 328, page 15: change to during night compared to day. Answer: Thank you for your suggestion. We have changed it. In Figure 8 (page 37), it may be better to graph 100 (# night observations – # day observations) / # night observations instead of # night observations - # day observations. Answer: Thank you for your suggestion. We have changed it. Line 387, page 18: The phrase “the values of these parameters were obtained from the CALIPSO platform” implies that the RH and temperature profiles are measured by the CALIPSO experiment. It would be better to state that auxiliary files specify these profiles. Please specify the origin of the auxiliary files. Answer: Thank you for your suggestion. According to the opinions of other experts, we have changed the study content and title, now paid mainly attention to vertical distribution of diurnal variation of ice cloud properties. So, we dropped the section. Line 388-389, page 18: change to Fig. 9 shows the 10-year global contour density plots of nighttime and daytime IWC, RH, and TE. Answer: Thank you for your suggestion. According to the opinions of other experts, we have changed the study content and title, now paid mainly attention to vertical distribution of diurnal variation of ice cloud properties. So, we dropped the Fig.9. Line 443-446, page 20: Rephrase. See comment on lines 206-209. Answer: Thank you for your suggestion. We have re-written it. Table 3. The numbers are too small. Expand the table into two parts to increase the font size. Answer: Thank you

for your suggestion. According to the opinions of other experts, we have changed the study content and title, so now paid mainly attention to vertical distribution of diurnal variation of ice cloud properties. So, we dropped the Table 3.

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