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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)

Dear Editor, Thank you very much for reviewing our research paper and providing the list of comments/suggestions raised by the learned reviewers which in turn helped us

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Discussion paper

in improving the quality of an earlier version of the paper. As per the suggestions of reviewers, we have gone through the entire paper giving suitable replies to their queries and revised the whole paper to correct presentation errors, as well as add some figures to the supplementary material. We deleted some of the contents of manuscript which are repetitions following the suggestions given by the reviewers.

The authors wish to thank the Handling Editor for his encouragement and support in contacting the reviewers to complete the peer-review process in time at the earliest. The authors are also grateful to the two anonymous reviewers for their constructive and useful comments which in turn improved the scientific content of an earlier version of the manuscript.

The modified text in the revised manuscript is highlighted with yellow color to distinguish with the normal text. However, the tracked version of submitted manuscript provided the changes/corrections implemented following the reviewer(s) suggestions is attached during the revision submission process.

Anonymous Reviewer # 1 Reply to major comments: First of all, we really appreciate your instructive and scientific suggestions on our paper. And we have revised them thoroughly following your suggestions and replies are given below. For the first point, we agree the idea that average ice cloud detections over entire vertical profiles (between -0.5 km and 20 km ASL) to build maps of ice cloud fraction is not reasonable. Therefore, we group the ICF into three types, that is, low, middle, and high to recalculate the result of ICF over the globe based on the cloud top pressure, according to Chepfer et al., 2010. doi: 10.1029/2009JD012251 that you listed, Thanks again! For the second suggestion, we are not agreed the suggestion. Namely, to eliminate PSCs from the dataset before creating any statistics. The reasons are as follows: The CALIPSO level 3 ice product is aggerated from newest version level 2 cloud product, and ice cloud samples are selected by a series of algorithms and quality assurance. Namely, the cloud aerosol discrimination (CAD algorithm) is used to distinguish the cloud and aerosol by the score of -100 to +100, and when the score bigger than 20,

the feature is identified to cloud, otherwise to aerosol. Moreover, the aerosol also can be further grouped into tropospheric aerosol and stratospheric aerosol, the latter includes PSC aerosol, volcanic ash, and sulfate/other. Then, the cloud phase algorithm can classify the cloud into water and ice phase. Consequently, ICF retrieved from ice cloud product should be eliminating the PSC aerosol to the large extent, and the PSC is included into the ice cloud, which would be reasonable. In addition, CALIPSO also released level 3 stratospheric aerosol product in 2018, which can further believe the ice cloud product does not contain the PSC aerosol to some extent. And the detailed quality assurance of ice cloud product has been added into the manuscript. For the third point, we accept the suggestion you provided. I drop the section of relationship between ice cloud properties and meteorological variable of relative humidity and temperature. For the fourth point, 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 accu-

rately 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. The replies we have provided are best up to our knowledge and hope the replies are given satisfactorily. Replies to the minor comments: Abstract: the abstract talks about "summer" several times, but it is unclear at this point that this means the NH summer. Please clarify the writing here (may be by talking about months instead of seasons). Answer: Thank you for your suggestion. We have corrected it. l.44: "the equatorial region of the NH": the equator is between the hemispheres, so this has no meaning. On the next line, "NH equator" has the same problem. Same thing on l.210 ("the equator of the NH"). Please ïňĄnd a correct way to say what you want to say (maybe reference latitudes). Answer: Thank you for your suggestion. We have corrected it. l.54-57: the last two sentences of the abstract merely describe what was done in the paper, they do not convey what the work found out. Please remove them (see second main comment). Answer: Thank you for your suggestion. We have removed it. l.131-146: It is unclear why all the information provided here is relevant to the study. Please either connect these explanations to the results that are presented (for instance by arguing sampling limitations are connected somehow to the behavior of the backscatter signal) or remove. The discussion of channels (l.132-135) is particularly confused: both 532 and 1064 backscatter coefïňĄcients are used to derive level 2 products. The 1064nm sentence has no verb. Answer:Thank you for your suggestion. We have removed it. l.154: please explain where does the IWC provided by the level 3 data come from? How what is retrieved and what are the uncertainties attached? Answer:The level 3 data of IWC come from level 2 cloud product retrieved from CALIPSO. The retrieved formula is from where σ is 532nm the volume extinction coefficient of km-1, and C0 = 119 g.cm-1 and C1 = 1.22 g.cm-1 are parameters obtained from an observed empirical relationship between lidar extinction and in situ measurements of cloud properties (Zhao et al., 2018; Pan et al., 2017). Consequently, the uncertainties due to the empir-

ical relationship of the formula.We have added and explained this content in this paper. l.159: how is a "ice cloud-accepted sampled" deïňĄned? Please explain. Answer:Ice cloud-accepted samples refer to the number of ice clouds samples passed the quality filters including quality assurance and quality control, respectively. And the detailed content can be found in the section 2.3 of the paper. l.168: I understand excluding outside bins 1 and 44, but why exclude bins 17 and 18, which are near the center of the distribution? Please explain. Answer: Bin 1 and 44 are very small or big, and small magnitude values with less confidence are stored in bin 17 and 18 from Table 1 below. So, we exclude them to make the result more reasonable.

l.176: "altitude of 60m": do you mean a vertical resolution of 60m? Or it is 60m ASL? Answer: Yes, it is 60m ASL. l.184: Where do the maps shown in Fig. 1 and 2 come from? Did you create the data yourself? How did you do it? Did you derive them from the 3D gridded level 3 data? In ïňĄg. 2, only half of the colormap appears to be used, please use all the color range (e.g. set the max IWC at 0.005 g/m3). Answer: Yes. Figure 1 and 2 come 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. l.194: see main comment about PSCs. Answer: We reckon that no need to eliminate PSCs from the dataset before creating any statistics. The reasons are as follows: The CALIPSO level 3 ice products is aggerated from newest version level 2 cloud products, and ice cloud samples are selected by a series of algorithms and quality assurance. Namely, the cloud aerosol discrimination (CAD algorithm) is used to distinguish the cloud and aerosol by the score of -100 to +100, and when the score bigger than 20, the feature is identified to cloud, otherwise to aerosol. Moreover, the aerosol also can be further grouped into tropospheric aerosol and stratospheric aerosol, the latter includes PSC aerosol, volcanic ash, and sulfate/other. Then, the cloud phase algorithm can classify the cloud into water and ice phase. Consequently, ICF retrieved from ice cloud product should be eliminating

the PSC aerosol to the large extent, and the PSC is included into the ice cloud, which would be reasonable. In addition, CALIPSO also released level 3 stratospheric aerosol product in 2018, which can further believe the ice cloud product does not contain the PSC aerosol to some extent. And the detailed quality assurance of ice cloud product has been added into the manuscript. l.207: I do not understand what the authors mean when they say the ICF peaks under the "ïňĆat" tropopause altitude. ICF maximas are not at the tropopause altitude, they are well below. Do they mean that the highest ICF are found in the tropics? Why refer to the tropopause at all then? It is a known feature of the tropopause that it is constant within the tropical belt. Same comment for l.444. Answer: Yes, the highest ICF are found in the tropics. We want to describe that the coverage of ICF generally exhibited a flattened cosine curve distribution, caused the strong mixing and heat transport by the Hadley circulations, where the peak is below the tropical tropopause. In addition, to clearly state the phenomenon, we have added the tropopause displayed in black solid line on the ICF of zonal altitude plot. l.207: please skip the deïňĄnition of the tropopause Answer: Thank you for your suggestion. We have skipped it. l.208: "... and decreases steadily towards the poles": the subject of "decreases" here is "the peak". Values of ICF do not decrease towards the poles, they even increase in some instances (e.g. Spring nighttime towards the South Pole). Maybe the authors meant that the altitude of ice clouds decreases. Please ïňĄx. Answer: Thank you for your suggestion. We have fixed it. l.217-221: the limited sampling of nighttime data during the summer season in the NH polarregionhasasimpleexplanation: duringJJAtheNHpolarregionsisinmostlypermanent daytime. So, there is only very limited nighttime data. During DJF the NH polar region is in permanent nighttime, so there is only limited daytime data. The opposite is true for the SH polar region: permanent daytime in DJF, permanent nighttime in JJA. Some data is there, but not much. This explains the season all limited sampling of nighttime and daytime data in Polar Regions. This is a fact related to the orbit of the Earth around the sun, which affects all observations, and not a CALIPSO limitation. Please clarify your discussion of this effect. The claim that Figure 3 "reveals" this well-known effect is a little exaggerated.

Answer: Thank you for your suggestion. We have added the reason you listed into our paper. l.222:"extrapolation can be used for more complete and accurate data": Extrapolation basically ïňĄlls out gaps in the data using existing information, but does not add information. Extrapolated data would not be more accurate. Answer: Thank you for your suggestion. We have removed it. l.230: here you attribute opposite (I think this is what you mean by "contradictory") variations of IWC and ICF to sampling biases. What do you mean by that? Sampling biases affect IWC and ICF detections in the same way: IWC cannot be retrieved where no cloud is detected. Please clarify. Answer: Thank you for your suggestion.We calculated the IWC by the sample sum of the bin in 2-16 and bin in 19-43. And we found in Fig.6 that more (less) samples of IWC in large bin of 19-43 (small bin of 2-16) in the daytime than the nighttime, which lead to the IWC in the daytime bigger than the nighttime. And we have re-write the section in the paper.

l.234: The "spike-shaped structures" in Fig. 4 are a major concern. If the IWC data contains quality ïňĆags, the authors should see if raising the quality requirements makes thespikesdisappear. Otherwise, I would encourage the authors to contact the creators of the level 3 dataset and ask them about these spikes. These spikes do not look geophysical, and if they are recognized product artefacts efforts should be made to remove them from an article proposed for publication. 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 spikesbecame 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. l.239: "we excluded the maximum..." The maximum was excluded from what? Please clarify. 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. l.263-265: this was already visible and clearer on Fig. 3. Please just reference the previous discussion. Answer: Thank you for your suggestion. We have corrected it. l.265-267: Why

is this interesting? Answer: Because the total number of samples for the daytime was smaller than nighttime in most of levels. However, the total number of samples for the daytime was more than nighttime which is below ∼5 km over the low and middle latitude for both hemispheres (because the x-axis is on the logarithmic scale and hence, the negative values were neglected). l.269: "negative values of the ICF": you mean a negative diurnal change of the ICF? Negative ICF should not exist. Answer: Yes, a negative diurnal change of the ICF. Fig. 6: In Figure 6, rows 3 and 4 show basically the same thing: the number of points in which data has been sampled. A requirement for IWC retrieval is the detection of an ice cloud, so I am guessing that values shown in rows 3 and 4 should be the same or at least extremely close. Evaluating the difference between rows 3 and 4 would inform about how frequently an ice cloud is detected from which IWC cannot be retrieved, it would say more about the domain of validity of the IWC retrieval algorithm and less about the relationship between cloud presence and IWC. In Figure 6 differences between rows 3 and 4 cannot be seen anyway. Again, the sampling variability tells us more about the instrument than it teaches us about clouds. It is ïňĄne to discuss the instrument sampling if it allows a discussion about clouds afterwards, but by itself it is of limited interest. The limitations which are described here were already discussed elsewhere (see for instance the 2009 series of CALIPSO papers that discuss sampling in JAOT, e.g. Powell et al. 2009 and Hunt et al. 2009). AnswerïijŽThank you for the reference you listed, and we are agreed to your idea about the point. However, in this paper, we want to emphasize the total number of IWC and ICF samples show the similar distribution during nighttime and daytime, which can be further verify the negative diurnal variation of IWC owing to more lager values in the day are sampled (discussed in Fig.6). Additionally, we re-write this section in the manuscript. l.288: "we revealed some interesting facts..." All the facts explained below are already known. It would be more accurate to say that your results conïňĄrm known facts about how CALIPSO samples clouds. Answer: Thank you for your suggestion. We have corrected it. l.291-293: this has already been discussed in sect. 3.3. Please sum up. Answer: Thank you for your suggestion. We have summed them up. l.294:

"this explains the behavior..." Your statements do not explain the opposite trends, they are consistent with the opposite trends that were already discussed. Explaining the trends would mean 1) proposing a mechanism that could lead to opposite trends and 2) support the validity of that mechanism through literature or additional data. This has not been done here. Answer: Thank you for your suggestion. We have explained it in the paper. We calculated the IWC by the sample sum of the bin in 2-16 and bin in 19-43 (see the Table 1). And we found in Fig.6 that more (less) samples of IWC in large bin of 19-43 (small bin of 2-16) in the daytime than the nighttime, which lead to the IWC in the daytime bigger than the nighttime. And we found the total number of IWC and ICF samples show the similar distribution during nighttime and daytime, which can be further verify the negative diurnal variation of IWC owing to more lager values in the day are sampled, which present the opposite trends of ICF.

l.302-304: please compare and contrast your results with sub visual cirrus values from Martins et al. 2010 doi: 10.1029/2010JD014519 the total absence of SVC over convection centers is particularly surprising and should be discussed. Answer: Thank you for your suggestion. We have compared and discussed it in our paper. And the results of Fig.3 and 5 from Martins about the sub-visual ice cloud are basically similar to our results of Fig.8. In addition, the paper is published in 2011 rather than 2010. l.311: thevaluesdocumentedheremightbecorrect,butwhyaretheyuseful/important? Please explain. Answer: We have dropped this part.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, 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. At present, the new manuscript is more paid attention on the vertical variation of ice cloud characteristics. l.326-328: this has already been described previously. Please sum up. Answer: Thank you for your suggestion. We have summed them up. l.329-332: this

has already been described for Fig. 6. Please avoid repetition. Answer: Thank you for your suggestion. We have deleted them. l.335-368: all this is basically a verbal description of Fig. 8: this is smaller here; this is larger there. If these descriptions are not tied to an interpretation, that tries to make sense of the variations and explain how they are due to physical processes, they are basically useless. I might as well just look at the ïňĄgure. These descriptions are a required step but are not sufïňĄcient. Please sum them up and point out to the reader which features are important and conïňĄrm or teach us things about ice clouds and IWC. Answer: Thank you for your suggestion. We have summed them up. l.369-378: this part attempts to provide some explanations for the ice clouds and IWC features described by Fig. 8, but only considers possible instrument/sampling biases. As said before, discussions of instrument biases are interesting, but only if they allow you to ignore the biases and reveal accurate facts about geophysical quantities. The biases discussed here are already known. Answer: The diurnal variation mainly contains artificial variation (e.g. instrument-induced, classification-induced, and sampling-induced variabilities) and real variation of physical process of ice clouds that you said, but the observations of ice clouds based on CALIPSO retrieves are not reasonable if we ignore the instrument biases. So, we want to express analyzing the diurnal variation of ice clouds should considered these factors by this part. Moreover, 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. So, we think our findings of the variation of ice properties in vertically resolved scale by CALIPSO level 3 global monthly ice cloud data, are favorable to improve the modeling of ice clouds. Sect. 3.7: as stated in the main comments, I do not see the point of this section and the ïňĄgures that go with it. Color scales of Fig. 9 mostly hide any possible correlations between the variables shown, but it appears the vast majority of RH and Temperatures are centered about a single main value, with little variability. The correlation coefïňĄcients suggest no correlation. Why the comparison should be done is not explained. Answer: We have deleted this

section based on your suggestion based on your major comment. Sect.4: See last main comment. In its present state this section merely restates everything that has already been said before. It makes no attempt to explain why any of the results is important or useful or new. No context is provided, no literature cited. Please ïňĄx this. Answer: Thank you for your suggestion. We have fixed them in the last section that cited the literature from previous studies. l.472-476: Here the authors state that their analysis suggests that the distributions of icecloudandIWCprovidedbytheCALIP-SOlevel3dataare"reasonableandreliable".There are several problems with this : 1) this goal was not presented as such in the abstract (i.e. the abstract does not say "in this paper we aim to show that the level 3 data are reasonable", it says "we aim to analyze the climatology of ice clouds and IWC"). 2) this goal has not been achieved: since you do not compare your level 3 statistics with literature or third-party data, there is no evidence in the article that suggests the values are reasonable and/or reliable. 3) the unexplained IWC spikes rather suggest that the level 3 IWC are in places neither reasonable or reliable. Product validation is an endeavor as important as studying cloud climatology, but in this present state the paper has achieved neither of these goals. Answer: Thank you for your suggestion. We have compared level 3 ice cloud product from CALIPSO with MODIS level 3 cloud products, and we also compare our results with the previous studies. Answer to technical corrections Please avoid "the CALIOP". Use "CALIOP" instead Answer: Thank you for your suggestion. We have corrected it. l.42: "On the other hand": please remove Answer: Thank you for your suggestion. We have removed it. l. 53: "(0.3<ICOD<1, ICOD>1)" this can be written as "(ICOD>0.3)". Answer: Thank you for your suggestion. We have re-written it. l. 89-90: please put the citations in chronological order Answer: Thank you for your suggestion. We have corrected it. l.110: "the active instruments like the CALIOP..." This sentence is not correct, please rewrite. Answer: Thank you for your suggestion. We have re-written it. l.147: "the CALIPSO lidar instrument released": the lidar did not release a dataset. NASA did. Answer: Thank you for your suggestion. We have corrected it. l.149: up to that point sentences were written in the present tense, now the writing switches to the past tense.

Please ïňĄx the tenses. Answer: Thank you for your suggestion. We have re-written it. l.165: the acronyms in equation 2 are not deïňĄned. Answer: Thank you for your suggestion. We have added it. l.176: "as well as three ïňĄles..." I don't understand. Please clarify. Answer: Thank you for your suggestion. We have re-written it. l.227 and elsewhere: "latitude-altitude distributions" -> "zonal altitude distributions" Answer: Thank you for your suggestion. We have corrected it. l.230: "and is attributed": what is the subject of that verb? Please ïňĄx the writing. Answer: Thank you for your suggestion. We have re-written it. l.246: "to analyze... quantities": please remove. Answer: Thank you for your suggestion. We have removed it. l.257: "the bins above the Earth's surface": all the bins are above the Earth's surface. Please rephrase. Answer: Thank you for your suggestion. We have re-written it. l.266: "nighttime which is below 5km": please ïňĄx the writing. Answer: Thank you for your suggestion. We have corrected it.

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