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

Anonymous Referee #1

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In this paper, the authors consider recent CALIPSO level 3 ice cloud detections and IWC retrievals, monthly gridded on a lon-lat-z grid over the 2007-2016 period. They average these values along various dimensions (altitude and longitude, mostly) in each season, plot and describe the results. They also consider the seasonal variations of the geographic distribution of ice clouds of various optical depths, and the diurnal variation of the altitude distribution of ice clouds of various optical depths.

As far as I can tell, the authors have correctly averaged and plotted the variables present in the level 3 files, and thus the article provides a good documentation of the dataset. The authors however did not in my opinion have clear scientific questions in mind when they drew up this study, and this shows in their methodological choices : 1) unless I'm mistaken the authors built the ICF maps from a 3D gridded cloud dataset, 2) the authors include PSCs in their ice cloud statistics. While these approaches are not technically incorrect, they make it impossible to compare the results presented here with existing literature, in which CF maps are generally built by considering cloudy and non-cloudy profiles in specific altitude ranges, and where tropospheric and stratospheric clouds are considered separately. The second half of the paper includes a puzzling comparison of IWC vs. RH and Temperature, which is never justified and brings little insight. Since the paper does not go far beyond a description of the figures, and there is no discussion of the results, the scientific value of the paper is not obvious. I expand on these points and others below.

Major comments

My first major comment concerns the approach followed by the authors to build a representation of the dataset they study. This representation makes it hard to put their results into perspective by comparing them with existing literature. As the authors do not do this themselves, it is not possible to evaluate if the work done by the authors has any scientific significance. In practice, I mean that as far as I can tell (this is not made clear in the paper) the authors choose to average ice cloud detections over entire vertical profiles (between -0.5km and 20km ASL) to build maps of ice cloud fraction. This choice means that low tropospheric clouds, high tropospheric clouds and stratospheric clouds are all mixed together in the resulting maps. Since those three kinds of clouds are driven by very different atmospheric processes and have very different impacts on the climate system, mixing them all together means it is not possible to derive learning from the maps presented here. Discussion of these maps is limited to e.g. pointing out that large CF over the poles can be linked to the presence of stratospheric clouds. Meanwhile, since the level 3 data used as starting point is altitude-sensitive, it would be entirely possible to separate ice clouds based on their altitude and build ICF maps for low, mid, and high-level clouds. Mixing all the vertical levels together in maps also means that PSCs are included in the ICF in polar regions. PSCs are not necessarily ice : they can be made of NAT crystals that include HNO3 or STS droplets that include H2SO4. It is unclear to me if the stratospheric "ice cloud detections" really only include ice PSCs or if they include all the other PSCs, which are more frequent but should not qualify as ice clouds. In any case, the processes that drive the distribution of PSCs are completely different from the ones that drive tropospheric ice clouds, and both kinds of clouds cannot be mixed together if we want to understand things about them. My suggestion to the authors would be 1) to eliminate PSCs from the dataset before creating any statistics, by using monthly maps of tropopause altitudes based on e.g. ERA5, and 2) to create maps of ICF for different vertical levels and discuss those separately (see below). Altitude levels to separate low/mid/high clouds can be found in e.g. Chepfer et al., 2010. doi: 10.1029/2009JD012251

Section 3.7 (Fig. 9 and 10) tries to identify relationships between ice water content and relative humidity and temperature. The authors never justify was this is a good idea.In the end, they find very poor correlations (always less than 0.5); these rather argue that there is no reason for these variables to be correlated and thus no reason to investigate their relationship. For the time being, this section and the attached figures could be summed up as "Our investigations did not suggest any significant correlation between IWC and either RH or Temperature". I recommend that the authors either explain why it is important/useful to investigate these relationships, and discuss how their results bring something interesting, or drop the whole section altogether.

Finally, the scientific content of the paper is very limited. The discussion of results is limited to explaining sampling variations due to instrumental biases (e.g. the poor sampling of nighttime data during the polar summer, whose obvious explanation is never provided, see below). I would expect this paper to try to learn things about ice clouds, not about how CALIPSO sees them. Once CALIPSO limitations and specific sampling behaviour are taken into account, what can we say with confidence about the remaining variations of ice cloud fractions and IWC? This should be discussed in the paper, and put in perspective with existing results from the literature. It is telling that beyond section 3.1 almost no work is cited, because no perspective is provided to understand why the results are important. The summary and conclusion section cites zero reference. I recommend that the authors attempt to put their results into perspective by comparing them with existing studies describing the global distribution of ice clouds and IWC, maybe using other instruments or reanalyses.

In the end, it is unclear to me wether the article wants to study cloud climatologies (as the abstract suggests) or provide a validation of the level 3 cloud/IWC products (as the conclusion suggests). In the first case it should discuss its results in light of existing literature. In the second case it should compare values of cloud fraction and IWC with other datasets for validation. In the present state it reaches neither objective.

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 (maybe by talking about months instead of seasons).

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 find a correct way to say what you want to say (maybe reference latitudes).

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).

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 behaviour of the backscatter signal) or remove. The discussion of channels (l.132-135) is particularly confused: both 532 and 1064 backscatter coefficients are used to derive level 2 products. The 1064nm sentence has no verb.

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?

l.159: how is a "ice cloud-accepted sampled" defined? Please explain.

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.

L.176: "altitude of 60m": do you mean a vertical resolution of 60m? Or 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 fig. 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).

l.194: see main comment about PSCs.

l.207: I do not understand what the authors mean when they say the ICF peaks under the "flat" 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.

l.207: please skip the definition of the tropopause

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 fix.

l.217-221: the limited sampling of nighttime data during the summer season in the NH polar region has a simple explanation: during JJA the NH polar regions is in mostly permanent 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 seasonal limited sampling of nighttime and daytime data in polar regions. This is a fact related to the orbit of the Earth around the sun, that 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.

l.222:"extrapolation can be used for more complete and accurate data": Extrapolation basically fills out gaps in the data using existing information, but does not add information. Extrapolated data would not be more accurate.

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.

l.234: The "spike-shaped structures" in Fig. 4 are a major concern. If the IWC data contains quality flags, the authors should see if raising the quality requirements make the spikes disappear. 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.

l.239: "we excluded the maximum..." The maximum was excluded from what? Please clarify.

l.263-265: this was already visible and clearer on Fig. 3. Please just reference the previous discussion.

l.265-267: Why is this interesting?

l.269: "negative values of the ICF": you mean a negative diurnal change of the ICF? Negative ICF should not exist.

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 fine 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).

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 confirm known facts about how CALIPSO samples clouds.

l.291-293: this has already been discussed in sect. 3.3. Please sum up.

l.294: "this explains the behaviour..." 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.

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 centres is particularly surprising and should be discussed.

l.311: the values documented here might be correct, but why are they useful/important? Please explain.

l.326-328: this has already been described previously. Please sum up.

l.329-332: this has already been described for Fig. 6. Please avoid repetition.

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 figure. These descriptions are a required step but are not sufficient. Please sum them up and point out to the reader which features are important and confirm or teach us things about ice clouds and IWC.

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.

Sect. 3.7: as stated in the main comments, I do not see the point of this section and the figures 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 coefficients suggest no correlation. Why the comparison should be done is not explained.

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 fix this.

l.472-476: Here the authors state that their analysis suggests that the distributions of ice cloud and IWC provided by the CALIPSO level 3 data are "reasonable and reliable".

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Discussion paper

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 analyse 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 endeavour as important as studying cloud climatologies, but in this present state the paper has achieved neither of these goals.

Technical corrections

Please avoid "the CALIOP". Use "CALIOP" instead

l.42 : "On the other hand": please remove

l. 53: "(0.3<ICOD<1, ICOD>1)" this can be written as "(ICOD>0.3)".

L. 89-90: please put the citations in chronological order

l.110: "the active instruments like the CALIOP..." This sentence is not correct, please rewrite.

l.147: "the CALIPSO lidar instrument released": the lidar did not release a dataset. NASA did.

l.149: up to that point sentences were written in the present tense, now the writing switches to the past tense. Please fix the tenses.

l.165: the acronyms in equation 2 are not defined.

l.176: "as well as three files..." I don't understand. Please clarify.

l.227 and elsewhere: "latitude-altitude distributions" -> "zonal altitude distributions"

l.230: "and is attributed": what is the subject of that verb? Please fix the writing.

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Discussion paper

l.246: "to analyze... quantities": please remove.

l.257: "the bins above the Earth's surface": all the bins are above the Earth's surface. Please rephrase.

l.266: "nighttime which is below 5km": please fix the writing.

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