Review of: ”An assessment of cloud top thermodynamic phase products obtained from A-Train passive and active sensors” by Zeng et al.

1.1 General remarks

The manuscript provides a detailed and statistically based comparison of cloud phase classification by different sensors, MODIS, POLDER and CALIOP. The results are shown as global averages and global maps for different cloud scenes. In general the cloud phase of MODIS and POLDER agreed in 73 % of all observation. For theses confident cases CALIOP has an agreement of 95 %. The remaining disagreement between the sensors is discussed by help of physical basis of the retrieval algorithms. In most cases the reasons for the disagreement could be identified using further information on the cloud scene such as vertical structure derived by CALIOP or horizontal heterogeneity. By this the study helps to identify the weakness and strength of each sensor. This is essential to develop a cloud phase retrieval using the combination of all sensors and to obtain a more confident cloud phase database.
The manuscript provides an important contribution to current and future research and is worth to be published as it substantially helps to interpret cloud phase products from satellite sensors. However, in my opinion the manuscript lacks of two major issues which have to be reassessed in detail before publishing the manuscript. First the interpretation of the results with respect to the vertical weighting function of the different sensors describing which vertical cloud layer is actually sensed by the sensor is insufficiently considered. Second, there already exist a synergetic phase classification combining POLDER and MODIS but was not considered in this study.
Below, I compiled a list of comments which have to be considered in a revised version of the paper. There might be some contradictory statements resulting from my misinterpretation of the text when first reading. I am sure the authors will know how to weight in such cases and how to improve the text to avoid misinterpretations by other readers.

1.2 Major comments

Vertical distribution of retrieved information:
As stated by the authors the three different sensors are sensitive to different parts of the clouds due to the physical basis of the method. POLDER and CALIP rely on information by single scattering which is limited to the cloud top. It is easy to estimate by Lambert-Beers Law that 99 % of the radiation that is scattered only once within the cloud can not have reached cloud optical depth larger 2.3 (CALIOP geometry). For POLDER considering the solar zenith angle this

is even less. However the MODIS method is based on absorption and multiple scattering. Thus the SWIR/VIS channels obtain information also from lower cloud parts which can be quantified by vertical weighting functions (Platnick, 2000; Ehrlich et al., 2009). These weighting functions differ for different wavelength, clouds and geometries but in general show that also lower cloud parts are sampled. MODIS IR algorithm will again have a different vertical weighting function. This means that for the interpretation of cloud phase products the vertical weighting has to be considered. Each sensor sees a different part of the cloud and disagreements in phase retrieval might result just from clouds with vertically separated phases. The discussion about these different views of the sensors is to short in the manuscript. Especially multi-layer and mix-phase clouds might be affected. In this regard the manuscript should be improved. I suggest to add at least a discussion on the problem. I'm sure some of the inconsistencies can be explained by considering the vertical weighting. Splitting up the MODIS retrieval into the SWIR/VIS classification and the IR classification might help too. MODIS IR, POLDER and CALIOP should be sensitive to the same cloud region, only the very cloud top.

Combined POLDER/MODIS algorithms:

In their conclusion the authors refer to an existing synergetic POLDER/MODIS phase algorithm (Riedi et al., 2010). When beginning to read the manuscript my understanding was that one motivation is to look at problems in the individual retrievals which may help to develop combined algorithms. Now there is already a combined algorithm which certainly deals with all the problems described in this manuscript. I do not understand why this algorithm was not applied to the dataset investigated by the authors. Riedi et al. (2010) did already investigate the weekness and strength of MODIS and POLDER. Not on a global scale but it was used to develop a more confident retrieval algorithm. This substantial progress in cloud phase classification should not be neglected here. Using POLDER and MODIS only separately is not the current state of the art. I may cite from the manuscript (conclusions):

"As a synergistic phase algorithm has been created from the PM Dataset (Riedi et al., 2010), more investigations are needed for this new phase product, which will help to quantify the value of the combined POLDER/MODIS retrieval techniques."

There is no better place than this manuscript to do that! You can simply add the combined retrieval to your analysis and demonstrate the improvements compared to the single algorithms.

Spelling style

There a many occasion in the manuscript where already known information is repeated. Summaries are given where it is not necessary because the information summarized was just given. Outlooks are given in the middle of a discussion. Motivations are repeated although the reader knows already why things are studied. Some conclusions are repeated frequently which gives the impression of self-praising comments. This should be avoided. It tires the reader because he has to read all the text but does not get any new information. I will try to list some of the most obvious passages which should be edited. Please read through the whole manuscript and revise it for a more focused and strengthened style.

P8379, 1-4: This was already known.

P8385, 4-10: Much to general. The reader does not need another introduction. Further, most of it was already written in the section before.

P8387, 8-11: Sounds a little self-praising, especially as this statement was repeated several times in the text. Can be moved into the conclusion. Further this synergetic algorithm already exists.

P8387, 13-15: That was already motivated and has not to be repeated here.

P8393, 25-28: Please remove these general conclusions in this section. The second sentence is repeated and irrelevant as the synergetic algorithm already exists.

P8394, 18-20: Move to conclusions. The word ”again” already shows that this comment was already given in the manuscript.

P8395, 13-15: One example of a summary which can be deleted. What is summarized was just written a few sentences above.

Conclusion:

This is not a conclusion. It is a short summary and an outlook. First the intention of the study was repeated, ”This study dedicated....”, ”The study....allow to provide...”, which is quite general but ok. What follows is a summary of the results. This summary is way too short and too general. There are no real conclusions. What is missing are answers to questions like: What are the main news provided in the paper. Which sensor has to be used in what cases? What are the most difficult cases and how can they be solved? Give the main numbers which are the result of the study and should be keept in mind by the reader.

The last part is a very enthusiastic and optimistic outlook but still no conclusion. ”...provide invaluable source of information...” sounds again a little self-praising especially as the benefit for studies on phase transition, etc. was not shown in the manuscript and is only speculation. Similar the phrase ”...it is anticipated...” indicates that this is an outlook but not a conclusion. I don’t want you to remove this outlook but the major part of the conclusion should be concluding remarks on the presented study and not speculations.

1.3 Minor comments

P8373, 22: The authors name three aspects to distinguish cloud phase. I suggest to add the particle shape as an additional aspect. In the manuscript this is mixed with the size but should be a stand alone parameter. The crystal shape affects the scattering phase function and is used e.g. by MISR for phase discrimination (McFarlane et al., 2005; McFarlane and Marchand, 2008;

Ehrlich et al., 2008).
P8376, 3: Please specify for what numbers the decision between ice, liquid or mixed is made in case of super pixels with different sub-pixel. This is essential and may bias the results depending on the decision made here.
P8376, 14: Typo: ”is a multi-...”
P8376, 14: Give number of bands. 443-1020 m looks like POLDER is measuring continuously.
P8377, 6: The description of the POLDER algorithm is a bit too general. Is there a way to give a compromised version of how thresholds are defined and how they are applied. This would be nice. Otherwise, state explicitly that details are given in Goloub et al. (2000).
P8377, 9: ”relatively” is always an expression which alone does mean noting. You mean ”higher compared to POLDER” and ”wider compared to POLDER”.
P8377, 12: Typo: ”band”
P8378, 6: Typo? ”differences exist” instead of ”it exists differences”
P8379, 4: What is the threshold for the thin clouds applied here? Thicker liquid clouds also will give a depolarization signal due to multiple scattering.
P8330, 1: How mixed-phase clouds would be classified from theory? Liquid or ice? In general I don’t understand. MODIS provides a mixed-phase classification. But is neither here nor in Section 2.2.1. stated what thresholds are applied to identify mixed-phase conditions.
P8381, 7: For cloud optical thickness here COT is used as symbol. Before τ was already introduced as symbol for optical thickness. I would suggest to stick to one symbol, preferable τ.
P8381, 15: To give a better overview about the three sensors and their methods I would suggest, to add a table including at least wavelength, pixel size, physical basis (single scattering, absorbtion), depth of vertical weighting for the penetration into the cloud, general restrictions.
P8381, 20: Here the data is divided into ice, liquid and mixed-phase. But how mixed is defined in all three individual retrievals? The general description only deals with the separation of liquid and ice. The decisions made for a classification of mixed-clouds has to be add in a revised manuscript as this might be of importance to understand the differences in the three retrieval products. Biases may erase as the single algorithms define mixed-phase clouds for different conditions.
P8381, 5: ”annual” is somehow misleading. It is only one year that was analyzed. Annual suggest more than one plot or more than one year of data. Better to repeat here the period of data you used in the study.
Figure 1: Numbers in this color code are very difficult to read and I think the pie chart is not necessary in addition to the numbers. Some contributions are not visible in the chart. I suggest to change the plot into a table which presents the 3x3 matrix where only the numbers are given. The fill color of each field may be chosen as color coded related to the numbers.
Can you additionally give the total number of cloud phase for each sensor without combining the

two sensors. This would be easy to implement in the suggested table. Then it would be easier to see, which phase dominates or not. E.g. The mixed-mixed-combination seems to be very rare. Is that because the algorithms differ so much or just because there are in general only few cases classified as mixed-phase?

P8381, 20: Similar to the figure, your numbers do not reveal for which phase the algorithm works best as the numbers are related to the total number of observations. If there are in general more liquid clouds present, then it is not surprising, that liquid clouds also give the largest contribution to the pixels with agreement. What I think is interesting is if there are tendencies that one phase is identified better than another. I would suggest to give the percentage of matching classifications for each individual phase. xx % of all liquid clouds have been identified consistently, xx % of all ice clouds.

P8383, 10: For this single cases of disagreement a discussion is given here but not for the others. Above you refer to Sections 5.4 and 5.5 for a discussion about reasons of the disagreement. This is inconsistent. I would prefer to discuss the results right here or shift all the discussion to section 5.

P8383, 19: ”satisfactory”. Are 75 % really satisfying? I wouldn’t agree considering that cloud phase is such an important parameter.

P8385, 13: What means layer-integrated? Which layer is integrated, especial for opaque clouds? CALIOP is not known to be able to penetrate clouds with opt. thickness higher than 3-5. So how do you expect to assess POLDER and MODIS with CALIOP?

Figure 3 and 4: Fig. 3 and 4 differ not much. Why? How many nonopaque clouds were observed? Are they really nonopaque as I can not imagine how CALIOP can penetrate a typical liquid cloud.

P8386, 21: How CALIOP defines a mixed-phase clouds? How many cased of mixed-phase did CALIOP observe in general. The low number of mixed-phase cases identified by CALIOP is probably due to the method. As in most cases the liquid fraction is dominating the mixed-phase clouds and thus the radiative transfer. Ice might be located mostly at lower cloud layers due to sedimentation. Lower cloud layers are not seen by CALIOP but by MODIS using SWIR/VIS wavelength.

P8387, 3: Please do not repeat yourself. Why you give here a summary from what you just presented five sentences before? Include your discussion right there!

P8387, 24: Do not Fig. 5-7 give the same information as do figures 3 and 4? I would suggest to combine the figures by just adding the numbers of agreement in Figure 3 and 4.

Why you are now adding broken clouds? Results for broken clouds should have been consistently shown also in a $\gamma - \delta$ plot similar to figures 3 and 4.

P8387, 24: above you discussed that CALIOP did not show many mixed-phase clouds. Why now this class is totally neglegted? Probably because CALIOP is not able to classify mixed-phase clouds at all. Then please revise your statement from above.

P8388, 2-3: This conclusion is not new. It was already shown in Figure 3 and 4. So again I suggest to combine Figures 5-7 with 3-4.

P8389, 12: This was already announced at Page 8385 line 1-2 but not discussed in section 4.

P8390, 10: Wouldn't it make sense to use the scattering angle instead of viewing zenith angle? Scattering at cloud top and thus the scattering phase function are related to the scattering angle between the direction of the Sun and the viewing angle.

P8390, 17-18: This sentence is not needed. The reader knows that already. Please remove.

P8390, 19: Introduce that and why MOIDS IR is now shown explicitly.

P8390, 25: Give numbers for the magnitude of increase /decrease in the text.

P8390, 28: "optical properties"? You mean SWIR/VIS to be consistent.

P8391, 2: Again, give numbers in the text.

P8391, 13: It is not only the forward scattering in combination with multiple scattering. Due to the particles scattering phase function clouds do have a certain BRDF, which looks very similar to the particle phase function (Ehrlich et al., 2012). As the phase function is not constant over the scattering angles observed here, also the BRDF varies. And this is what you observe here.

P8391, 16: "solar radiation" I would change this into "solar reflectivity".

P8391, 23: This assumes that the position of the Sun is in line with the MODIS cross line. This is only valid for the satellite flying at latitudes where the sun stands in zenith position. For other geometries at higher latitudes the scattering angle may differ. It might even become constant for all the swath when the sun is far behind the satellite.

P8392, 1: Typo: "is observed"

P8392, 6-7: This sentence is again written like an introduction but only wastes space. The results follow very soon in the text and that the fact is important is obvious otherwise you would not have presented it. So you can remove this sentence.

P8392, 9: "strong polarization signature" this is related to POLDER and not CALIOP. Please add this here as the reader might think that it also holds for CALIOP. But CALIOP observes backscattered radiation where spheres have no polarizing effect.

P8392, 15: How the cirrus cases are choosen? Not all thin clouds observed by CALIOP may be cirrus. Is the -40C threshold used here?

P8392, 17: OT is the third definition of cloud optical thickness in the manuscript after τ and COT. Please use the τ.

P8393, 6: Is MODIS high sensibility to thin cirrus due to the IR classification or the SWIR/VIS classification?

P8393, 21-22: There is no discussion why these cases MODIS-ice and POLDER-mixed occur for specific cloud optical thickness. Do you have any explanation?

P8394, 2-6: Please do not mix up brightness temperatures and brightness temperature differences. BT-Differences make use of the different refractive indices of ice and liquid water (imaginary part). The refractive index of supercooled droplets is still the same as for warm liquid droplets. So why there should be a problem? Only the 11 μm brightness temperature alone is affected but not the differences.

P8394, 10: Remove ”to one year of”. This does not have to be repeated here.

P8395, 20: What does ”unsaturated” mean here?

P8396, Sec. 5.6: Quite short. What about ice and snow surfaces below liquid clouds? This should be a problem to for MODIS-SWIR/VIS and Polder if cloud optical thickness is low.

P8397, 10: ”High confidence”. What if both sensors are wrong? Ok, this can not be validated here. But should be mentioned. 100 % agreement between POLDER and MODIS does not mean there is for 100 % the correct cloud phase identified.

P8397, 13: Why the statements are written so general. The general problems have been clear already in the introduction. Now with your study you have much more insight. Details should be repeated in the conclusions.

P8397, 29: ”high confidence”. 75 % agreement as shown in the study is not really high in my opinion!

Figure 2: The scaling is misleading. All maps should have the same color code scale. Otherwise a comparison is not possible. Of course, then e.g. the mixed-mixed class can not be seen at all in the plot. Alternatively, you can scale all plots to the total number of observations in each class. Then the plots are not anymore comparable with regard to the numbers but the main outcome of the plots is the global pattern, so numbers do not matter. Theses numbers are already presented in Fig. 1.

Figure 3-4: Here the same scaling problem occurs. When using different scales to obtain a similar color code, then you also can normalize all plots to the total number of observations of each class.

Figure 8: The blue line in panel b is crossing the legend. You can move the legend further right to avoid that.

Figure 8: Legend in panel a. The letters are overlapping. Please add some more space.

Figure 8: Description of panel b is missing in the figure caption.

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