

Reply to: Anonymous Referee #2

Major comments:

1. Line 71: the author mentioned that “no research-based, objective alternatives to the 0/0/100/100 interpretation currently in use have been put forward”. However Line 250 reviewed the validation work conducted by Wang et al. (2016) which also estimated the cloud fraction for four MODIS cloud masks. The reviewer suggested reviewing Wang’s work in the introduction and also emphasizing what’s new in this work. For example, Wang’s work focused on daytime only, this work included daytime, night time and both day and night time. Moreover, this work examined how those four cloud fractions changed for different MODIS cloud mask algorithm paths and latitude regions.

Wang et al. (2016) is an excellent study. It validates the MODIS cloud mask (daytime only), with a focus on multilayer clouds, and considering different cloud regimes (with 2D histograms). However, the latter study does not provide a CALIOP-based cloud fraction for each of the four MODIS cloud mask classes. It may be possible to obtain these fractions (global values only) by an analysis of the confusion matrix (Table 2) presented by the authors. However, no direct information is provided about these values, how such statistics could be derived, or why (while this is the main objective of our study). For this reason, I prefer not to change the Introduction. I do, however, fully acknowledge the work of Wang et al. (2016), and refer to their results in the Discussion.

2. The cloud fractions were derived with two months data, i.e., January and July 2015. While the author demonstrated the fractions could have a large variability depending on environmental conditions. Could they also have a seasonal variation? How valid to apply the same numbers to different seasons for the whole MODIS mission?

The seasonality of the CALIPSO-based cloud fraction for MODIS cloud mask classes can be expected wherever environmental conditions are dominated by a strong seasonal cycle, in particular regions where the cloud regime changes noticeably. On the other hand, seasonal environmental change is consistent with changes in the frequency of per-location MODIS algorithm paths. Therefore, when regional CALIOP-based fractions per algorithm path are used (instead of fixed global fractions) the seasonality effect is balanced (at least partially). An operational use of CALIPSO-based fractions would require the development of a relevant ‘climatology’. An investigation of such a climatology would be an interesting extension of this study.

3. The author considered CALIPSO data as “ground truth” by including all cloud layers detected by CALIOP. As CALIOP data reported quality flags, it is possible to choose confident clouds only. For example, including clouds with cloud-aerosol discrimination score between 20 and 100 (low, middle and high confidence) or 70 and 100 (high confidence only) by specifying the range of parameter CAD_Score. Not sure how this filter might change the current findings in the paper.

Our study found that 95.6% of analysed CALIOP observations had CAD confidence of at least 70%, and confidence was below 20% for only 1.5% of data. These statistics did not differ between day and night, or January and July. High, stable CAD values makes it possible to conclude that filtering for data with CAD >70% or >80% would have no impact on the results. On the other hand, CAD results varied slightly more in the tropics, and this issue is discussed in the paper.

4. In the paper, the cloud fractions are further estimated for each cloud mask algorithm path and day/night conditions. It is noted that the CALIOP has different detection sensitivity during day and night, i.e., CALIOP is able to detect more thin cirrus clouds around the tropical region at night than during the day. This might help understand the day/night discrepancies in Figure1-3.

Additional information about CALIOP daytime/ night-time sensitivity has been added to the Discussion.

5. As briefly touched by the author in Line 238, the level 2 CALIOP cloud layer product reported detected cloud layers only. It is very possible there are aerosol layers detected and those aerosol layers would be reported in aerosol products but not in cloud products. In this scenario, the sky is not exactly “clear”. To avoid confusions, some researchers use “cloud free” instead “clear”.

I agree that ‘cloud-free’ is much more accurate in the context of this research, and I have changed ‘clear’ to ‘cloud-free’ whenever possible. Nonetheless, I have retained ‘clear’ in the name of the MODIS cloud mask, since these names are widely (and officially) used in MODIS product documentation.

Minor comments:

1. Abstract: keep consistency when describing four cloud fraction numbers and cloud mask categories. Line 7: “confident cloudy”, “probably cloudy”, “probably clear”, “confident clear”. Line 14: 21.5%, 27.7%, 66.6%, 94.7%.

Corrected, as suggested.

2. Line 16: “selected locations”? Please give a few locations as examples.

Examples added, as suggested.

3. Line 17: “error” → “uncertainty”?

Changed – ‘uncertainty’ is the more relevant term.

4. Line 18: What is “our method”?

The method used in the study to calibrate MODIS cloud amount. This sentence has been rephrased.

5. Line 19: “robust” is a strong word. Does the author would like to say something like “We recommend using the cloud fraction ratios found in this work to improve MODIS estimates.”

The sentence has been rephrased.

6. Line 20: “other mission”? Other passive missions?

Passive cloud imagers – the sentence has been rephrased.

7. Line 24: “W m-2” should be “W m⁻²”.

Corrected.

8. Line 48: “The procedure implemented by NASA...” → The procedure implemented by MODIS science working group?

I agree. The procedure was developed by the MODIS Science Team or – more precisely – the Atmosphere Discipline Group within the MODIS Science Team. As the MODIS Science Team is a collaboration coordinated by NASA, I used NASA, but I agree that MODIS Science Team is more accurate. NASA has been changed to MODIS Science Team throughout the manuscript.

9. Line 51: “- see, for example, ” → e.g. ?

Changed.

10. Line 54: “NASA’s approach” → standard procedure? It is not an approach from an agency. Instead, it is from MODIS science working group.

Changed. See reply to comment 8.

11. Line 54: “... are both allowed and in use.” → “... are adopted by other groups.”?

Changed (shortened) to: “... are in use.”

12. Line 63: Moved “in Switzerland” after “observations”. It would be nice to specify the number of ground-based observations, i.e., “... compared MODIS data with n ground ground-based observations...”.

Changed, as suggested.

13. Line 70: “NASA standard approach” → standard procedure or standard approach?

Changed to ‘procedure’.

14. Line 71: “... currently in use have been put forward” is confusing. Does the author mean “... currently widely used are still missing” or something like that?

Rephrased.

15. Line 72: “... based on quantitative, empirical lidar observations” is confusing. Does the author mean “... based on a quantitative analysis with lidar observations”?

Rephrased.

16. Line 75: The CALIPSO was launched in 2006 instead of 2016.

Corrected.

17. Line 77-78: Consider removing “This is because” and “which means that” to make a concise and formal statement.

The sentence justifies why the study uses CALIOP as a reference. The phrase, “Furthermore, the use of short...” at the beginning of the following sentence is a logical continuation. Therefore, I prefer to leave the paragraph as it is.

18. Line 83: Add “with CALIOP observations” after “... correspond to”.

Added, as suggested.

19. Line 83: Again it is not an approach from an agency. The author probably meant “current standard approach” or “current standard procedure”.

Changed. See also reply to comment 8.

20. Line 84: Does the author mean “Finally, we evaluate whether the MODIS Level 3 standard approach is reliable”?

Clarified, as suggested.

21. Line 101: Consider removing “This is made available”.

Rephrased, as suggested.

22. Line 103: Consider replacing “product; this was used to assign” with “with”.

Rephrased, as suggested.

23. Line 108: Below 8.2 km, CALIOP has a horizontal resolution 0.333 km not 0.33 km.

‘0.33 km’ corrected to ‘0.333 km’

24. Line 109: Between 20.2 km and 30.1 km, CALIOP has a horizontal resolution 5/3 km and vertical resolution 180 m. From 30.1 km to 40 km, the horizontal resolution is 5 km and the vertical resolution is 300 m. Please refer to Table 2 in Winker et al. [2006].

Corrected and clarified, as suggested.

25. Line 114: “CAL_LID_L2” → level 2 cloud layer products.

Changed, as suggested.

26. Line 115: (version 4.20) → (version 4.20, CAL_LID_L2_01kmCLay-Standard-V4-20)?

Product codename added, as suggested.

27. Line 119: “Number Layers Found” variable → “Number_Layers_Found” parameter

Changed, as suggested.

28. Line 130: “... January and July 2005 ...” should be “... January and July 2015 ...” Any special reasons to choose these two months?

Yes, these two months represent atmospheric conditions for summer (July) and winter (January) in the northern hemisphere. The selection of these months makes it possible to investigate contrasting cloud regimes in mid-latitudes (more cumuliform in summer, more stratiform in winter) and season-dependent conditions for cloud detection (e.g. snow cover).

29. Line 141: Add “MODIS” after “perfect” would help a reader understand.

Added, as suggested.

30. Line 147: Based on Table 1, should the number “86.7%” be “64.2%” at night?

In fact, it should be ‘84.2% at night’ (as in Table 1) – corrected.

31. Line 151: Should the number “77.4%” be “73.3%”?

Corrected.

32. Line 157: Is this region “ITCZ”? Does this high frequency misdetections due to high sensitivity of CALIOP? In other words, CALIOP detected very thin cirrus clouds which are invisible to MODIS.

Yes, it is the intertropical convergence zone. I have expanded on cloud detection by MODIS and CALIOP at low latitudes in the Discussion.

33. Line 159: "... MODIS tended to falsely detect cloud rather than fail to detect it". This sentence is confusing. Does this mean higher percentage occurrence or larger area spatial extent? Should "Only" be removed?

The statement was deleted.

34. Line 166: It is not exactly "every fifth MODIS" even though the percentage is about 20%.

Changed to "one fifth of MODIS".

35. Line 172-173: "no significant day/night difference" even though it is 12.3% for 'probably cloud'?

Clarified.

36. Figure 3g and 3h: What does black color over Southern Ocean mean?

It means there were no confident clear detections by MODIS in these regions at that time.

37. Line 183: Should 'probably cloudy' be 'probably clear'?

Corrected – 'probably clear' is the correct term.

38. Line 186: What does "this" in "..., but this was ..." mean?

Rephrased and clarified.

39. Table 3: Use same terms to describe snow-covered conditions in the context and table caption. For example, use "Snow-free" and "Snow-covered" or "No snow" and "Snow".

Corrected, as suggested ('snow-covered' and 'snow-free' are now used consistently).

40. Line 205- 215: The author chose three cloud masking algorithm paths for detailed discussion. It would help a reader understand why those three if providing some explanations. Explain "Results" in Line 205 and "A similar pattern" in Line 211. Which results? Which pattern?

Four algorithm paths are described in the text. The first is "the combination of night, an oceanic background and snow-cover (or sea ice)". This scenario is notable because it "constituted the 'most cloudy' scenario". The second is "snow-free land at night", this was chosen because: "Results [for it] were most consistent with the standard Level 3" (already mentioned in the manuscript). The two other scenarios are "snow-free land during the day", and "ice-free oceans". The choice of the latter is justified in the paper: it is "the most frequent algorithm path". I agree that the justification of the choice of "snow-free land during the day" was missing. Therefore, following the Reviewer's suggestion, I have added an explanation (it is of particular interest for land/ vegetation MODIS remote sensing).

Lines 205 and 211 have been clarified, as suggested.

41. Line 223: Add a dot between MODIS collection "6" and "1"?

There are two conventions in use: a three-digit name with leading zero (005, 055, 006, 061, etc.), or to divide a collection number by 100 and use a coma (5.0, 5.5, 6.0, 6.1, etc.). I prefer to use the first, hence '61' has been changed to '061'.

42. Line 225: It is confusing to discuss level 3 product here since no plots or work on level 3 clouds presented so far.

Clarified. The implications for Level-3 data are presented in the Discussion, but not before. The first paragraph of the section only introduces issues that are discussed in the following paragraphs. I have made this point clearer in the new version of the manuscript.

43. Line 235 and Line 240: The author claimed that temporal and spatial separations between Aqua and CALIPSO do not impact the results significantly. If not complicated, it is a good idea to show the plots when using different time and range shifts.

I have prepared the plots, as suggested. I also agree that they might be interesting for some readers. However, I leave it to the Editor to decide whether they should be included in the main text, or as additional/ supporting online material (the latter would be my choice).

44. Line 246: Explain acronym “AVHRR”.

Explained, as suggested.

45. Line 316: What is the spatial grid used to plot Figure 8?

All figures use the equirectangular projection with $2.5^\circ \times 2.5^\circ$ spatial resolution.

46. Line 321: The author drew a conclusion “Whenever MODIS cloud amount is estimated at a spatial resolution of ~ 10 degrees of finer, ...”. There seems no evidence in the paper to support this conclusion. Something missing?

Ten degrees longitude/ latitude was the approximate area of cloud amount uncertainties in China, along the coast of the Arabian Peninsula, north-west Africa, and some locations in North America. However, I agree that the figure could be misleading when considering, for example, polar regions where the area is much larger. Consequently, the reference to “10 degrees” has been deleted, and replaced by “regional/local”.

47. Line 324: Discussions on MODIS level 3 cloud product could be moved from “Summary and Conclusions” section to previous “Discussion” section

I prefer not to move the discussion about Level 3 data from the Discussion to the Results. The key ‘technical’ objective of the study was to derive CALIOP-based cloud fraction from MODIS. The outcome of this work is reported in the Results section. A discussion of the implications of these results for calculating global cloud amounts is a different matter. In my opinion, the present structure of the manuscript clearly separates the results of the study’s calculations from a discussion of their impact.