

## Response to reviewers

This document details our reply to the reviewer comments and lists how the manuscript has been modified. Original reviewer comments are in italics and our itemized response is in red color.

Reviewer #1:

*General comments: This paper is an important contribution, presenting characteristics of three classes of marine low clouds classified by their morphology and type of mesoscale cellular convection (MCC) on a global scale. The classification is based on an artificial neural network scheme developed in a previous study. The physical and radiative properties are presented. Many results are new and important for understanding the cloud and precipitation processes in these types of clouds that are main modulators of cloud radiative forcing with high albedo and cloud coverage. The manuscript is well written and the results are clearly presented. I recommend that this paper is published with minor revisions. There are several suggestions for revisions as described below.*

*Major points*

*1. P6986, L12: The retrieved optical thickness and effective radius are not available or not accurate when the sun is low. Therefore, high latitude regions in winter seasons could not be investigated with the method used in this paper. Nevertheless, Fig. 5 present results for global maps including the high latitude regions in winter. I am wondering how reliable these results are.*

We agree with the reviewer that MODIS cloud optical depth and effective radius retrievals at high latitudes may be problematic due to the influence of low sun elevation angles and the possible presence of sea ice particularly during the winter months. For this reason, we excluded all low cloud regions beyond about 65° North or South from our statistical analysis. However, in Fig. 5 we still present results for these regions but the results should be taken with caution. We modified the text as follows:

In section 2:

“Also, it is emphasized that cloud retrievals based on MODIS radiances may be problematic at high latitudes due to the effect of low solar zenith angles and the possible presence of sea ice during the winter months. Therefore, low cloud regions above and below about 65° N or S are excluded in the subsequent analysis.”

In section 3:

“Closed MCC are also the dominant type of low cloud morphology in the Arctic Ocean east of Greenland (i. e., the Greenland Sea). However, this result should be taken with caution due to the inherent uncertainty in MODIS cloud retrievals (i. e., cloud optical depth and effective radius) at high latitudes mentioned in Section 2.”

*2. Sect 4: In my view, I do not feel strong necessity to describe technical details in this section to present the case study results that are not very interesting. The results are all reasonable but it seemed to me that there are few new findings. If implications from the case study and association with subsequent sections are clearly described, it may be helpful for readers.*

Our study presents a novel concept for analyzing the impact of cloud morphologies on the physical and radiative properties of marine low clouds. As such, we think that a proof of concept as presented in section 4 is adequate. However, we shortened this section to make it more concise and appealing to the reader. In particular, we omitted some technical details as suggested by the reviewer.

*3. The mesoscale-domain-mean shortwave reflectance and transmittance are determined primarily by cloud fraction and cloud optical thickness. While the authors present variability of cloud fraction, there is no explanations about the cloud optical thickness. Before an explanation of the variability of mesoscale-domain-mean shortwave reflectance, it would be of interest to see the variability of cloud optical thickness.*

We agree with the reviewer and added cloud optical thickness to our analysis in Section 6.

*4. Also, results in Figures 14 and 15 seem to be for domain-mean reflectance and transmittance including contributions from cloudy and clear-sky pixels in the domain. Please specify that definitions to avoid misunderstanding. On Page 6999, line 24, I think this sentence is misleading because the mesoscale-domain-mean shortwave reflectance discussed here may be confused with mean reflectance of cloudy pixels excluding contributions from clear-sky pixels.*

We rephrased the text in section 6 to be more clear:

“We emphasize that the inferred statistics of radiative properties of MCC types are representative of the cloud field on the scale of several tens of kilometers (including clear-sky pixels) rather than on the scale of individual clouds contributing to the cloud field. This is because the footprint of the CERES instrument is about 20 km and, thus, much larger than the footprint of CloudSat or CALIPSO. Therefore, we are unable to infer the radiative effect of individual Sc clouds but only the radiative effect of the (mesoscale) field of clouds on the scale of a few tens of km.”

*Minor points*

*Page 6986, line 9: The MODIS measures the radiances, and “irradiance” should be replaced with “radiances”.*

Done.

*Page 6986, line 10: Please specify a Collection number of the MODIS product.*

It's collection 5. We added this information in Section 2.

*Page 6987, L9: “The higher horizontal and vertical resolution of the lidar allows. . .” Exact values of the resolutions would be of interest.*

The resolution of the lidar is roughly 1 km along track, 300 m across track and 75 m in the vertical. This is now mentioned in the text of Section 2.

*Page 6990, line 10: “previous satellite-based estimates (Leon et al., 2008)” Would be interested in some more details of similarity and difference between methodologies used in the present study and that of the previous study.*

The similarity is that both studies use a combination of the CALIPSO feature mask and the CloudSat GEOPROF cloud mask for the detection of marine low clouds and drizzle. Also, there are some minor technical differences such as the choice of thresholds used in the CPR cloud mask, the height threshold used for the identification of low clouds, and the Z-R relationship applied for the conversion of radar backscatter to rain rate. The important novel element of this study is the combination and analysis of cloud, precipitation and radiation observations from the A-train within the cloud classification framework, which allows us, for the first time, to analyze the physical properties and radiative impact of low clouds as a function of the cloud morphology on a global scale.

*Page 6991, line 11: Please exactly define the “frequency of occurrence of closed MCC”. Is this a fraction of frequency of closed MCC occurrence to total frequency of all low cloud regimes?*

All MCC observations are binned into  $5^\circ \times 5^\circ$  regions. In each grid box, the frequency of occurrence is the fraction of occurrences of a certain MCC type to the total number of all MCC occurrences expressed as a percentage value. We clarified this in section 3 and in the caption of Fig. 5.

*Page 6995, line 24: “low cloud fraction determined from the CPR”. Why not use the CALIOP here to determine the low cloud fraction?*

The detection of low clouds uses a combination of radar and lidar following Marchand et al. (2008) and Mace et al. (2008). Thus, the lidar information is taken into account whenever possible. We rephrased the sentence to avoid confusion.

*Page 7002, line 20: “the differences are not found to be statistically significant”. If so, I think the first sentence in Conclusion 6 should be just removed from the Conclusions.*

Actually, the differences are statistically significant based on the Wilcoxon rank-sum test. We modified our statement accordingly:

In section 5.3: “Based on the nonparametric Wilcoxon rank-sum test (Wilks, 2006), the median cloud top height differences among the MCC categories in the global data set are statistically significant at the 95 % confidence level.”

However, we removed the part of the sentence in the conclusions to make it more concise.

Reviewer #2:

*This is an great overview of results from the A-Train satellites using a statistical technique to classify low-level clouds into several 'types.' The scope is quite broad, looking at the entire globe, picking out several important regions in the subtropics and midlatitudes, and including the seasonal cycle. This is a preliminary study, and one hopes that it is followed up with more detailed examinations of some of the intriguing results that are reported. The main weakness of the paper is that it only uses one year of data, but as the authors have explained, this is already a considerable effort. I think the paper should be published with just some minor revisions, which are described below mainly as some questions and suggestions.*

#### Minor Comments

*1. In Section 2, a couple of points that would be stronger if quantified: (a) the fraction of scenes that lack cellular structure (maybe this isn't part of the algorithm, though), (b) the fraction of scenes that are homogeneous Sc that are grouped into closed MCC, (c) fraction of scenes obscured by higher clouds, and (d) fraction of scenes excluded because they are clear. Maybe these numbers are not available, but I think many readers would be appreciative if they could be included.*

All accepted scenes are classified into one of the four MCC types. By definition, (a) the fraction of scenes that lack cellular structure and (b) the fraction of scenes that are homogeneous are identical. Because the number of low cloud scenes without MCC is small (only a few percent), we decided to merge the no-MCC category with the category for closed MCC. This is stated in section 2 of the manuscript. However, because the no-MCC class is so sparsely populated this merger does not affect our analysis or statistics in any significant way.

The fraction of scenes obscured by mid- and high-level clouds (c) and the fraction of clear scenes (d) vary tremendously in space (and maybe season). Unfortunately, we did not save these statistics as part of our processing but plan to look at these in the future. We think the number of clear scenes is very low but the number with some high clouds is strongly variable, going from a few percent in subtropical stratocumulus regions to a majority in the ITCZ.

*2. (very minor!) "Circumpolar Southern Oceans" is a curious choice of labels. Why not the more common "Southern Ocean"? Maybe to be more specific, it could be the "midlatitude Southern Ocean" (MSO)?*

Agreed. We changed "Circumpolar Southern Oceans (CSO)" to "Southern Ocean (SO)".

*3. In a couple of places the "Arctic Ocean east of Greenland" is mentioned, but it isn't clear if that is the same as the "North Atlantic" box in Figure 1 or not. If so, I think that box is entirely within the Atlantic, and shouldn't be called the Arctic. If not, then I think it should be labeled or better explained.*

With "Arctic Ocean east of Greenland" we mean in fact the Greenland Sea. This is now mentioned in

the manuscript in Section 3 and the conclusions. The Greenland Sea is located north of about 65 N and, thus, in geographical terms part of the Arctic. It is not part of our North Atlantic box. There is clear evidence for the presence of low (presumably mixed-phase) clouds in this particular region from both CloudSat and MODIS data. However, we deliberately excluded this region from our analysis because of the limitations of MODIS radiances and cloud retrievals at high latitudes and the potential impact on our cloud classification algorithm.

*4. On page 6989, the text says "... subtropical high-pressure systems and considerable upwelling of cold oceanic waters..." This is in regard to seasonality, so I wonder if there is a link between the seasonality of upwelling and clouds? If not, then consider removing that part of the sentence.*

The seasonal cycle is indeed strongest in regions promoting upwelling (see for example Fig. 6 in Wood, 2012). However, it is not clear if the seasonality of low cloud fraction is simply upwelling limited. These regions have strong SST seasonal cycles due to a shallow mixed layer in the ocean which effects the seasonal cycle of low cloud fraction but there are also other factors such as continental effects on the seasonality (e. g., Richter and Mechoso, 2004). We rephrased the sentence as such:

"The seasonal cycle tends to be stronger in the subtropical regions west of continents that have strong subtropical high-pressure systems and considerable upwelling of cold oceanic waters such as in the southeast Pacific (SEP) and southeast Atlantic (SEA). These regions have stronger seasonality in low clouds because of the strong seasonality in low cloud controlling factors such as SST and LTS."

*5. On page 6990 it says that LTS is derived from ECMWF analysis. Can any more detail be included? Is this from 5 years like in Figure 1, or just for the year of MODIS data that is used for the MCC types? Monthly or 6-hourly? On the native grid, or coarsened? Speaking of the time periods, that year of MODIS data should be stated explicitly in the text, but I only see that it is 2008 in the captions of Figure 5 and Table 3 and in the case study section.*

The meteorological data is provided by the ECMWF-AUX CloudSat data product. The ECMWF-AUX data set is an intermediate product that contains the set of ancillary ECMWF state variables interpolated to each CloudSat CPR range gate. The interpolation is performed based on the geolocation and time stamp of the CPR profile with respect to the bounding ECMWF grid points and analysis times and linearly interpolated in space and time. All statistics are derived based on 5 years of ECMWF-AUX data. We now state this in the manuscript and in the caption of Fig. 4.

We also added the year number (2008) of the analyzed MODIS data to the text in Section 3 as suggested by the reviewer.

*6. On page 6990, it is mentioned that there's a lag between LTS and cloud cover, so there are other players at work controlling the cloud. Is this part of the analysis based on monthly averages, or instantaneous data? If instantaneous, it should be mentioned that the LTS-cloud correlation gets weak*

on short timescales (see Zhang et al. doi:10.1175/2009JCLI2891.1 for example).

Thanks for pointing this out to us. Indeed, the analysis is based on interpolated (in space and time) data from the ECMWF-AUX CloudSat data product (see our reply to previous comment). We added a sentence in this regard including the reference provided by the reviewer: "Also, the correlation between LTS and cloud fraction is weaker on shorter time scales as suggested by Zhang et al. (2009)."

7. On page 6991, there's a sentence: "Interestingly, there is also clear indication of closed MCC types over the equatorial cold tongue in the eastern equatorial Pacific especially during boreal winter (SON)." Why is it interesting?

We find that the vast majority of low clouds in tropical regions exhibit cellular but disorganized morphological characteristics. The only region different in this regard is the equatorial cold tongue complex in particular during boreal winter. While this observation may be expected and obvious to the reviewer, we think it is an interesting feature and worth pointing out. However, we rephrased the sentence slightly and used "notably" instead of "interestingly".

8. Similarly, on page 6992: "Interestingly, the most prevalent MCC types in the NEA are disorganized and open MCC with little contributions from closed MCC, which in turn explains the overall low value in low cloudiness." I'd suggest rewriting to say why it is interesting, maybe something like, "The relatively small value of low cloudiness in the NEA may be explained by noting that the prevalent MCC types are disorganized and open, while closed MCC is much less common."

Agreed. We rephrased the sentence as such:

"Interestingly, the most prevalent MCC types in the NEA are disorganized and open MCC with little contributions from closed MCC. Thus, the relatively small value of low cloudiness in the NEA may be explained by the small contributions from closed MCC types, which are less common in this region."

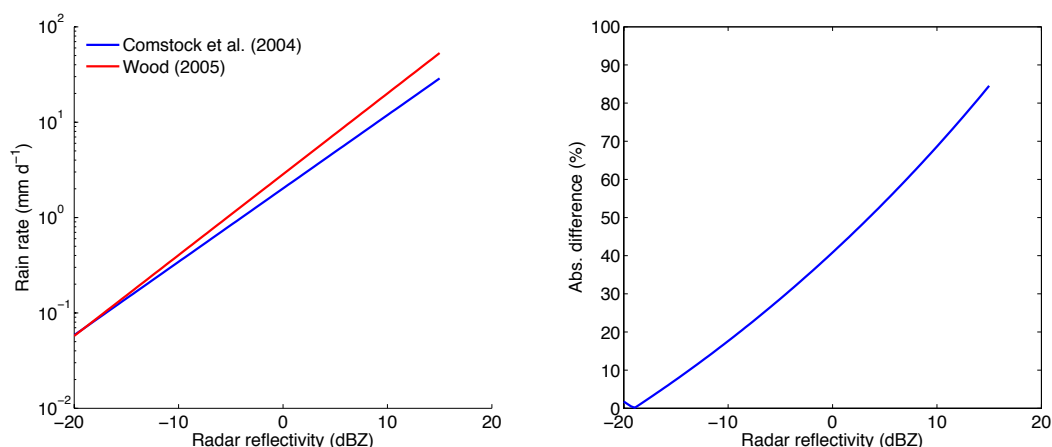
9. page 6996, I wondered whether the strongly skewed distributions of cloud fraction suggest something about time scales of cloud changes (i.e., autocorrelation in cloud fraction)?

This is an interesting comment. The value of cloud fraction is determined directly from the number of cloudy profiles seen by CloudSat/CALIPSO for each overpass. So, spatially there is some level of autocorrelation within a given overpass segment. However, each consecutive overpass occurs at a different location within our broadly defined regional boxes, which should decrease the autocorrelation from one overpass to the next.

10. page 6996, The text mentions using an appropriate Z-R relationship, but I wonder whether a Z-R relationship valid for subtropical stratocumulus would be valid for extratropical stratocumulus (where presumably there could be differences in vertical velocity and drop distributions)?

This is a good point and we agree with the reviewer that there could be differences in drop distributions and vertical velocities between subtropical and midlatitude stratocumulus that could affect the validity of the Z-R relationship. From extratropical marine stratocumulus (Fig. 11 and Table 2 in Wood 2005), the best fit Z-R is  $Z=12.4 R^{1.18}$ , whereas we used  $Z=25 R^{1.3}$  following Comstock et al. (2004). Fig. 1-R shows a plot of these two Z-R relationships and the absolute difference over the range of radar reflectivity values encountered in our study. As can be seen from the figure the error is on the order of a few tens of percent for lightly and moderately drizzling stratocumulus but much larger in the case of heavy drizzle. Clearly, more research is needed to better constrain the Z-R relationships and we acknowledge this point in Sec. 4 of the manuscript:

“This Z-R relationship has been found appropriate for subtropical marine Sc clouds (Comstock et al. 2004) and we apply it consistently to all marine low clouds in our study. However, we emphasize that Z-R relationships are inherently uncertain and may induce considerable errors due to uncertainties in the microphysics (e. g., drop size distributions) and differences in the environmental conditions (e. g., vertical velocities).”



**Figure 1-R: Z-R relationships for subtropical (blue) and extratropical (red) marine stratocumulus (left panel) and the absolute difference between these relationships (right panel) as a function of radar reflectivity.**

11. page 6998 & Conclusion #6, This study nicely shows the difference between the cloud types in drizzle and CTH, but this conclusion could be made easier to digest. Wouldn't it sound a lot catchier to simply say "Thicker clouds rain more." ?

Following the reviewer's suggestion, we included this sentence at the end of our discussion in Section 5.3 and in conclusion 6.

12. The conclusions in general come across as a little wordy. The numbered list is great, but I'd suggest trying to make each item shorter, more like 2-3 lines. Figures (all very minor, just suggestions)

Following the reviewer's suggestion, we shortened the conclusions to make them more concise.



*Figures (all very minor, just suggestions)*

*Figure 3: -I'd suggest adding "error bars" to the total cloud fraction to give a sense for the variability in each month and region. Also, I suggest making the lines thicker and switching from dashes to all solid and colors for the drizzle (maybe light blue for light drizzle and dark blue for heavy drizzle).*

We changed the black dashed and dash-dotted lines to solid colored lines as suggested by the reviewer and added "error bars" to give an indication about the variability in terms of one standard deviation. This is noted in the figure caption.

*Figure 4: - I'd suggest moving the LTS lines to Figure 3, making them a different color (maybe orange or red) and labeling the LTS value on the right-side axes.*

We added lines for estimated inversion strength (EIS) to Fig. 4 and, thus, decided to keep the LTS line in the same figure.

*Figure 5: Maybe put the season labels on the left side instead of on top of each map, and also put the MCC type on top of each column. Also delete the longitude labels. Also, consider using a color scheme that is not diverging (i.e., white in the middle).*

We added the MCC type on top of each figure column and changed the color scheme. The white spaces refer to no data. We clarified this in the figure legend.

*Figure 7: I was surprised that the point at 21S, 81W is closed MCC. Is this an artifact of the figure being too small to see the fine details?*

The automated MCC classification is far from perfect and this is an example of misclassification. The artificial neural network classifier has a false detection rate of about 10-15% as stated in Section 4 of the manuscript. Misclassified scenes tend to have features difficult to classify for the human observer (Wood and Hartmann, 2006).

*Figure 9: Label the panels with the type, even though it is obvious. Consider combining into one panel, with three box/whisker objects for each geographical region (maybe colored differently for open/closed/disorg).*

We considered this suggestion but the plot turns out too busy then. We labeled the figure panels as suggested by the reviewer.

*Figures 9 & 11: Why are the whiskers 2-sigma instead of total range or 10/90 percentiles? Especially when considering skewed distributions, it seems like that would be more natural.*

In many applications, the whiskers extend to the lower and upper quartile plus/minus 1.5 times the inter quartile range (2.7 sigma). We have chosen to limit the range of the whiskers to two standard deviations. The total range would also include outliers, which are typically indicated as dots. We excluded these points here to avoid unnecessary clutter.

#### *Typos & Editorial Suggestions*

*Section 2: -"each cloud scence constitutes" – 'scence' should say 'scene' -"volume is greater or equal 50%." should say "volume is greater or equal to 50%."*

All done.

*Section 3: -"braod strip around the global." – 'global' should be 'globe' -"heavy drizzle is tracking the" should be "heavy drizzle tracks the" -"LTS peaks out in" should be "LTS peaks in" -The last sentence, "Details of the MCC statistics for various regions at subtropics and midlatitudes are given in Table 3." seems a bit awkward. Maybe rephrase as, "Table 3 lists the frequency and cloud cover for each region and MCC type."*

All done.

*Section 4: -Suggest deleting "the near-coastal waters"*

Done.

*Section 5.2 & conclusion #5: I suggest avoiding this use of parentheses, and simply writing out both relationships to increase ease of reading. See Eloquent Science by Schultz for a longer discussion of why not to use this convention.*

Agreed. We rephrased the sentences accordingly.

*Section 5.3: -"The little difference in cloud top" – suggest "The small difference ..."*

Done.