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Dear the editor of Atmospheric Chemistry and Physics,

Enclosed please find our revised manuscript (acp-2014-213) submitted to Atmospheric Chemistry and Physics. It has been revised according to the referees' comments. The title of this manuscript slightly changes from "... satellite observations ..." to "... satellite-retrieved data ..." .
We wish this manuscript deserves to publication.

Sincerely,

Takuro Michibata

Response to Reviewer #1 of acp-2014-213

Dear Reviewer,

Thank you very much for taking your time to review our paper.
I am returning herewith a manuscript revised according to reviewers' comments.
I hope that the manuscript is now acceptable for publication in *ACP*.

[RC]: Referee comment in *Italic*
[AC]: Author comment

General Comments

[RC] *This paper repeats several recent global scale analyses based on CloudSat and MODIS data in specific regions in the midlatitude Pacific ocean and the Eastern Asian continent. Mostly the methodology borrows from Kubar et al., 09 and Suzuki et al, 10. Findings on these regional scales tend to be similar to those found on the global scale. The methodology and analyses is generally sound while the interpretation is slightly suspect in a few areas outlined below. The paper doesn't add very much to the prior studies from which it borrows. There are interesting differences between land and ocean results which could be an area that could be much more thoroughly explored. This could be a fruitful area for the authors to explore in the future.*

I recommend that the paper requires minor revision prior to publication.

[AC] We would like to thank the referee #1 for his/her very insightful comments, which make our manuscript better. Our discussion and corrections on individual issues are below.

Minor Comments:

[RC1] *Page 5, Line 4: State that the cloudmask = 30 is from the Geoprof mask. Only very few people will know what mask = 30 means.*
[AC1] CloudSat cloudmask value greater than 30 means good/strong echo, which estimated false detection less than 4.3% (Marchand et al. 2008), therefore high confidence is guaranteed.
We have changed this sentence slightly, "a cloud mask value greater than 30, which means high-confidence detection," to "a cloud mask value greater than 30 (good/strong echo), which means high-confidence detection (estimated false detection < 4.3%; see Marchand et al., 2008, Table 1),".

[RC2] *Page 7: The results for the inland industrial areas are very non-intuitive. Land or ocean one would expect precipitation occurrence to map most strongly into LWP based on simple microphysical arguments. Whether it be land or ocean, the Another plausible explanation might be that there are errors in the retrieval algorithms. Perhaps the cloud types are different over the industrial areas. I would think that there would be more cumulus than over the ocean. MODIS retrievals for cumulus clouds are much more prone to retrieval errors than are stratocumulus because of the horizontal inhomogeneity. See the recent publications from Zhibo Zhang for example.*
[AC2] Thank you for your helpful advices and suggestion. Following short discussion of

cloud types and the MODIS retrieve error have been added in the first paragraph of section 3.1: “The results suggested that the precipitation occurrence is most strongly related to LWP, except for the Industrial area. It is noteworthy that there are large seasonal differences of more than 7 K in LTSS in the Industrial area. Therefore, there is a possibility of different cloud types over the Industrial area; i.e., more cumulative cloud in JJA (unstable lower LTSS environment) than over the oceanic area. The passive MODIS sensor tends to retrieve errors on cumulative inhomogeneous cloud (e.g., Zhang et al., 2012; Zhang and Platnick 2011; Zinner et al., 2010) because of its simplifying assumptions; i.e., clouds are plane-parallel and homogeneous, any effects of drizzle/rain drops are ignored (Zinner et al., 2010), etc. These assumptions may lead to retrieval bias of CDR; e.g., illumination (shadowing) effects can lead to overestimation (underestimation) of COT and underestimation (overestimation) of CDR (Marshak et al., 2006). The larger CDR and smaller COT are estimated with increasing cloud inhomogeneity, which results in underestimation of LWP for cloudy scenes (Painemal et al., 2013). Therefore, care should be taken with regard to this background of CDR retrieval error and underestimation of LWP, especially over the Industrial area in JJA.”.

[RC3] *Page 9, Line 6: The differences in LWP do not necessarily mean the cloud lifetime increases. Without a really good causal mechanism and a lot more analysis you shouldn't speculate about this.*
[AC3] Yes. We see your point. Kubar et al. (2009) well documented the drizzle occurrence by using LWP-N_c diagram (see their figure 12), as we mentioned in our discussion paper. Their results showed that the drizzle frequency decreases with increasing N_c under constant LWP, which partly indicates aerosols second indirect effect (see also Leon et al., 2008, figures 8 and 10). In our results as well, similar characteristics were generally observed, though using not drizzle occurrences but Z_{max} to know drizzle/precipitation intensity in our analysis. We think that our suggestions about the possibility of cloud lifetime are important. We tried to emphasize the cloud lifetime effect associated with LWP and N_c by our method for the contrast of aerosols concentration (land versus ocean), but indeed, our first try of seasonal/regional analysis to understand aerosol–cloud interaction need further work.

[RC4] *Page 9: CloudSat cannot reliably measure cloud thickness. There are two problems. First for precipitating clouds there are reflectivity values that are from precipitation, not cloud. Second for non-precipitating clouds, the reflectivity at cloud base are most likely too weak to be observed. You need to mention these things. What you are really measuring is the hydrometeor thickness subject to the minimum detectable signal of the CloudSat radar.*
[AC4] It is a very important point. Thank you for your suggestion. According to your comment, we have added a few sentences to explain the notice and information, at the end of the first paragraph of section 3.4: “However, it should be noted that the "cloud geometrical thickness" mentioned here does not always accurately represent the cloud thickness. Specifically, in some cases of non-precipitating cloud, determination of the cloud base is difficult because the reflectivity at this point is too weak to be observed. However, in the case of precipitating cloud, the detected value would include not only the cloud but also some of the precipitating layer. Thus, the “cloud geometrical thickness” represents the detected hydrometer thickness.”.

[RC5] *Page 10, Line 19: cloud growth is insensitive to LTSS. I think that you have this backwards.*
[AC5] Needless to say, the atmospheric stability, especially updraft velocity is a very important factor for cloud/drizzle/rain droplet growth. What we really wanted to express is, the CDR is a more dominant factor than LTSS, presented in figure 8.

Thank you very much for reviewing our paper.

Sincerely yours,

Takuro Michibata

References (New added references are highlighted)

Marchand, R., Mace, G. G., Ackerman, T., and Stephens, G. (2008), Hydrometeor detection using CloudSat – an Earth-orbiting 94-GHz cloud radar, J. Atmos. Ocean. Technol., 25, 519–533, doi:10.1175/2007JTECHA1006.1.

Zhang, Z., A. S. Ackerman, G. Feingold, S. Platnick, R. Pincus, and H. Xue (2012), Effects of cloud horizontal inhomogeneity and drizzle on remote sensing of cloud droplet effective radius: Case studies based on large-eddy simulations, J. Geophys. Res., 117, D19208, doi:10.1029/2012JD017655.

Zhang, Z., and S. Platnick (2011), An assessment of differences between cloud effective particle radius retrievals for marine water clouds from three MODIS spectral bands, J. Geophys. Res., 116, D20215, doi:10.1029/2011JD016216.

Zinner, T., G. Wind, S. Platnick, and a. S. Ackerman (2010), Testing remote sensing on artificial observations: impact of drizzle and 3-D cloud structure on effective radius retrievals, Atmos. Chem. Phys., 10, 9535–9549, doi:10.5194/acp-10-9535-2010.

Marshak, A., S. Platnick, T. Varnai, G. Wen, and R. F. Cahalan (2006), Impact of three-dimensional radiative effects on satellite retrievals of cloud droplet sizes, J. Geophys. Res., 111, D09207, doi:10.1029/2005JD006686.

Painemal, D., P. Minnis, and S. Sun-Mack (2013), The impact of horizontal heterogeneities, cloud fraction, and liquid water path on warm cloud effective radii from CERES-like Aqua MODIS retrievals, Atmos. Chem. Phys., 13, 9997–10,003, doi:10.5194/ acp-13-9997-2013.

Kubar, T. L., Hartmann, D. L., and Wood, R. (2009), Understanding the importance of microphysics and macrophysics for warmrain in marine low clouds, Part I: Satellite observations, J. Atmos. Sci., 66, 2953–2972, doi:10.1175/2009JAS3071.1.

Leon, D. C.,Wang, Z., and Liu, D. (2008), Climatology of drizzle in marine boundary layer clouds based on 1 year of data from CloudSat and Cloud-Aerosol Lidar and Infrared Pathfinder Satellite Observations (CALIPSO), J. Geophys. Res., 113, D00A14, doi:10.1029/2008JD009835.

Response to Reviewer #2 of acp-2014-213

Dear Reviewer,

Thank you very much for taking your time to review our paper.
I am returning herewith a manuscript revised according to reviewers' comments.
I hope that the manuscript is now acceptable for publication in *ACP*.

[RC]: Referee comment in *Italic*
[AC]: Author comment

General Comments

[RC] *The authors investigate differences in satellite-retrieved liquid cloud properties for different East Asia land and North Pacific ocean regions. The comparison is interesting. However, I am not convinced that the authors have adequately argued that these regional differences are due to aerosol effects. I recommend that the authors consider the comments below, especially the two major comments. I am hopeful that an appropriately revised manuscript would be suitable for publication in ACP.*

[AC] We would like to thank the referee #2 for his/her thoughtful reading and very positive comments.
We tried to revise our manuscript so as to answer to your comments.
Our discussion and corrections on individual issues are below.

Specific comments (major)

1. ***Data and methodology.*** *More information about data and methods would be helpful. In particular:*
[RC1] *(a) Please provide approximate horizontal and temporal resolution of the CloudSat 2B- TAU data. Are the CloudSat data analysed on a level 2 grid, or is it gridded to a regular lon-lat grid prior to analysis?*
[AC1] We have inserted the additional information with a reference for the readers, in Section 2.1 as follow: "The vertical and spatial resolutions of the CloudSat data products are approximately 480 m and 1.4 × 1.8 km (across and along tracks), respectively. However, the data are vertically 2× oversampled, and therefore ~240 m sampled data are available (Stephens et al., 2008).".
We applied the former analysis method.

[RC2] *(b) Are MODIS data also used in places (as suggested in Section 3.1, Section 4 and Fig 1)? If so, please describe the MODIS data in Section 2, and also provide the appropriate data source acknowledgement in the acknowledgements. Throughout the paper, please be careful to make it clear which data are CloudSat-derived and which are MODIS-derived - it is currently somewhat ambiguous in places, although I am assuming that most of the cloud products are CloudSat apart from Nc.*
[AC2] We have added the description of MODIS data with some references for the readers, in the end of section 2.1, as follow: "The passive sensor MODIS traverses aerosol–cloud properties at high frequency and resolution, using 36-channel spectral bands (Plantnick et al., 2003; Remer et al., 2005). The level 3 (collection 5.1) 1° × 1° gridded aerosol optical depth (AOD) at 0.55 μm from Aqua/MODIS (Parkinson, 2003),

which is a part of the A-Train constellation (Stephens et al., 2002), is used in our study.". The sub-section name was slightly changed, and an acknowledgement for the MODIS group was also added, in revised manuscript.

In addition, we changed the sentence of p10520.4, to clearly explain the data derived from CloudSat product: "where τ_c and r_e were obtained from MODIS retrieval, matched along the CloudSat footprint (i.e., CloudSat 2B-TAU product, mentioned earlier).".

[RC3] *(c) Please include further discussion of uncertainties and possible retrieval errors in the satellite-retrieved products. In particular, note that CDR (from MODIS at least) may be affected by drizzle (Zinner et al, 2010, doi:10.5194/acp-10-9535-2010). May this impact the interpretation of some of the results in Section 3?*

[AC3] Thank you for your helpful information. Zinner et al. (2010) reported that a retrieval of cloud droplet is unlikely affected by drizzle, based on large eddy simulation. Zhang et al. (2012) also showed similar results. On the other hand, CDR retrieval by MODIS tends to occur large uncertainty for horizontal inhomogeneity cloud, in particular, cumulus cloud. The discussion of satellite retrieval and its errors have been added in section 3.1: "The results suggested that the precipitation occurrence is most strongly related to LWP, except for the Industrial area. It is noteworthy that there are large seasonal differences of more than 7 K in LTSS in the Industrial area. Therefore, there is a possibility of different cloud types over the Industrial area; i.e., more cumulative cloud in JJA (unstable lower LTSS environment) than over the oceanic area. The passive MODIS sensor tends to retrieve errors on cumulative inhomogeneous cloud (e.g., Zhang et al., 2012; Zhang and Platnick 2011; Zinner et al., 2010) because of its simplifying assumptions; i.e., clouds are plane-parallel and homogeneous, any effects of drizzle/rain drops are ignored (Zinner et al., 2010), etc. These assumptions may lead to retrieval bias of CDR; e.g., illumination (shadowing) effects can lead to overestimation (underestimation) of COT and underestimation (overestimation) of CDR (Marshak et al., 2006). The larger CDR and smaller COT are estimated with increasing cloud inhomogeneity, which results in underestimation of LWP for cloudy scenes (Painemal et al., 2013). Therefore, care should be taken with regard to this background of CDR retrieval error and underestimation of LWP, especially over the Industrial area in JJA.".

[RC4] *(d) The derivation of estimated Nc would be better placed in Section 2 rather than Section 3.1. Further details would be beneficial. For example, please state the assumptions used to derive the equations (e.g. adiabaticity, constant Nc with height etc). In light of these uncertainties and satellite-retrieval errors (point c above), it would be good to discuss the validity of the Nc approximation, particularly in relation to precipitating clouds and ocean vs land.*

[AC4] We have placed the derivation of *Nc*, LWP, and LTSS (p10519-19 ~ 10520-8) to Section 2.2, according to the referee comment. In addition, we have added some additional explanation of the derivation of *Nc* with reference (Wood 2006) for the readers, and of the uncertainty of estimating *Nc*, in Section 2.2: "Γ_{eff} is the adiabatic rate of increase in the liquid water content with height, which is a function of two variables, profile of temperature and pressure, as shown in Fig. 1 of Wood (2006). The difference in CDR retrieval error between land and ocean; e.g., due to the differences in cloud type (e.g., Zhang et al., 2012), may also cause uncertainty in estimation of *Nc*. However, we apply a CDR uncertainty threshold of < 1 μm, as mentioned above, which reduces *Nc* uncertainty as much as possible. Other possible errors due to the assumption of deriving *Nc* (e.g., adiabaticity, vertical homogeneity) are documented elsewhere (e.g., Grandey and Stier 2010; Kubar et al., 2009).".

[RC5] *(e) It would be helpful to define all the symbols/acronyms used in the paper within Section 2, even if these means re-defining acronyms/symbols which have already been introduced in Section 1 (eg CDR) or defining Section 3 acronyms early (e.g LTSS). There*

are quite a few to keep track of, and many readers may visually search Section 2 if they are confused as to the meaning of an acronym/symbol at any point. (Additionally, it would be helpful to repeat these definitions in the table and figure captions.)
[AC5] Some acronyms (e.g., CDR, COT, Nc, LTSS) were re-defined in Section 2, according to the referee comment, as mentioned in AC4. In addition, the table and figure captions were modified, in revised paper.

[RC6] *(f) The sentence starting at p10519.11 explains that the choice of cloud temperature cut-off is reduced for some regions. Is it not possible that this introduces bias? I think it would be much more sensible to keep a fixed temperature cut-off, even if this means the Inland and NE China results are insignificant and/or noisy in DJF due to a lack of data.*
[AC6] According to the referee comment, we analyzed again in these two regions of DJF by same criteria to the others, and modified the statistic value for Table 1 and figures 5–7. The following notice in Introduction was deleted: "However, because few data meet these criteria in the Inland and NE China areas in DJF, we also include data for clouds with temperatures above 265K, only in DJF in these two regions.".

[RC7] *(g) The North Pacific 1/2/3 regions are referred to as 'Ocean' regions, whereas the other four regions are referred to as 'Land' regions (e.g. Table 1). Is a land-sea mask used to select ocean-only or land-only pixels within each of these regions? If so, please state this clearly in Section 2 and Fig. 1. If not, then the Japan, Northeast China and Industrial China should not be referred to as 'Land' regions due to a large fractional ocean cover. In particular, the Japan region appears to be more than 50*
[AC7] The referee comment makes sense. However, we simply just called as "Land regions" where expected air pollution by land origin (i.e., dust or anthropogenic) aerosols. The notice of usage of "Land" here have been added in the beginning of section 3.1: "The land–sea mask is not applied in our analysis, and therefore the data for the Japan, NE China, and Industrial area, including the ocean part, do not necessarily represent data only over the continent.".

[RC8] *(h) Are the results sensitive to choice of region size? Might biases be introduced by using smaller regions over land compared to over ocean?*
[AC8] We summarize the result of different region size over land areas, in Table S1 below. Same size as in oceanic area (i.e., 25° × 25°), and smaller size (10° × 5°) are shown. The introduced biases by using smaller regions over land compared to over ocean would be not large, because the result is not sensitive to the choice of region size.

Table S1. Cloud physical parameters for different size region.

	Land				
	Inland	NE China	Industrial	25°N–50°N 100°E–125°E	25°N–30°N 110°E–120°E
The number of samples	693(0)	1315(1)	3927(4540)	5935(4541)	964(1421)
τ_c	22.2(N/A)	24.5(0.73)	19.5(35.9)	20.9(35.9)	20.3(35.2)
r_e $[\mu m]$	11.9(N/A)	11.9(10.0)	12.3(10.5)	12.2(10.5)	12.0(10.3)
LWP $[gm^{-2}]$	148(N/A)	161(4)	129(205)	138(205)	132(198)
N_c $[cm^{-3}]$	154(N/A)	139(28)	125(257)	131(257)	120(263)
Maximum dBZ	-5.8(N/A)	-8.1(-27.3)	0.8(-1.1)	0.9(-1.1)	0.3(0.4)
Cloud-top height [km]	3.7(N/A)	2.7(1.4)	3.4(2.3)	3.3(2.3)	3.2(2.3)
Cloud-base height [km]	2.7(N/A)	1.6(1.2)	2.3(1.4)	2.2(1.4)	2.0(1.6)
Geometrical thickness [km]	1.0(N/A)	1.1(0.2)	1.2(0.9)	1.1(0.9)	1.2(0.7)

2. ***Interpretation of results.*** *Although the results provide an interesting analysis of liquid clouds for the different regions, I am not convinced that they necessarily provide much information about aerosol-cloud interactions. In particular, the following alternative explanations warrant further discussion:*

[RC9] *(a) May differences between land and ocean be partially explained by retrieval errors in eg Modis derived Nc? (CloudSat data may less prone to difference in retrieval errors between land vs ocean.)*

[AC9] We have added the discussion of the uncertainty of *Nc*, which derived from CloudSat products, in Section 2.2. Please see the author's reply to referee #2-comment 4 (AC4), comment 3 (AC3), and comment 17 (AC17) as well.

[RC10] *(b) May differences between the different regions be due to differing meteorology that is independent of any aerosol influence? Differing meteorogy is likely to be major issue over land. Even over the ocean, spatial gradient changes in the meteorolgy (Grandey and Stier, 2010, doi:10.5194/acp-10-11459-2010) might play an important role. (In the authors' defence, they have considered some meteorological/seasonal factors, e.g. focusing on liquid water clouds only, consideration of LTSS, splitting into DJF and JJA seasons, mention of land surface heating on p10523 and mention of updraft strength on p10526.)*

In light of (a) and (b) above, I think it is difficult to link the ocean vs land differences to aerosol effects. However, differences between the ocean areas (e.g. more polluted North Pacific 1 vs more pristine North Pacific 3) may possibly point to aerosol effects, but it is still difficult to discount aerosol-independent meteorological gradients.

Although the authors have addressed the meteorological issues to some extent, further discussion is warranted. Any possible aerosol influence should be stated much more tentatively in the Abstract, Results and Conclusions, a it might be appropriate the change the Title. Or, if the authors can satisfactarily address the points raised here, the argument and evidence for an aerosol influence should be presented more clearly.

[AC10] It is very critical and a difficult issue, especially analyzing over mid-latitude region (both land and ocean) because of the spatially large physical/meteorological variation.

Spatial gradient changes in the meteorology are one of the important roles for the seasonal and/or regional differences of cloud physics.

Grandey and Stier (2010) investigated the effect of spatial scale choice on global estimates of radiative forcing based on different regional sizes study. They discussed the possibility that large regional scale may lead significant errors, because of relatively small spatial gradient of meteorology. In our study as well, spatial gradient change in the meteorology might be one of the possibility of land/ocean contrast, in addition to the aerosols effect.

We added the following explanation for spatial difference of meteorology, as follow:

(in Section 3.4)

"The effects of spatial difference of meteorology on aerosol-cloud interaction were not considered in our study; therefore, further analyses are necessary. For example, the difference in the autoconversion rate over land and ocean, or in JJA and DJF, may provide some insight into the indirect aerosols effects (e.g., Stephens and Haynes 2007; Sorooshian et al., 2013). Although the data presented here are insufficient to link the ocean versus land differences to aerosol effects, further studies to determine the effects of atmospheric conditions (i.e., aerosol concentration, static stability) on cloud physical structure would be valuable.".

(Conclusion)

"This study does not preclude the possible effect of spatial gradient changes in the meteorology on aerosol-cloud interaction, and further analyses taking such environmental conditions into consideration are required.".

In addition, the notice of our interpretation and future work, were added in conclusion.

Please see the author's comment to referee #2-comment 20 (AC20) as well.

Specific comments (minor)

[RC11] ***Use of 'observations'.*** *Throughout the manuscript (including the title and abstract), using 'satellite-retrieved data', 'satellite retrievals' or even just 'satellite data' would be preferable to 'satellite observations'.*
[AC11] Thank you for your advice. We have re-examined the usage of "observation", throughout our manuscript. We modified the related parts, in the revised paper.

[RC12] ***Use of 'pristine'.*** *Throught the manuscript, the ocean areas are sometimes referred to as 'pristine'. Looking at the AOD values in Table 1, this is not necessarily the case, particularly in North Pacific 1.*
[AC12] We corrected/removed the related parts, in revised manuscript.

Abstract.
[RC13] *It would be helpful to mention which satellite instruments/datasets are used.*
[AC13] We added the information of derived data source for readers, in revised manuscript.

Introduction.
[RC14] *(a) References could be provided to support statements in the two adjacent sentences starting at p10517.28 and p10518.1 and the sentence starting at p10518.9 (i.e., which other studies are being referred to in each case?).*
[AC14] We slightly changed the sentence starting at *p10517.28, and* added references above later two sentences (starting at p10518.1 and p10518.9), in revised manuscript.
p10517.28- : changed the end of this sentence, "... based on satellite data, as mentioned above."
p10518.1- : added "(e.g., Kawamoto and Suzuki 2013)" to the end of this sentence.
p10518.9- : added "(e.g., Nakajima et al., 2010)".

[RC15] *(b) Brief discussion of some other papers investigating aerosol-cloud-precipitation interactions would be beneficial. For example, papers by Sorooshian et al. (2009, doi:10.1029/2009GL038993; and 2013, doi:10.1002/jgrd.50523) may of particular relevance to this paper.*
[AC15] Thank you for providing the important references for our study. Both of studies by Sorooshian et al. are very interesting and profitable to link A-Train observational data with modeling study. Following brief discussion have been added to introduction: "Sorooshian et al. (2009) performed a binning study of LWP to clarify the effects of aerosol perturbation (e.g., precipitation susceptibility, aerosol cloud interactions), and suggested that intermediate LWP (~500–1000 g m^{-2}) cloud tends to be more susceptible to aerosol than shallow cloud with low LWP. Furthermore, they expanded the study of Stephens and Haynes (2007) who introduced a method of estimating conversion (from cloud water to rain water) rates from CloudSat-CPR and MODerate-resolution Imaging Spectroradiometer (MODIS) retrieved data, and discussed the relationships between conversion rate and aerosol types, associated with the category of lower tropospheric static stability (LTSS) and LWP (Sorooshian et al., 2013). ".

Results.
[RC16] *(a) p10521 first paragraph: please clarify that the discussion refers to liquid water clouds only.*
[AC16] This comment is helpful for making our study object clearer. We added the

phrase “only liquid phase warm cloud” in revised version.

[RC17] *(b) Section 3.3: how may errors in the MODIS-derived Nc (e.g. drizzle contamination of the effective radius retrieval or lack of adiabaticity) relate to these results?*
[AC17] We have added some additional discussion, in revised paper. Please see the author’s reply to reviewer #2-comment 3, 4 (AC3, AC4) as well.

[RC18] *(c) Section 3.3: what exactly is meant by the term ‘transition’? More explanation of this term would be helpful, particularly in relation to the fact that the figures are contructed by looking at many different clouds, rather than the development of single clouds. (An alternative place to discuss this would be Section 2.)*
[AC18] This part slightly changed from “transition process” to “transition process (from cloud droplet to drizzle and precipitation)”.

[RC19] *(d) Section 3.4: A brief re-explanation of CFODD (including a redefinition of the acronymn) would aid many readers. This could be done either in the text or in a figure caption.*
[AC19] We have already defined “CFODD” in the Section 1 (p10517.24). The explanation about the method of CFODD is sufficiently provided in p10525.6–14.

Conclusions.
[RC20] *(a) The importance of complementary numerical modelling (mentioned at p10526.27) could be re-emphaisized in Section 4.*
[AC20] The short discussion of the future work and the importance of complementary numerical experiment have been added in the end of section 4: “We determined some of the characteristics of aerosol-cloud interaction based only on satellite data. However, composite studies with numerical modeling (e.g., sensitivity experiments for the influence of aerosol and atmospheric stability to cloud physics) are required to gain a detailed understanding of the aerosol–cloud interaction.”. In addition, please see also AC10, for author’s future work more specifically.

[RC21] *(b) Redefinition of the symbols and acronymns (especially CFODD) would be helpful for readers who are skimming the paper for the first time.*
[AC21] We corrected.

Table and figure captions.
[RC22] *(a) Many readers would find it helpful to have more explanation (of methodology, not interpretation) in the captions. In particular, it could be helpful to redefine any acronymns/symbols used in the table/figure, and also state the satellite source (e.g. CloudSat CDR, MODIS derived Nc).*
[AC22] We changed some of the caption of figures and table in revised manuscript, to understand easily for readers. Thank you for your advice.

[RC23] *(b) Fig 1: the missing data over the Sahara, Arabia, the Himalayas and Greenland should be a different colour (e.g. grey or white), not purple (the colour used for zero).*
[AC23] We modified fig. 1, according to your comment. Missing values are shown in white.

[RC24] *(c) Fig 1: the lines showing the regions could be added to the top figure (in addition to also being showing in the bottom figure).*
[AC24] Modified figure is uploaded in revised paper.

[RC25] *(d) Figs 3 and 8: row labels stating the seasons (e.g. JJA for first row) would make the figure clearer.*
[AC25] Modified figures are uploaded in revised paper.

Technical corrections/suggestions

[RC26] *p10518.21: please clarify whether January 2006 or December 2006 is the first month for the DJF season. Similarly, please clarify the final month.*
[AC26] December 2006 is the first month, and February 2009 is the final month in DJF term. This sentence was slightly changed as follow: "and December, January, and February (DJF) from 2006--2009 (i.e., December 2006--February 2009)".

[RC27] *p10518.19: define ECMWF.*
[AC27] Done, thanks.

[RC28] *p10518.24: 'Desert; we' to 'Desert. We'.*
[AC28] We corrected.

[RC29] *p10519.2: 'Japan' to 'the Japan region'.*
[AC29] We corrected.

[RC30] *p10519.22: define ρ_w.*
[AC30] Done, thanks.

[RC31] *p10521.2: explain what is meant by 'typical cloud properties'.*
[AC31] This sentence is changed to "COT and CDR are the typical cloud physical variables.".

[RC32] *p10524.2: consider rewriting the first sentence of Section 3.4. It may be clearer to write 'Cloud geometrical thickness, cloud top height, and LWP are cloud macrophysical variables.' (Unless you particularly want to emphasize cloud geometrical thickness.)*
[AC32] We mainly discuss cloud geometrical thickness and cloud vertical structure, in this section, therefore the sentence has no need to be changed. Thank you for your suggestion.

[RC33] *p10524.3: consider replacing 'corresponds to' by 'is offset by a constant from' or similar.*
[AC33] This sentence is changed from 'corresponds to' to 'is offset by a constant from'.

[RC34] *p10524.4: a value for cloud base height could be provided.*
[AC34] The cloud-base height was summarized in revised Table 1.

[RC35] *p10524.9: 'rather than' to 'stronger than the relationship' or similar.*
[AC35] This sentence is changed from 'rather than' to 'stronger than the relationship', according to your comment.

[RC36] *p10525.18: consider replacing 'insensitive' because this may carry causal connotations for many readers.*
[AC36] We changed 'seems insensitive to' to 'seems to have little relation with'.

[RC37] *pp10528.8: should an acknowledgment for the MODIS data be included?*
[AC37] We added the acknowledgement for the MODIS data. Please see the authors' reply to referee #2-comment 2 (AC2) as well.

Thank you very much for reviewing our paper.

Sincerely yours,

Takuro Michibata

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