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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The effects of aerosols on water cloud microphysics and macrophysics based on satellite observations over East Asia and the North Pacific" *by* T. Michibata et al.

Anonymous Referee #2

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General comments

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.

Specific comments (major)

1. **Data and methodology.** More information about data and methods would be helpful. In particular:

(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?

(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 N_c.

(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?

(d) The derivation of estimated N_c 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 N_c 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 N_c approximation, particularly in relation to precipitating clouds and ocean vs land.

(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.)

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(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.

(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

(h) Are the results sensitive to choice of region size? Might biases be introduced by using smaller regions over land compared to over ocean?

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:

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

(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

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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.

Specific comments (minor)

3. **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'.

4. **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.

5. **Abstract.** It would be helpful to mention which satellite instruments/datasets are used.

6. **Introduction.** (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?). (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.

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7. **Results.** (a) p10521 first paragraph: please clarify that the discussion refers to liquid water clouds only. (b) Section 3.3: how may errors in the MODIS-derived N_c (e.g. drizzle contamination of the effective radius retrieval or lack of adiabaticity) relate to these results? (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.) (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.

8. **Conclusions.** (a) The importance of complementary numerical modelling (mentioned at p10526.27) could be re-emphaisized in Section 4. (b) Redefinition of the symbols and acronymns (especially CFODD) would be helpful for readers who are skimming the paper for the first time.

9. **Table and figure captions.** (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 N_c). (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). (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). (d) Figs 3 and 8: row labels stating the seasons (e.g. JJA for first row) would make the figure clearer.

Technical corrections/suggestions

p10518.21: please clarify whether January 2006 or December 2006 is the first month for the DJF season. Similarly, please clarify the final month.

p10518.19: define ECMWF.

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p10518.24: ‘Desert; we’ to ‘Desert. We’.

p10519.2: ‘Japan’ to ‘the Japan region’.

p10519.22: define ρ_w.

p10521.2: explain what is meant by ‘typical cloud properties’.

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.)

p10524.3: consider replacing ‘corresponds to’ by ‘is offset by a constant from’ or similar.

p10524.4: a value for cloud base height could be provided.

p10524.9: ‘rather than’ to ‘stronger than the relationship’ or similar.

p10525.18: consider replacing ‘insensitive’ because this may carry causal connotations for many readers.

pp10528.8: should an acknowledgment for the MODIS data be included?

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 10515, 2014.