

The authors have improved the quality of their manuscript. As long as they give due consideration to the minor comments below, I recommend publication in ACP.

****Specific comments (minor)****

1. Abstract, p2.22–24: I'm still not fully convinced that the results necessarily show that ``aerosol–cloud interaction mainly occurs around the cloud base in polluted land areas during the winter season.'' I would recommend that you communicate some uncertainty by re-writing this sentence e.g. ``our results suggest that'', ``our results are consistent with'', or just include ``may'' somewhere in the sentence.

2. Introduction, p2.30–36: The first sentence refers to aerosol–cloud interactions, the second to the aerosol direct effect, and the third to aerosol–cloud interactions again. The logic of these sentences could be rearranged to make them less confusing.

3. Data and methodology. This section has been improved by the inclusion of further details. However, the structure/flow could be improved further. In particular, the discussion of derived-Nc could be moved to a section on the MODIS data.

4. Results, p5.106–p6.2. This is one possible explanation. May it not also be possible that V-shape over the Japan region may arise due to land vs ocean differences that are independent of aerosol effects? After all, the Japan region has a mixture of land and ocean.

5. Results, p6.32–35. In light of the modified discussion about the nature of the cloud geometrical thickness measurements (p6.15–24), it might be helpful acknowledge that the strong correlation between cloud geometrical thickness and Zmax may be driven by precipitation causing an increase in cloud hydrometeor thickness (i.e. the direction of causality is not clear).

6. Results, p7.22. Consider adding `or other meteorological factors' after the Nakajima et al. reference.

7. Results, p7.42–52. I appreciate that you have added this paragraph following my earlier comments. However, the paragraph contains two different ideas: 1. the fact that genuine aerosol–cloud interactions may behave differently under different meteorological conditions; and 2. the fact that meteorology may drive aerosol–cloud relationships (even in the absence of any aerosol–cloud interactions). Some readers may be confused by this. It would be worth considering how to make this distinction clearer to readers. (In light of this distinction, you may also want to consider modifying the final sentence of the conclusions at p7.92.)

8. Conclusions, p7.88. I think `suggesting aerosol–cloud interaction' (or `consistent with...') would be preferable to `reflecting aerosol–cloud interaction'. I would also suggest adding

a sentence after this, along the lines of ``However, we cannot completely exclude the possibility that other meteorological factors may be responsible for the differences between land and ocean.''

****Technical corrections/suggestions****

p2.47–p3.30: This updated paragraph is very long. Breaking it into two or more shorter paragraphs would improve readability.

p4.13: incorrect use of semi-colon.

p4.38: `suggested' to `suggest'.

p4.43 and p4.45: should `cumulative' be `cumulus'?

p4.50: using parentheses (brackets) like this is confusing. The easiest way to remedy this would be to include just one logical flow (i.e. delete the three words in brackets).

p4.90: I'm still confused by the use of `the typical'. Does this have a technical meaning? If not, then consider replacing this with `are commonly considered cloud physical variables' or similar.

p7.54–95: long paragraph that could be broken down to improve readability.

Fig. 1: In the response to reviewer 2 (AC24), you mentioned that Fig 1 had been modified as per my suggestion to indicate the regions in the top figure. However, the updated draft does not appear to incorporate this change. (To clarify, I do not expect you to make this change. I am merely pointing it out in case you accidentally submitted an outdated version of the figure. The other figure changes are looking good.)