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Interactive comment on “The effect of solar zenith angle on MODIS cloud optical and microphysical retrievals” by D. P. Grosvenor and R. Wood

Anonymous Referee #2

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The authors present a novel approach to quantify the impact of solar zenith angle (SZA) on cloud optical and microphysical retrievals from MODIS. They select a high northern hemispheric latitude region over ocean, for which in mid-summer a wide range of SZA is encountered in overlapping orbits of the polar orbiting Terra and Aqua satellites. Focussing on a small and uniform region as well as a limited time period allows separating SZA effects from real variability in cloud properties. They find considerable increases in cloud optical thickness, moderate decreases in particle size, and – as a result – strong increases in droplet number concentration for SZA exceeding about 70 degrees.

While many studies on the effect of SZA (as well as viewing angle) on cloud property retrievals can be found in the literature, these are either based on simulated data or they do not cleanly isolate SZA from other effects. This paper contains an innovative

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approach to resolve that latter problem. The study has been set up very carefully, and results are thoroughly presented. I only have some relatively minor comments on the scientific content of the paper.

My main concern is the length of the manuscript. While discussion of the existing literature is an essential part of a scientific paper, the current manuscript goes too far in this respect, making it look like a review paper rather than a scientific study. In addition, the presentation of results could be made much more concise. The lengthy discussions take away the flow and focus from the paper, making it a tough read. I suggest to shorten the manuscript significantly and give hereafter some suggestions how to do this.

* Specific comments *

Title: The paper only deals with marine liquid water clouds. This should be reflected in the title.

P304, L9-11: From Figs. 5 and 6 it appears that τ , r_e and N_d are all essentially constant up to 70 degrees rather than 65 degrees. The value of 65 degrees appears throughout the paper, and should be consistently changed. This is important, because the ‘quick reader’ will mainly remember this value, and may then distrust more data than justified from the results in this paper.

P304, L14: I would say the changes in r_e are not ‘somewhat’ but ‘an order of magnitude’ smaller than in τ .

P305, L22: It should be made clear at the beginning of the paper that the effective radius is retrieved at the cloud top.

P314-315, Sect. 2.2.3: The authors choose the large-scale (5 km) variability of cloud top temperature as a measure of inhomogeneity. This is a valid choice, but I would very much like to see also the sub-pixel (250 m) variability in reflectances included as well. This metric was used in several previous studies (e.g. Zhang and Platnick, 2011) and

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it seems more relevant for explaining artifacts in 1-km cloud property retrievals. This would be especially interesting since some of the results obtained using the CTT-based metric are counter-intuitive, i.e. mainly the decrease in tau with increasing sigma_CTT in Fig. 11.

P318, Sect. 2.3.2: More details should be provided on how the MODIS 1x1-degree dataset has been generated. First of all, what has been done differently compared to the official MODIS L3 product? I guess the fact that different overpasses are kept separate? Are there more differences? Please also briefly explain what the sub-sampling involves, and what 'joint-L2' means. State which collection has been used. Have MODIS quality flags been applied?

Following up on the previous point: I assume the authors have used MODIS collection 5.1. Recently, collection 6 has (partly) become available. A short statement in the outlook on whether results are expected to change in this collection would be welcome.

P320, L4: It seems the actual reason is not so much what is described on P320, but rather on P321 (i.e. the likely misidentification of phase at lower temperatures, or at least different phase identification between low and high SZA).

P 323, L8-10: Why would the effect of higher cloud fraction at higher viewing angle play a role at high SZA and not at low SZA?

P329, L5: I would like to see a reference for the statement that the reliability of 1.6-micron r_e retrievals is 'still a matter of debate'.

P339, L11: Could the authors clarify what they mean by this statement? Is it an encouragement to the MODIS team to alter their level-3 generation approach?

P341, L28: I don't see these values mentioned above.

P366, Fig. 6: The effective radii at low viewing zenith angles are significantly lower than those at high viewing angles for all NIR bands. This deserves mentioning in the text. It is consistent with the findings of Maddux et al. (2010, see their Fig. 2) at lower

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latitudes (and lower SZA).

P371, Fig.11: In panel (b) the uppermost point is missing.

* Suggestions for shortening (and rearranging) the manuscript *

Abstract: Please only mention the key findings here. Suggest to reduce paragraphs 3 and 4 to a few sentences.

Sections 2.2.1 and 2.2.2 are a literature review, whereas Sections 2.1, 2.2.3, and 2.3 (largely) describe the adopted methodology. I would propose to have the former two sections first (as 'literature review'), followed by the methodology sections.

Sections 2.2.1 and 2.2.2 are very long. Try to identify the main retrieval artefacts in a concise way.

Section 4 is again very long. Especially 4.1 reiterates a lot of what was written in Sections 2.2.1 and 2.2.2.

Appendix B: Suggest to remove the last two paragraphs. This concerns observations made for other clouds / over other surfaces, and are thus not directly relevant for the present study.

Appendix C: Consider to remove last paragraph (seems not relevant for the present study).

* Technical corrections *

P304, L19: Remove 'which'.

P304, L26: separate 'r_e3.7' and 'r_e'.

P304, L15, L24; P308, L2: It is not clear what these 'processes' are.

P311, L7: Add hyphen between MODIS and derived.

P311, L8: AMSRE should be AMSR-E.

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P312, L16: ‘valid, or irrelevant’: these seem to be opposite characterizations; please clarify.

P314, L13: PZ11 has not been introduced yet.

P315, L20: Suggest to write ‘datasets are’, since there are different instruments and collections.

P316, L11: Add ‘(earlier)’ after ‘later’.

P316, L25: Consider revising the phrase ‘than are necessary’. Who determines ‘what is necessary’?

P320, L5: Should ‘also’ be replaced by ‘already’?

P320, L16: Do you mean ‘will’ instead of ‘can’?

P323, L15: Tables should be introduced in order of appearance (i.e. Tables 1 and 2). Same holds for Figs. 6 and 7.

P325, L22: Replace ‘because’ by ‘i.e.’ (it is not a reason).

P326, L17: cloud top ‘temperature’ would be more precise than ‘height’.

P356, Table 1 as well as Tables 2 and 3. These are hardly readable. Make sure that font size is considerably increased in the finally published manuscript.

P363, Fig.3: Add that this is for the selected region in Fig. 2.

P365, Fig. 5: Add that errors as discussed in the text are indicated by horizontal bars, and that for most points the errors are so small that they cannot be seen (at least that’s what I assume).

P365, Fig. 5: Suggest to call ‘sensor zenith angle’ ‘viewing zenith angle’ throughout, and abbreviate it with VZA. Also the lower limit of viewing angle can be omitted (i.e. write $VZA < 41.4$).

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P367, Fig.7: In the caption change '>' to '<', or better: write 'theta < 41.4'. In the legend change 85 to 81 degrees (the upper limit of MODIS optical property retrievals).

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 303, 2014.

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