Response to the Reviewer's Comments

March 15, 2022

We would like to thank the reviewer for the effort in helping us improve the manuscript. Below we respond point-by-point to the comments, with the reviewer comments in black, and our responses in blue. The manuscript has been revised accordingly. The line numbers in the response are for the revised version with tracked changes.

Comments by Reviewer #1

This study investigates the changes of Twomey effect for marine warm clouds with various influential factors including the updraft, precipitation, retrieval errors, and vertial co-location between aerosol and clouds. Valuable results have been obtained, which can improve our understanding of the radiative impacts from aerosol-cloud interaction from perspective of satellite observations. Also, the paper is well written. I personally think this manuscript is suitable for publication after a minor revision.

We thank the reviewer for this thoughtful and constructive statement. We have revised the manuscript carefully according to the reviewer's comments. Please see the following detailed point-by-point responses.

Detail comments

1. Line 19-21, In addition to the radiative impacts of aerosols by serving as CCN, aerosols can also affect the development of clouds and then precipitation and radiation by modifying the thermal structure of atmosphere via direct radiative effect.

   Thanks for the comment. This effect is now included in the introduction on lines 23-24.

2. Line 24-25, regarding the rapid adjustments, one reference is suggested here which showed the increase of cloud liquid water path and decrease of cloud re with incrased Nd via Twomey effect, Zhao and Garrett (2015, doi: 10.1002/2014GL062015).

   Thanks for bringing this study to our attention. We now cite this paper in the revised text (line 29).

3. Line 28-29, Actually even with the same climate model simulation (such as CAM5), the aerosol first indirect effect also varies with the aerosol variables that are used to present the aerosol amount.

   We agree with the reviewer that the choice of aerosol proxy can play a critical role. This point was discussed in the original manuscript (now lines 41-42).

4. Line 33-41, There are various influential factors, which are not limited to these five points. For example, the existence of precipitation particles within clouds as indicated by Yang et al. (2021, doi: 10.1029/2021JD035609) based on satellite observations, the aerosol amount or availability of water vapor amount as indicated by Qiu et al. (2017, doi: 10.1016/j.atmosenv.2017.06.002), cloud types or vertical locations as indicated by Zhao et al. (2019, doi: 10.3390/atmos10010019), and potential large uncertainties in cloud retrievals as indicated by Zhao et al. (2012, doi: 10.1029/2011JD016792), and so on.

   Thanks for the comment. These influential factors have been discussed and the relevant references have been cited in the introduction.

5. Line 54-56, Good idea. However ,with this assumption or method, we may limit the cloud types as cumuliform clouds.

   We thank the reviewer for this very important comment. To ensure the applicability of CBH as a updraft proxy. we now restrict the analyses related to CBH to convective clouds only by adopting the threshold of LTS<16 K (Rosenfeld et al., 2019). A similar dependence, i.e., increasing S with CBH, is also seen even after constraining the cloud type (Figure 2a in the revised manuscript). It is worth mentioning that, beyond the CBH, the CGT was used as an alternative proxy for the updraft regardless the cloud types, since it has been observed to be associated with the cloud-base updraft for shallow cumuliform clouds(Lareau et al., 2018) and also correlated with cloud-base updraft for stratiform clouds via modulating cloud top cooling (Zheng et al., 2016). Therefore, the conclusion of updraft-dependency holds for all cloud types. We now clarify these in the method section on lines 160-166, and the Figure 2a is revised accordingly.

6. Line 65-66, The reference mentioned above (Yang et al. 2021) also took use of the simple threshold value method with 14 um.

   Thank you. The paper is now cited in the revised manuscript.

7. Line 69, Do the authors mean "Solving this problem is helpful to ..."?

   Thanks for the comment. We have rephrased the sentence as suggested by the reviewer (line 85).

8. Line 77-81, Actually, the existence of aerosols could also cause biases to satellite-based cloud retrievals. As indicated by Li et al. (2014, doi: 10.1002/2013JD021053), the existence of absorbing aerosols could cause the satellite based retrieval of optical depth lower, effective radius higher, and so on.

   Thanks for the comment. We now cite this paper and discuss the retrieval uncertainty on lines 94-95.

9. Line 107-110, previous studies have already indicated that the aerosol-cloud interaction is sensitive to the spatial resolution. How do the authors consider this point?

   Thanks for this comment. The reviewer is right that the varying spatial resolution can also play a role. We did not explicitly consider this issue since this is not the focus of this study, but a discussion on this issue is now added on lines 130-133.

10. Line 110-111, It is well known that the retrieval uncertainties are large over polar regions, how about that over land regions? A reference might be helpful.

    Thanks for the suggestion. We do this in the revised manuscript (line 134).

11. Line 112-125, why are the Level 3 aerosol data but Level 2 cloud data used in this study?

    This is because the use of L2 cloud data can avoid the aggregation bias of N_d calculation (Feingold et al., 2021), and also enable us to select more reliable cloud retrievals on the level of the satellite pixel (1x1 km2) with different flags as detained on lines 154-159. These reliable cloud retrievals are then aggregated to larger scales to match Level 3 aerosol retrievals.

12. Line 123, it might be better used as Feingold et al. (2021)

    Thanks for the reminder. Corrected on line 153.

13. Line 130-136, what are the potential limiations or uncertianties in the cloud base height retrievals by the introduced method? It is worthy to briefly describe.

    The best performance of this algorithm is achieved for clouds with CBH around 1 km and CGT below 1 km. For such heights, which are characteristic for oceanic clouds considered in this analysis, the root mean square error ranges between 300–350 m. It is important to note that the MISR cloud-base height retrieval is limited to CBH $>$ 560 m (Böhm et al., 2019). At this lower end of the detection range, a slight underestimation of the CBH is expected (Böhm et al., 2019). This is now clarified on lines 170-174.

14. Line 136-141, even within non-precipitating clouds, drizzle could exist and affect the aerosol-cloud interaction, as indicated by the reference mentioned earlier Yang et al. (2021), how could the authors consider this impact?

    Thanks for the question. As the drizzle affects the aerosol-cloud interactions in a same manner with rainfall, i.e., as a strong sink of N_d, we considered both 'liquid precipitation' and 'possible drizzle' as precipitating clouds (see line 179). The important result of Yang et al. (2021) is now mentioned on lines 180-181.

15. Line 180-181, One possibility is the large volume of datasets. Could the data selection also play a role to the higher correlation?

    The reviewer has a great point. It might somehow play a role. However, given the similar cloud type, i.e., ice-free and single layered clouds, selected in both studies, we can not conclude the data selection is the reason for the higher correlation.

16. Line 200-201, it is easy to understand that the low AI zone is more likely aerosol-limited. However, I cannot understand why the high AI zone is close to updraft-limited regime if we do not know how large the updraft is? Could the authors expalin more?

Thanks for the question. As we have limited the proxy of updraft to a certain range (< the 10th/25th percentile or > the 75th/90th percentile) in Fig.3 and Fig. S2, it is thus assumed that the ratio of updraft to AI is dominated by AI. A statement on this is now added on line 248

17. Line 226, I would suggest using the same format, either with or without parathesis for ln AI.

    Thanks for the reminder. We now remove the parentheses of ln (AI), ln (N_{d}), ln(SO4C/B/S) throughout the text.

18. Line 233-237, if possible, I personally would like to suggest seperating this long sentence to a few short sentences.

    Thank you for the suggestion. This sentence is now split into three short ones (lines 288-290).

19. Line 270, 'appears to'-> 'appear to'

    Thank you. Corrected.

20. Line 317, why do the authors choose to use daily time series values instead of hourly?

    Because aerosol and cloud properties are linked on a daily basis throughout the manuscript, we thus choose daily time series to derive the temporal CV so as to quantify the day-to-day variability of SO4.

References

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Lareau, N. P., Zhang, Y., and Klein, S. A.: Observed Boundary Layer Controls on Shallow Cumulus at the ARM Southern Great Plains Site, J. Atmos. Sci., 75, 2235–2255, https://doi.org/10.1175/JAS-D-17-0244.1, 2018.

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