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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Quantifying Cloud Adjustments and the Radiative Forcing due to Aerosol-Cloud Interactions in Satellite Observations of Warm Marine Clouds" *by* Alyson Douglas and Tristan L'Ecuyer

Anonymous Referee #2

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Review of: Quantifying Cloud Adjustments and the Radiative Forcing due to Aerosol-Cloud Interactions in Satellite Observations of Warm Marine Clouds By Douglas and L'Ecuyer

This study uses satellite observations with the addition of model aerosol data and reanalysis meteorological data to calculate the effective radiative forcing due to cloud-aerosol interaction in warm clouds over the oceans. The authors decompose the forcing to two components: due to the Twomey effect (RFaci), and due to cloud adjustments (CA), which in this case include only changes in cloud cover (without including changes

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in LWP). The analysis is conducted also regionally and as a function of LWP, inversion strength and RH in the free troposphere. The binning according to the last two criteria is done to account for the meteorological dependency. The calculation is also done separately for precipitating and non-precipitating clouds. I think that this paper presents some interesting results that worth being published. However, I think that the paper includes some limitations that are not all fully acknowledged in the manuscript. Hence, including a more comprehensive discussion about these limitations and maybe weakening the conclusions accordingly will improve the paper.

General comments

1) If I understand correctly, calculating the radiative forcing based on the multiplication of the susceptibility calculated in present day with the total change in AI between present day and preindustrial assume linearity of the susceptibility with time. As you show that the susceptibility is a function of the environmental conditions and it is known that the environmental conditions changed, it is not clear how valid is this assumption. In addition, I think that your calculation assumes that the frequency of occurrence of each bin of EIS, RH and LWP remain the same between PD and PI (as you do not account for changes in the frequency of occurrence -eq. 9). I can't see any reasons for that to be true. Hence, and because of the large uncertainty in PI aerosol conditions, it might be better to stay only with the susceptibility calculations and not present the forcing calculations. I leave it to the authors to decide. 2) Feedbacks between clouds and the environmental conditions are not discussed and accounted for sufficiently. It is known that the environmental conditions may change differently under different aerosol conditions. In particular, the EIS and RH (maybe not at 700mb but definitely below that) may be affected by the clouds feedback on the environmental conditions differently under different aerosol conditions. In addition, direct aerosol-radiation interaction may influence the environmental conditions. Hence, the binning according to the meteorological conditions may not be independent of the aerosol conditions. I suggest to add a discussion about that. In addition, the separation to precipitating/non-precipitating

conditions could be, under certain conditions, due to aerosol effect (total rain suppression could be found in shallow clouds under polluted conditions). This effect is not discussed and you treat it as if it was external. 3) Co-variability between aerosol and cloudiness and the uncertainty in the causality relationships are not discussed enough. I appreciate that binning the data according to EIS and RH at 700 mb may reduce the role of co-variability between aerosol and cloudiness. However, some co-variability may still remain. For example, it was previously shown (Nishant and Sherwood, 2017) that under some conditions, near surface wind speed have a positive correlation with both aerosol concentration and cloudiness (CF in this case). It is possible, and even expected, that wind speed will be partially corelative with EIS and RH but not sure to what extent. I suggest to add a dissection about those limitations. 4) Uncertainties due to the semi-direct effect are not mentioned. Form satellite observations it is impossible to distinguish between the aerosol microphysical effect and the semi-direct effect but the latter is very likely to affect your calculations. I suggest to include a discussion about that. 5) Referring to the forcing only from warm marine cloud as ERFaci and RFaci might be confusing with the total estimations for all cloud. I appreciate that you mention the focus on warm clouds over the ocean directly in the tile and in many other places but I still think that the use of general terms here could be confusing. 6) At many places along the manuscript you mention "buffering" as if it was an artefact that one should avoid in his analysis (i.e. "While LWP being held approximately constant accounts for some variability in the meteorology, explicitly holding the stability and free atmospheric contributions fixed within regimes of EIS and RH700 will further control buffering and modulation of λ by the environment."). I think that if indeed clouds under different aerosol levels change differently the environmental conditions to reduce the total aerosol effect, that is something important to understand. In addition, I think you don't properly define what you mean by "buffering". That term could be used to describe many mechanisms.

Specific comments:

L3: CA is not defined hear. Consider writing in full cloud adjustments.

L10: if RFaci and CA counteract and the total effect is small I would say that it could be attributed to damped susceptibility (or buffering). Why is it “erroneously”?

L21: what do you mean by “cloud forcing”? is it the cloud radiative effect? I think it is better not to use forcing here and stick with the common definition of radiative forcing.

L68-72: consider adding here that the sign of the effect was also shown to be a function of the background aerosol concentration.

L82: the non-monotonic response was shown for other cloud properties (such as cloud fraction and top height) as well as for precipitation. Hence, I don’t understand why is it important to separate specifically this effect from the rest.

L105, L109, L141 and other places: again, maybe better to use radiative effect here instead of forcing.

L119: SPRINTARS was run (in the paper you are citing) in a T21 resolution (∼5.6o) and hence is not “cloud resolving” at all.

L250-258: I couldn’t really understand how the uncertainty was calculated. I think more details are needed for it to be reproducible. What is the magnitude of error added to PI and PD AI estimations? How did you choose this magnitude?

L265: you are comparing here the estimated forcing for only warm cloud over the ocean with the total estimation from the IPCC report. I don’t think this competition is valid. In addition, in the introduction you cited a few papers showing that most of the ERFaci is coming from warm clouds over the ocean. How that can go together with the relatively low estimations you are getting for warm cloud compared to the total forcing?

L310: you cite here a paper focusing on deep convective clouds. Consider adding papers discussing warm cloud invigoration.

L 312: I don’t understand the claim here. Why determining the casualty of aerosol

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effect on LWP is more difficult than for CF?

L320: why is that a sign of “buffering”? it just means that the aerosol effect is non-monotonic and change sign. The aerosol level at which the sign flip is a function of the environmental condition as was shown before.

L417: the possible change in precipitation could also be relevant between PI and PD making point 1 (general comments above) even more critical.

L426-430: you don’t mention here, at the beginning of the conclusion section, that these estimations are only relevant for warm cloud over the oceans. It could look like you are giving general estimations here.

L442: I think that this could also be due to the semi-direct effect of absorbing aerosols.

L445: again, if RFaci and CA counteract and the total effect is small I would say that it could be refer to as “buffering”.

Technical comments L102 and L107: ECWMF -> ECMWF? Anyway, should be written in full (and maybe also add a citation).

L401: “on the both the”

Reference Nishant, N., and Sherwood, S. C.: A cloudâĂŘresolving model study of aerosolâĂŘcloud correlation in a pristine maritime environment, Geophysical Research Letters, 44, 5774-5781, 2017.

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