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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 #1

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The authors utilize remote sensing observations and a regime-based approach to isolate the effects of varying aerosol index on cloud microphysical (1st indirect effect) and cloud macrophysical properties (adjustments). The authors utilize regimes of above-cloud RH and stability. LWP is binned to account for variations in cloud state in each regime. The results show that in some regions adjustments and the first indirect effect have opposing signs. The authors also show that as LWP increases the radiative response to AI saturates. The analysis presented here satisfies the important problem of separating variability due to meteorology from aerosol-cloud interactions (aci). The

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authors find a relatively weak ERFaci from warm-topped clouds over oceans, which appears to be due to dimming in regions in the equatorial Atlantic and Indian ocean.

While I appreciate that the authors are applying the methodology developed in a previous study, it is hard to understand what is being done and I think the authors could briefly summarize their methodology to allow readers to more efficiently refer to DL19. The description of the observational data sets could be much more substantial. It is confusing what observational and modelling data is being used for what. In some cases it appears that observational data sets that are not appropriate are being used, but it is hard to confirm this from the data section. One solution that might make this un-ambiguous would be to create a table of variables and data sources.

A critical issue with this paper is use of area-mean LWP (in-cloud LWP*CF) from microwave when the authors imply they are using in-cloud LWP based on wording in the paper (ln 153). From reading the discussion in DL19 I believe that scene-mean LWP from AMSR is just used to filter data into rough bins, and does not play a role in the analysis beyond this. While this is probably not a big problem, the authors may want to clarify what the footprints of the different data sets are that they are using, possibly with a diagram overlaid over an actual satellite image to allow readers who are less familiar with remote sensing to contextualize what is being shown, especially because the authors are using active instruments averaged along track with passive instruments. In particular, in this regard I am confused how the authors are overlapping along-track averaged CF from Cloudsat-CALIPSO with AMSR LWP and a diagram might be helpful. A nice image of the actual cloud field from MODIS on the background would be helpful to readers trying to contextualize the retrievals in terms of cloud features.

The authors ultimately present a correlative study to predict ERFaci (or at least ERFaci for warm-topped clouds over oceans- see comments below). Characterizing covariance is important but does not guarantee an accurate prediction. In the case of aerosol-cloud adjustments in particular, there is not a unique causality flowing from aerosol to cloud (Wood et al., 2012; Gryspeerdt et al., 2019). In this context, and because their ERFaci is rather weak compared to other studies it seems possible that their analysis conflates aci with precipitation scavenging and other confounders (Gryspeerdt et al., 2019), which would tend reduce correlation strength between aerosol and cloud amount (eg precipitation scavenging is strongest when there is a lot of cloud and there tend to be less cloud and more aerosol off the coast of continents). The authors need to either apply their analysis in a GCM simulating PI and PD (Gryspeerdt et al., 2017; Gryspeerdt et al., 2016; McCoy et al., 2019; Costa-Surós et al., 2019) to make sure that their analysis methodology has predictive power, or examine the response of cloud to some sort of transient change in aerosol (Malavelle et al., 2017; Toll et al., 2019) and make sure that their analysis trained over different data can predict the response to the transient change in AI. Without these falsification tests of their predictions, it is unclear what predictive use their correlation model has in that there is no way to falsify their predictions. Even an approximate calculation using model LWP, CF, SW, and AI without any complex output along the satellite overpass (which doesn't appear to be a major source of error compared to problems from low aerosol amount as shown in Ma et al. (2018)) would provide a much more powerful validation of what the authors are hypothesizing is the ERFaci.

The authors need to refer to their ERFaci as ERFaci_liquid-topped_over_oceans (or at least that is my take from the methodology and Eq 9). Is it possible to use this metric regarding warm-topped maritime clouds, and what they know about the relative occurrence of the clouds that this analysis is performed on, to allow them to extrapolate to global ERFaci? A similar strategy is employed in Costa-Surós et al. (2019) to related forcing over Germany to global forcing.

Specific changes:

Pg 1 ln 3: ERFaci is a combination of microphysical (RFaci) and macrophysical changes (adjustments) and the latter could be further split into changes in extent and thickness(Gryspeerdt et al., 2019; Gryspeerdt et al., 2017; Gryspeerdt et al., 2016). As written this implies that thickness stays constant and the only possible adjustment is CF. I understand now that this is more like the intrinsic extrinsic separation in other studies (Christensen et al., 2017), but this would be better to clarify in the abstract.

Pg. 2 ln 40: The goals of DL19 overlap a lot with the goals of the present study. A sentence like 'The present study expand on DL19 in the following ways:' would be helpful. I believe the primary difference between these studies is the inclusion of adjustments, but it would be helpful to state that explicitly for readers to rapidly ingest what is happening.

Pg. 3 ln 85: It would help readers to quickly process what data sets are being used to describe what variable to use subheaders here (2.1 Data, 2.1.1 Warm cloud fraction). This is stylistic, but I found it hard to understand where precipitation measurements were coming from. I think that it would help a lot to have a table of what the precise data sets used are, especially since some of the remote sensing data sets being used may be inappropriate, but it is unclear if they are actually used (eg AMSR rain rates, although I believe these are not used despite being mentioned).

Pg4 ln 124: is the material not shown in the citation? If it's in the citation no need to put not shown here.

Pg 4 ln 125: Swelling is a key issue in trying to understand adjustments. I believe that swelling is not an issue for SPRINTARS because the model can be internally consistent, but an additional comment is needed about MACC aerosol swelling. It's unclear that MACC can fully correct for swelling given the very complex way that swelling occurs (Christensen et al., 2017; Twohy et al., 2009). This needs to be explained and caveated. Also, why mix MACC aerosol and MERRA2 meteorology? MERRA2 produces a very similar aerosol reanalysis to MACC and this would avoid confusing MERRA2 meteorology with aerosol reanalysis in a different framework. Also- how are SPRINTARS and MACC not sensitive to precipitation scavenging? Presumably both data sets have a precipitation sink of aerosol otherwise it would be very hard to maintain realistic aerosol.

Pg5 ln126: The MACC AI is effectively satellite AI because it is nudged to satellite radiances. At some point in this paper it is necessary to caveat the use of AI with the results of Ma et al. (2018), which found that satellite inability to detect low aerosol loading biases inferred aci.

Pg5 ln 129: It would be good to note that microwave LWP is area*in cloud LWP. In this context it is a little confusing relating this to Twomey on line 154 because that is for in-cloud LWP, not area mean LWP.

Pg.5 Ln140: This methods section is really short. I understand that the authors refer to DL19, but I think it would help readers evaluate this paper more quickly if a paragraph or so was taken to summarize DL19.

Pg. 5 ln 147: The authors refer to partitioning into precipitating and non-precipitating clouds. I am not clear how this is done. On line 245 it looks like 2C-RAIN-PROFILE is being used- this needs to be caveated that it will only see relatively heavy precipitation, but will miss light rain events. Other parts of the methodology makes it sound like AMSR-E precipitation is being used, which is problematic due to the AMSR-E precipitation just being a partitioning of condensed liquid by SST.

Pg. 6 Eq3-6: how do the authors account for CF being bounded between 0-1 in this calculation?

Pg. 7 Ln 197: The authors assert that they have accounted for the effects of precipitation on the aerosol-cloud-precipitation system. This is not supported by any evidence or citations, but is an important justification of the validity of the analysis presented here.

Pg. 8 ln 221: The authors assert that by binning LWP they reduce the chances of buffering. One thing that should be mentioned in this study is that AI and LWP will naturally anti-correlate due to precipitation and scavenging correlating with cloudiness (eg LWP or CF) (Wood et al., 2012) and due to air mass history leading to both drier and more aerosol-laden air (Gryspeerdt et al., 2019). These non-causal relationships are not meaningful to ERFaci, but can substantially affect the covariability of cloud macrophysical properties and aerosol, and thus the inferred aci strength (McCoy et al., 2019). It is possible that the LWP binning and precipitation stratification reduce this effect. However, the authors must show some demonstration of the predictive ability of this method by either (1) applying it to GCM data (in this case SPRINTARS) and showing that their methodology when applied in a GCM can accurately reproduce the GCM response to enhance aerosol as in Gryspeerdt et al. (2016) or McCoy et al. (2019) – or – (2) examining one of the transient aerosol emissions identified in recent studies (Malavelle et al., 2017; Toll et al., 2019) and see if their characterization of sensitivity of cloud to aerosol has some predictive ability. Without this sort of test there is no guarantee that the inferred ERFaci_warm-topped_oceanic is accurate.

Pg 9 Ln 261: The authors find an ERFaci that at the very weakest end of what would be expected based on existing best-estimates (Bellouin et al., 2019). This is of course completely fine, but it would be interesting for the authors to add some discussion of why their forcing is so relatively weak compared to other empirical estimates of ERFaci from observations. The authors do cite the AR5 range, but this is for the range of GCM estimates, which may not be as appropriate to consider their results relative to as observational constraint studies. I suspect that this is partially because the authors are not really looking at ERFaci, but ERFaci_warm-topped_oceanic. As such I recommend the authors do not use the terminology ERFaci. In the interest of relating this result to forcing the authors could consider using this methodology applied to GCMs (as noted above I view this as a necessary condition to this analysis) and then using the GCMs to extrapolate this result to ERFaci as in Costa-Surós et al. (2019).

Pg 10 ln 300: An alternative explanation of the weakening precipitation effect in clouds with higher LWP may be that precipitation increases with LWP, which means that precipitation scavenging becomes larger, which in turn means that the true adjustment strength is obscured by non-causal covariance between aerosol and cloud macrophysics (see discussion in McCoy et al. (2019)).

Figure 7 and ln 456: The authors find a large ERFaci in the SH, which is really surprising given the very small change in anthropogenic aerosol in these regions. Figure 1 shows change in AI, but it is a bit hard to distinguish small changes from zero and the authors may want to consider some sort of log normalization to their color scale. However, strong ERFaci exists along a line around 40°S, which is hard to square with studies examining pristine days in the PD (Hamilton et al., 2014). That is to say, the pattern of ERFaci in this study is dramatically different than the RFari shown in, for example, aerocom (Myhre et al., 2013).

Figure 7: While I think it's good to pursue analysis to its conclusion by applying it to all data, I am surprised at the positive RFaci and CA in the tropics. Can the authors comment on whether their analysis is sensitive to retrieval errors in convective cloud? In particular, a positive forcing due to RFaci is quite unusual- while it may be due to biomass burning aerosol above cloud in some regions via semi-direct effects or blocking reflective light (so not really aci) (Bellouin et al., 2019), the appearance of a positive RFaci seems to be more related to SST, than aerosol type given its appearance over the tropics, and far away from strong aerosol sources.

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