The paper investigates the effect of aerosols on cloud radiative effect while taking into account the covarying influence of meteorological factors. The sensitivity of the cloud radiative effect to aerosols is derived by sorting the data by LWP, stability and entrainment. The data is retrieved from satellite observations and reanalysis, with AI serves as a proxy for the aerosols load in the atmosphere. The results show that the global aerosol indirect effect is over estimated when not accounting for the covariability. This is probably due to buffering of the clouds response by meteorology.

We thank the reviewer for taking the time to read and comment our paper. We will go through now and address each comment below.

The authors use the term "inverse Twomey effect" which sounds physically strange. I think that the darkening of the clouds, which refers here as "inverse Twomey effect", is the response of the LWP. The LWP decreases when cloud droplets are smaller due to evaporation (entrainment), resulting in less bright clouds. This explanation is also given in the literature that the authors cite. In addition, the "inverse Twomey effect" gets much attention in the paper, perhaps more than it should. It seems to be a rather minor effect as it occupies only a small fraction of the overall samples, as shown in most of the figures.

We originally chose the term "inverse Twomey effect" as the clouds darkening go against the common assumption of the first indirect effect, however you are correct and this may have been an poor choice of words. The microphysical pathway to the darkening is not the same as the Twomey effect. We have revised our study to show that there is a general darkening effect, but the source of the darkening, whether it be a reduced cloud fraction or reduced albedo, remains unknown.

To avoid any confusion over the Twomey effect and what we were calling the "inverse Twomey effect," we changed all references of "inverse Twomey effect" to darkening or warming.

The authors write: "Constraining aerosol-cloud interactions using the local meteorology and cloud liquid water". It sounds like LWP is not part of the meteorology. However, meteorology determines boundary layer depth, and therefore also the cloud depth and LWP. Furthermore, moisture, which also controlled by meteorology in part, can alter cloud base height, and thus LWP. The authors should make it clear what they mean by meteorology.

While boundary layer depth determines the maximum cloud depth, there are variations in the LWP of warm boundary layer clouds. Decoupling, cloud breakup, and precipitation can alter the LWP of the cloud independent of the boundary layer height. We therefore wanted to account for these processes separately from the influences of the meteorology like stability and entrainment of free atmospheric air.

We agree there should be more clarity on the difference between liquid water path and local meteorology. We have added "While the stability and entrainment directly affect the LWP, we consider the LWP separately from the local meteorology as it represents the cloud thermodynamics more than the local environmental conditions." in section 2.4.2 Cloud States page 7, line 9 to address the connections. We believe it is very *common to use the term local meteorology or meteorology and not imply liquid water path.*

The terminology used along the manuscript is inconsistent. For example, the authors use the term stability for both low level stability and inversion strength (though are similar). The same with entrainment and RH, cloud regimes and cloud states/morphologies. This is confusing.

We agree that the terminology should be explained and remained more consistent. We have clarified what some statements may mean in the methodology and have stuck with a consistent terminology for each type of regime.

We have added "Here, EIS is calculated using MERRA-2 temperature and relative humidity profiles and indicates the stability of the boundary layer." To section 2.4.1 page 6, line 23.

We have added to the section 2.4.2 page 7, line 1 "Although there are other definitions of cloud regimes and cloud states used in other studies (e.g. Oreopoulos et al. (2017)), throughout ours cloud state or cloud morphology refers to the set of observations binned by liquid water path." to inform the reader of the wording we have chosen for the study.

We have added to section 2.4.1 page 6, line 29 "When referring to the effects of entrainment, it means the effects of RH." to inform the reader in the methods that the relative humidity reflects the effects of entrainment on the cloud.

Instead of using aerosols indirect effect and CRE, the authors are encourage to use the IPCC new terminology that more clearly distinguishes the key mechanisms by which anthropogenic aerosols alter the energy balance of the earth (e.g., https://doi.org/10.1175/AMSMONOGRAPHS-D-15-0033.1).

In our study, we are only finding the sensitivity of the clouds, not the ERFaci. We chose to focus on the methodology of distinguishing the signal of the warm cloud CRE to aerosol from other factors in this study, not to determine the radiative forcing of aerosol-cloud interactions. Our terminology is consistent with others in the field and we chose not to use IPCC terminology because we are not quantifying a forcing, only a sensitivity.

The captions are short and do not provide sufficient information to understand the Figures without digging into the text. Also, the captions sometimes do not present all the subplots in some of the Figures.

We agree our captions were too brief. We have added more detail to the captions to explain every part of the plot(s) shown.

"Local Meteorology" seems to be a key factor in the study (the authors chose to have it in the title). I think that this point is not enough explained in the introduction and should be emphasized more in the conclusions.

We agree that local meteorology should be focused on more in the introduction and have added "Constraining the local meteorology, or the characteristics of the environment around the cloud, as well as cloud type can significantly alter the *magnitude of the AIE compared to single, unconstrained global linear regression (Gryspeerdt et al., 2014)." to page 2, line 16.*

Specific Comments

P1 L24 Provide a reference.
We have added (Albrecht, 1989) as a reference for that statement.

P2 L1-2. This is a 1.5 line paragraph. Perhaps you can discuss here the relative contribution of the cloud life time effect and cloud albedo effect.
These two lines are part of the first paragraph of the introduction. The ACP Discussion formatting makes it seem like it is a separate paragraph.

P3 L17. I'm not sure a paper from 2014 can be considered "recent"
We have removed recent from that sentence.

P3 26. Decoupling between cloud and ocean? Provide references here and in the following sentences to establish the relationship between RH and decoupling.
We have added "...by increasing the temperature and humidity gradients at the cloud top (Lellewen 2002)." to explain how RH affects the decoupling process.

P3 L33. I would change effective radius to droplet size, and LWP to optical thickness.
We have changed the sentence to "In his original work, Twomey postulated that cloud albedo ought to increase with aerosol provided LWP is held fixed, after 10 all, albedo is dependent on the optical depth and effective radius." replacing LWP with optical depth.

P4 L1. AMF?
We have expanded this acronym to "Amospheric Radiation Measurement Mobile Facility."

P4 section 2.2. Please provide the spatial resolution of the data. Is the data from the different instruments is co-located to a single resolution?
We have added to section 2.2 Cloud "All data is interpolated down to CloudSat's ~1km footprint." Further, in section 2.1 Data we state "The A-Train is a series of synchronized satellites which allow for collocated observations from a variety of instruments (L'Ecuyer and Jiang, 2011)."

Why did you decide the upper threshold of LWP to be 400? Did you use optical thickness threshold to avoid additional uncertainties (see e.g. https://doi.org/10.1002/qj.2405).
We chose a limit of 400 because it removes outlier cases of convective warm clouds and other thicker clouds that are not the focus of this study. Less than 5% of warm

clouds in our dataset had an LWP above 400. Additionally, having 400 as an upper limit reduces the impacts of warm rain on aerosol-cloud-radiation interactions.

If I understand correctly, the CF is determined based on a 12 km segment (P4 L29). A single open cell for example can cover 12km, which would give 100% CF, while the clear area in between cells would give 0% CF. Scaling is very important in determining the CF. You also exclude clouds with LWP

We have explained how we quantify cloud fraction further in section 2.2 Cloud page 5, line 9:

"An along-satellite track cloud fraction is determined by finding the average number of warm cloud pixels that satisfy these criteria (seen by CloudSat or CALIPSO, below the CloudSat determined freezing level, and LWP between .02 and .4 kg) over each 12 km segment of the CloudSat track on a pixel by pixel basis, a scale that represents both the local scale length of the boundary layer and field-of-view used to define cloud radiative effects from Clouds and the Earth's Radiant Energy System (CERES) (Oke, 2002)."

Our cloud fraction is pixel by pixel, meaning that as cloudiness changes at a 1km scale, the cloud fraction increases or decrease by 1/12th.

Make it clear in the second eq. that the F_all sky is only for the SW.

We have changed our statement to "It is easy to show that for the shortwave radiances:" *before F_all sky equation on page 5.*

P5. The Equations have no numbering.

The equation numbering appears on the far right of the page. There is no way to change the formatting of this as it is set by the ACP Discussion Paper template.

P5 section 2.3. You use AI as a proxy for aerosols. However, AI is retrieved only where there are no clouds. This is something that should be discussed.

We have addressed this in section 2.3 Aerosol by adding to page 6, line 9 "While AOD and the Angstrom exponent from MODIS are not available in cloud scenes, the collocated dataset interpolates these between clear sky scenes in order to infer an AI in cloudy scenes."

P5 L22. "The cloud sensitivity" suppose to be cloud albedo sensitivity?

We have clarified this further in section 2.5 Sensitivity page 7, line 26 by adding "he warm cloud radiative sensitivity to aerosol, or λ, is defined as the linear regression of the shortwave CRE against ln(AI). While other studies have called similar metrics a susceptibility, we use the term sensitivity." This is not the cloud albedo sensitivity as ours can include effects on cloud extent/lifetime. To delineate a cloud albedo sensitivity, the indirect effect/ERFaci would have to be separated by its parts, the RFaci and cloud adjustments.

We have clarified throughout the study that we are deriving the warm cloud radiative sensitivity to aerosol.

P6 L6 "inversion strength" is first mentioned here, which seems to be equivalent to the stability.

You are correct. We have added "Stability of the boundary layer is indicated by the EIS." to section 2.4.1 Environmental Regimes to clarify this.

P6. What do the numbers above the sigma mean?

The numbers above sigma represent the number of regimes. I.e. we use 7 cloud state regimes, 10 regimes of EIS, and 10 regimes of RH in equation 6, while in equation 7 the number of regimes is reduced due to sampling on a regional vs. global basis to 4 cloud state regimes, 5 regimes of EIS, and 5 regimes of RH. This is common notation when using sigma (Σ) notation of summation.

P7 section 2.6. The cloud regimes are simply LWP bins? Definition of cloud regimes is far more complex (e.g. https://doi.org/10.1002/2016JD026120).

We understand that other studies have defined cloud states/regimes differently than other studies and have added to address this in section 2.4.2 page 7, line 6 "Although there are other definitions of cloud regimes and cloud states used in other studies (e.g. Oreopoulos et al. (2017)), throughout our results and analysis, cloud state or cloud state regime will refer to observations binned by liquid water path."

P7 L30 "Low LWP clouds are less sensitive to aerosol" - but it is the thinnest clouds that response the strongest to the Twomey effect.

We have rephrased our statement to reflect that based on our results, the thinnest clouds showed the lowest sensitivity. We have added to page 10, line 13 "From Figure 2, the lowest cloud states are less sensitive to aerosol, with a steep increase at ~.8 kg/m2." This is a result seen in our analysis based on observations with minimal constraints, unlike the model Twomey used which was idealized and did not include processes that could reduce the CRE of extremely thin clouds.

P8 L9-10 Define the LWP bins.

The limits of the LWP bins can be seen on the figures and would add very little if explicitly stated in the text. We have added to further clarify how we established these limits on page 7, line 15

"The number of LWP bins decreases from global to regional analysis due to sampling; on a global scale, seven LWP regimes are used, while on a regional scale, only four LWP regimes are used. Limits are placed to separate out the signals of low LWP clouds vs. high LWP clouds, as low clouds may be affected by evaporation-entrainment feedbacks while high LWP clouds may be affected by precipitation (Jiang et al., 2006; L'Ecuyer et al., 2009). While the environmental regimes are established on a percentile basis, cloud state regimes are set by having an increased number of bins for the lowest LWP clouds and a bin limit always set at 150 g to delineate clouds which are extremely unlikely to precipitate (< 150 g/m2) and clouds more likely to precipitate (> 150 g/m2) (L'Ecuyer et al., 2009)."

Do you do any significant tests?

Yes, to include the regime in analysis it must have at least 100 observations and a Pearson correlation coefficient greater than .4. These criteria are also in place when the sensitivity is found on a regional basis, where the environmental regimes are more likely to have less than 100 observations or a worse linear fit. We have added to section 2.5 Sensitivity page 7, line 31 "The sensitivity is only included if there are 100 observations within the regime and the linear regression Pearson correlation coefficient is greater than .4."

Figure 6 panel h is not mentioned in the caption. What does the color bar mean?

The colorbar for panel h is the summed, weighted sensitivity. We have added to caption for Figure 6: "Panel (h) is the summed, weighted sensitivity within each environmental regime. The weighted, summed sensitivity is -10.6 Wm−2/ln(AI) (sum of panel (h)). Note the colorbar for panel (h) is adjusted due to weighting."

P14 L1. This sentence needs context and further discussion, rather than just stating it.

We have chosen to remove this sentence.

P16 L5 "top" of what?

We have changed this to "top panel of Figure 8."

P16 L7 "stability, entrainment and cloud morphology" are equal to EIS, RH and LWP?

Yes you are correct, we use the terms stability, entrainment, and cloud morphology interchangeably with EIS, RH, and LWP respectively in the discussion. We have addressed this through earlier comments and clarified our terminology in the Methods section.

P16 L18. An explanation regarding the relationship between entrainment and particle size is needed here.

We have added to the Discussions section Page 19, line 23 "Entrainment of drier air will force evaporation, decreasing particle size, while entrainment of moister air could have no effect or a reverse effect, increasing the number of CCN within the cloud."

Considering adding figure 9 to figure 8.

We separated them to help the reader focus on figure 9, where all constraints are in place, rather than only a panel of figure 8. Figure 9 is the final focus of our discussion and therefore is better suited to be its own standalone map, rather than a panel of figure 8.

P18 L16-18. I'm not sure about the context of Jiang et al. 2006 here. In their study the additional aerosols were related to enhanced evaporation, which limited the cloud life time. The study was focused on cumulus field.

We chose to cite Jiang 2006 as it was one of the first studies to theorize an entrainment-evaporation feedback. While their findings were limited to cumulus, this *does not mean the process could apply to other warm clouds like the thinner cloud states of our study. We have added to the Discussions page 20, line 7 "…which would be the result of forced evaporation and reduced particle size. The reduced particle size would affect the lifetime of the cloud as well as the cloud albedo, reducing the sensitivity of the warm cloud radiative effect to aerosol loading as seen in our results for some unstable, dry regions (Jiang 2006)."*

P18 L19. How turbulence decreases the activation efficiency of aerosols? Turbulence can also lead to secondary nucleation due to super saturation fluctuations.
Turbulence and higher in cloud updraft speeds can increase the efficiency of aerosol activation under certain conditions. Stable boundary layers have almost a "cap" at the boundary layer top, which acts to dampen cloud growth. Unstable boundary layers are less likely to have the "cap," meaning more turbulence and higher updraft speeds lead to higher cloud tops with possibly the same amount of activation. We have added to page 20, line 4 "Unstable conditions lead to strong vertical mixing and a reduced aerosol sensitivity, as activation favors strong vertical mixing in a stable environment. Unstable local meteorologies alter the conditions of aerosol activation (Cheng 2017)." to explain the role stability plays in modulating aerosol-cloud interactions.

P18 L26. You mention here that wind speed can affect cloud cover. Why didn't you include also wind speed in your parameters?
We chose to use only EIS and RH as constraints on local meteorology as they are the strongest modulators with CRE along with LWP. During initial analysis, using multivariate linear regressions, we found the highest correlations and amount of variance explained with EIS, RH, and LWP than suface wind. We have added to page 20, line 14 "Surface winds were not included in analysis because the dependence of the warm cloud radiative response to aerosols depends most on LWP, RH, and stability, with only some regions showing a dependence on surface winds in our initial analysis." to explain this reasoning.

P19 L19-21. I would expect decreasing stability not to decouple clouds from the surface due to more mixing. Also note that decoupling that occurs when the stability is increased can inhibit cloud breakup (https://doi.org/10.1029/2018GL078122). Please clarify. Where is the role of aerosol here?
The decoupling process occurs when warm marine boundary layer clouds move from a stable to less stable environment. A less stable boundary layer is more likely to have a higher boundary layer top height, increasing the chances of the cloud becoming decoupled from the surface. We have added to explain this process further to page 20, line 34 "The negative sensitivities seen in the unconstrained top panel of Figure 8 are likely a result of this process, which happens simultaneously with a reduced stability, and epitomize how a single linear regression of warm cloud CRE against ln(AI) can capture meteorological effects when unconstrained (Wyant 1997)."

P20 L10-11. It would be helpful to reference the relevant figures here and in the last paragraph where the sensitivities are given.
We have added the appropriate figure references to the Conclusions section.