Overall comments:
I am pleased that the authors have worked hard to address most of the comments - the manuscript is much clearer now. Many of my concerns have now been satisfactorily addressed. However, there are several key comments from the first review that were not addressed, and one comment where my suggestion was misinterpreted, for which I have added clarification. I also have a few new comments, now that I better understand the methodology.

Comments not addressed from the first review:

"Section 2.3: A major issue with using the MODIS AI, which is a column-averaged value, is its inability to co-locate aerosol layers with clouds. In some cases, for example, there may be long-range transported aerosols at high altitudes that lead to high AI values that are not representative of the aerosol conditions that the low clouds of focus in this study experience. This is of particular concern over the low-altitude marine regions of focus in this study, which are often quite clean toward the surface. Uncertainties in aerosol-cloud co-location have high potential for biasing these results in time and space, and potentially may lead to incorrect conclusions. Therefore, I recommend that the authors not only acknowledge this uncertainty here, but that they also at minimum include a short literature review of whatever is known about when and where the biases would be most likely to occur (e.g., based on CALIPSO and/or model output) for the regions of focus in this study. If the authors have the resources to do extra analysis to clarify or reduce this uncertainty as it applies to their results specifically, that would make this contribution much stronger."

"Equations 2-5: As written, it is unclear how the weighting was done. Does $N_{i,j,k,l}$ imply that the data were weighted one time, or four separate times with each summation compared to N_k? Others may be able to get this information immediately from looking at the equation, but for readers like myself, the clarification is important."

"Section 3.3: To support the conclusions in this section, I strongly recommend that more information be provided on which colors of the grid cells of Fig. 3a are significantly different from zero. To address the significance, the authors might consider obtaining a confidence interval around the slope for each grid cell (preferably a bootstrapped confidence interval, as that would be valid even if the data don't meet all the assumptions of a normal linear regression analysis). That would indicate whether the slope in each individual grid cell is significantly different from zero, and this information could be conveyed in the figure with markings on the grids."

"Fig. 4: Again, here, I think it would be really useful to note which of the grid cells are significant. Sample number in each grid cell will go down as resolution increases, and that would presumably impact the weighted mean values presented and discussed with respect to this figure, so significance would be a useful metric to help evaluate these results."

"Fig. 1: Please clarify that the R^2 value in Fig. 1 is describing the blue points and not the underlying distributions of all the data, because the largely overlapping red bars would suggest that in fact the correlation of the more raw data before that averaging happens is much smaller.

Attaching a p-value to this and other similar figures appearing later in the paper seems appropriate."

"Section 3.1: Many others have provided similar values to the -12.81 value provided here. It might be good to compare this finding with findings from previous works."

p.7, l. 6: "same number **of** observations"?

Fig. 3: Is the 11 supposed to have a period after it?

Comments on the responses:

Introduction/Methods: Please clarify to the readers why RH and LWP, which are not independent variables, are considered separately, and essentially independently, in this study.

> *The relative humidity of the free atmosphere (defined as 700mb) and the liquid water path from AMSR-E are independent variables. The RH is primarily a function of the vertical motion in the free atmosphere and large-scale circulations, while the LWP is primarily a function of cloud depth, stability, in-cloud microphysical processes, and other boundary layer conditions. While there may be some relationship between these quantities, both can independently modulate aerosol indirect effects ... two clouds with distinct LWP may respond differently to aerosols even in similar RH environments. Thus RH does not directly control the LWP of a cloud or completely define how the SW cloud radiative effect varies with aerosol concentration.*

Thanks for the explanation, I think I now better understand the study's methodology. The term "RH" is used throughout the paper, and at various points I thought that the authors meant the different RH values in the vertical column, rather than RH at the 700 mb pressure level. To avoid others being similarly confused, it would helpful to more clearly differentiate between the two in the text. The authors might consider using a term such as RH_{700} instead of RH, for example, as appropriate (e.g., in the figures, and in equations, as well as in the text). I completely missed this differentiation in the methods, for example, when it is written that, "*When referring to the effects of entrainment, it means the effects of RH. All observations within the 5% - 95% percentiles of both EIS and RH are partitioned into regimes of percentile limits*." I also missed this in the introduction, for example as written here: "*Including RH in aerosol sensitivity studies accounts for some decoupling influence. Models affirm the effects of entrainment on the cloud layer depend in part on RH, as LES have shown RH moderates cloud feedbacks in low warm cloud simulations (Van der Dussen et al., 2015).*" Others might miss that too.

It might be worth clarifying that the Van der Dussen paper referenced above focused on the **difference** in RH at the surface and 700 mb, and not specifically on RH at 700 mb alone, as was done in this study.

So now, taking a step back, what the authors are saying above is that entrainment (if RH at 700 mb really is a good proxy for entrainment, see comment below) is mostly independent from LWP. It is still not obvious to me that LWP and entrainment should be independent - if you have dry air entering the marine boundary layer, wouldn't that tend to reduce LWP on average? Maybe the authors should just plot the LWP vs. RH at 700 mb variables for the dataset to demonstrate the relationship (or lack thereof).

P. 7, l. 4: "*The relative humidity at 700 mb is used as a measure of the effect of entraining free tropospheric air.*" As I understand it, the RH at on vertical level is assumed to be representative of the whole vertical column up to the freezing point, or at least to provide important information for the whole column. However, RH at 700mb will be most relevant for clouds in that general altitude range. Will this bias the results, or add error? What is the variability in cloud locations? This was not quantified. Why not just use RH at the appropriate altitude ranges where the cloud layer is found?

700 mb is the most common level used to represent the free atmosphere. Boundary layer clouds entrain free atmospheric air, so using a level like 700 mb ensures we're getting an accurate picture of the air entering the cloud layer without any contamination from the cloud layer itself in the relative humidity (Karlsson, 2010).

It helps that I now understand the methodology a bit better. However, I am still a little confused. I can see how RH at 700 mb tells one something about how dry the free tropospheric air is that hypothetically could be entrained, and I know there are studies that show that RH at 700 mb is in fact correlated with various cloud properties. However, I am not sure that means that RH at 700 mb is a close proxy for actual entrainment. The authors might consider restating the description of what meteorological factor(s) that RH at 700 mb represents in a more specific way throughout the paper.

Fig. 5: This figure seems very likely to have an error. I do not see how the clouds at two extreme EIS and RH values can have the highest frequency of occurrence. If the x- and y-axis ranges were selected appropriately, one would expect the points approximately in the middle to be most frequent, and the points at the edges to be least frequent. Also, why is there such a strong mirror-like diagonal pattern in the plot? Natural data rarely show such a distinct pattern unless the x and y variables are highly related to each other. Please check that the data plotted here are correct.

There is an inherent relationship between the estimated inversion strength (EIS) and the relative humidity of the free atmosphere (RH). To alleviate any misunderstandings of the two meteorological variables, the relationship between EIS and RH has been explained in more detail in the Methods. The EIS depends in part on the height of the 700 mb isobar, which would directly depend in part on the relative humidity of the free atmosphere (define as 700 mb). There is some covariance between these parameters that we have now tried to address. Figure 5 is correct as it simply shows that marine warm clouds exist within environmental regimes of EIS and RH. There are well known phenomenon controlling each that lead to a relationship between the two that is not the *focus of this study as could be explained further in "On the relationship between stratiform low cloud cover and lower-tropospheric stability" by Wood and Bretherton 2006. All following analysis and figures are correct according to our observations and reanalysis used.*

We have added to section 3.3 to address the pattern: "The mirror pattern is likely the result of the EIS in part having a slight dependence on RH, as the RH can alter the height of the 700 mb level needed to calculate EIS. This does not impact results as this dependence is accounted for by environmental regimes."

And "The moistest, most unstable and the driest, stablest environmental regimes always have the largest number of observations. The moist, unstable regimes are likely comprised of trade cumulus or other pre-convective cloud types in unstable regions like the ITCZ. The dry, stable regimes are likely comprised of marine stratocumulus cloud decks off the coast of west coast of continents with large scale subsidence drying the free atmosphere above."

Ok, thanks, the new text is very helpful in better understanding this figure. But the response above has led to some new confusion on equations 6-8. If RH at 700 mb is used to define EIS, one of the variables being integrated is included in another variable being integrated separately in the same equation. Does this method then make sense, and should we be worried about co-dependency resulting in spurious results? Maybe it matters, maybe not, or maybe it does matter a bit, but logistically this is the best that can be done with the data available. Either way, perhaps this issue should be addressed in the text.

Section 2.2 or 3.5: A map of the frequency of observations of the subset of clouds compared to all clouds observed in the region would be very helpful for interpreting the relevance of this study. Are the types of clouds studied here more common in some locations than in others, and is there any geographic bias in Figures 8 and 9?

The focus on the study is to reduce the impact of influencing factors like RH, LWP, and EIS on estimating the warm cloud indirect effect. The frequency of clouds is not important, only the sensitivity of certain cloud regimes to aerosol. Including a map of cloud fraction or frequency would convey the message that the frequency is what determines the warm cloud indirect effect, when our study is focusing on how specific regimes of warm clouds independent of frequency can dominate the warm cloud radiative sensitivity to aerosol. Other studies on warm clouds note their prevalence globally.

We have added to the Introduction page 1, line 17 "These clouds are most prevalent off the western coasts of continents as marine stratocumulus, as trade cumulus near the tropics, and as stratus in the storm track regions (Ackerman 2018)."

My apologies if I was not clear enough here. The suggestion was not for the authors to include a CF or a total cloud frequency map. Instead, the suggestion was that they include a map that shows how often their subset of clouds occurs relative to the total cloud observation number.

Please keep in mind that a lot of data were excluded in this study. After excluding all clouds over the ocean at temperatures below 0 ^{o}C, and all multi-layer clouds, another ~20% or so of the remaining data was excluded by removing the remaining most extreme 10% of the each of the EIS and RH at 700 mb data, and any LWP data below 20 g/m^2 or above 400 g/m^2.

Currently, the reader does not have much context for where these data were excluded, but it would not be unreasonable to guess that the excluded data are not randomly distributed throughout the sampling region. Just as an extreme hypothetical example, what if the RH criteria removed 5% of the data in one grid box, but 80% of the data in another? The above suggested map would provide the community with important context for how relevant Figure 9 (the aerosol sensitivity range estimate for this subset of clouds) is at a given region of the planet.

Comments on the new text:
eq. 3: It would be helpful to define LCL and z_{700}.

p. 6, l. 13: "*Binning by relative humidity when evaluating the sensitivity should reduce some bias from aerosol swelling in humid environments*."

Wouldn't that actually add to the bias, because swelling occurs more in high RH environments, so those grids would have a consistently higher apparent AI relative to the other grids?