In general, I like the paper's approach to the critical problem of understanding aerosol impacts on clouds, and how they carefully dealt with co-varying meteorology. I also like how they combine measurements from different sources to get a broader picture of the system and to constrain the observations better. This paper has potential to be very important and useful for/referenced by a host of other studies using a similar methodology in the future.

We thank the reviewer for taking the time to read and comment on our paper. We will first address the major points, and then the specific comments.

Substantial extra work is required to clarify and explain the methodology. Another person attempting to replicate this study could probably not do so, as it is written now, and this makes it difficult for a reviewer to fully judge the value of the work. Unfortunately, by the time I reached section 3.5, the cumulative uncertainty I had with the methodology was large enough for me to have strong reservations about my ability to judge the meaningfulness of this section. That said, I do believe that if the authors answer the questions in the specific comments fully, I can better judge of the work in the next revision.

We agree that more information should be added to help those would like to reproduce our study. The methodology section has been expanded upon. We hope that by addressing the questions outline below further rectify this issue and help the reviewer and future readers understand how they could implement a similar methodology.

There is a possibility that serious errors were made regarding Fig. 5 that might have a large impact on the results. For the reasons listed in the specific comments below, I request that the authors double-check their results carefully, and if necessary, re-run the analysis.

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.

A variety of confounding factors were ignored here, including the difficulties in co- locating aerosols and clouds based on AI, errors and biases in near-cloud satellite aerosol detection, and potential confounding influences of

aerosol semi-direct and direct effects. These should be addressed and acknowledged.

These are now more distinctly addressed and acknowledged in the methods section. We agree there is some measure of uncertainty when using satellite observations to understand cloud and aerosol as clouds invariably affect near-cloud aerosol.

Throughout the paper the conclusions tended to be a bit overstated. It should be made clearer in the manuscript that the study only focuses on a subset of data, and that this subset is not necessarily broadly representative of all conditions.

The focus of the study and the conclusions drawn from the results do only apply to warm marine clouds, however these clouds are vital to understanding many different parts of the climate such as the sensitivity and radiative balance as mentioned in the introduction. A significant source of error in the IPCC's climate sensitivity is from the indirect effect. I have added more reminders in the introduction and methods that this study applies only to warm marine clouds. The importance of understanding and quantifying the warm cloud indirect effects is widely accepted. Twomey's 1977 study of the impact of pollution on Earth's albedo has been cited over 2000 times, while Albrecht's later study in 1989 has been cited over 3400 times. Aerosol impacts on continental and poleward clouds are offset by the brighter surfaces and therefore reduced impact of the indirect effect in these regions.

Specific Comments

Title, abstract, and conclusions: I suggest that the title and abstract better clarify the focus on warm marine clouds, and contain some stronger hints of the large remaining uncertainties (e.g., by adding "Better Understanding Aerosol-...." to the title). The reason for this suggestion is that large groups of clouds were excluded in this study, and there were some fairly major inherent uncertainties in the methodology. The study focused on daytime, single-layer, warm clouds, and as best I can tell, it only includes clouds with latitudes < 60^o and over the ocean. LW forcing at night was excluded entirely. Therefore, the study cannot address several important complex cloud-radiation interactions related to aerosols. For example, the method presented here would not address ice nucleating effects or seeding effects in multi-layer clouds, which can be quite complex. The results may also not be applicable to terrestrial areas where, for example, diurnal effects of heating can be much more variable.

This is true. As stated above, warm marine cloud systems are known to exert a strong influence on climate sensitivity, but these are certainly now the only cloud type on Earth. The title has been adjusted to: "Understanding Shortwave Aerosol-Cloud-Radiation Interactions in Marine Warm Clouds Using Local Meteorology and Cloud State Constraints." We have also identified the exact clouds we are studying in the abstract.

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.

Methods: Please add a table where readers can quickly find what subset of clouds were included in the study. From Figures 8 and 9, it appears that terrestrial clouds and clouds poleward of 60o were eliminated. However, this is not explicitly stated in the paper. A concise central location to find which latitudes, LWPs, and temperature levels, etc. for the subset of clouds assessed in this study would be useful.
We have added on page 5, line 2 "between 60◦N and 60◦S."
This information is provided in section 2.2 Cloud. We state in the first line of this section "…restrict analysis to single-layer, marine warm clouds between 60◦ N and 60◦ S" and "satisfy these criteria (seen by CloudSat or CALIPSO, below the CloudSat determined freezing level, and LWP between .02 and .4 kg)" when explaining the observations chosen for analysis. We feel this is too little information to warrant adding an entire table to the manuscript.

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)."

p. 4, l. 27: "*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 freezing level, and LWP greater than 20 g m-2)*"

What is meant by "below freezing level"? Is that determined from MERRA2 temperature profiles below 0 oC? Please clarify.
Freezing level is determined by the CloudSat 0^{o} isotherm from ECWMF-AUX product. Below freezing level means the entire cloud observed by CloudSat and other satellites collocated with CloudSat was contained to the layer at or below freezing level. The focus of our study is on liquid containing clouds only, not mixed phase or ice. Therefore, by limiting to clouds below freezing level, we guarantee the clouds do not contain ice or supercooled liquid.

As I interpret it, this suggests that the altitude or pressure level where the results are obtained varies by profile, and that this altitude would probably vary quite a bit over latitude and surface type. If this is the case, how do cloud altitudes in the study vary?
Clouds do vary with altitude, however by focusing on maritime liquid clouds, the variation will be limited by the boundary layer height. This varies with EIS, which we account for in our regime framework. In essence, by accounting for EIS, we are also accounting for any effects of cloud top height. Further, when the sensitivity is calculated on a regional basis, this will further constrain any small variations in height.

Can we rule out that vertical variation in the clouds being studied would not add substantial error or bias the results (e.g., by introducing different aerosol types at different levels, or horizontal/vertical winds, etc.)?
We cannot rule out that regional variation exists in aerosol type or cloud type, which is why the results are eventually found on a regional basis to account for some of this bias.
We have added to section 2.2 Clouds "All observations are restricted to below the freezing level of CloudSat which is determined using an ECWMF-AUX collocated reanalysis dataset and set where ECWMF determines the 0^{o} isotherm." And further on in the same paragraph we remind the readers again that observations are "below the CloudSat determined freezing level" to clarify that it is below the freezing level determined by CloudSat and not MODIS. We have also added that "Marine warm clouds fitting these parameters reside within the boundary layer." to the end of the Cloud section in the Methods to clarify these will be low-level, boundary layer clouds.

p. 5, l. 3: "The shortwave cloud radiative effect (CRE) is then defined in terms of the all sky and inferred clear sky forcings from CERES and cloud fraction from CloudSat." How is CloudSat cloud fraction defined? Which conditions are included in "all sky" conditions? From what I understand, situations when there are multi-layer clouds, and clouds below freezing temperatures, etc. are excluded. If this is correct, then the term "all sky" may be a little confusing, and perhaps other wording would be better.

Yes, as acknowledged elsewhere, this analysis is only for warm maritime clouds. The set of observations our analysis is based on is explained in detail in section 2.2 Clouds. To remind the reader that our analysis is for only a subset of clouds, we altered all "CRE" to "warm CRE" and "cloud" to "warm cloud." This is consistently mentioned further now in the results, discussion, and conclusions sections as well.

p. 5, l. 3: "*The shortwave cloud radiative effect (CRE) is then defined in terms of the all sky and inferred clear sky forcings from CERES and cloud fraction from CloudSat*." How is CloudSat cloud fraction defined? Which conditions are included in "all sky" conditions?

Cloud fraction is defined in Section 2.2 "Cloud"

> *"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 freezing level, and LWP greater than 20gm2) over each 12 km segment of the CloudSat track, 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)"*

From what I understand, situations when there are multi-layer clouds, and clouds below freezing temperatures, etc. are excluded. If this is correct, then the term "all sky" may be a little confusing, and perhaps other wording would be better.

We have added "All-sky radiances from CERES are not restricted to any type of scene and include the raw radiances observed by CERES." to section 2.2.

p. 5, l. 15: Which version of MODIS is used, and why? What is the resolution of these data and how does that relate to the cloud resolution?

We have added to section 2.3 Aerosol "MODIS AI is derived from the auxiliary dataset (MOD06-1km-AUX) developed from the overlap of the CloudSat CPR footprint and the MODIS cloud mask at pixel level."

Section 2.3: MODIS has a variety of known issues with reliably detecting aerosols near clouds. What kind of cloud screening was used, and how sensitive are the results to this choice? It would probably be useful to note in the paper that binning the data by RH conditions could create some biases, due to aerosol swelling near high-humidity conditions typical near clouds.

We have added

> *"*While AOD and the Angstrom exponent from MODIS are not available in cloudy scenes, the collocated dataset interpolates these between clear sky scenes in order to infer an AI in cloudy scene. For lower cloud fraction scenes, this interpolation is more accurate, however it is possible that in higher cloud fraction scenes, the accuracy of AI is reduced. This is a source of uncertainty within our results, but with constraints on cloud state, the error of this interpolation method should be reduced. Binning by relative humidity

when evaluating the sensitivity should reduce some bias from aerosol swelling in humid environments.

to section 2.3 Aerosol.

Equations 2-5: On the first read-through, I was quite confused about the upper limits of summation (e.g., the number 7 in equation 2). Justifying the specific choice of those numbers might make more sense in a later section (e.g., section 2.5) than in section 2.4, where they first come up. Therefore, I suggest the authors make the equation more generalizable by having the upper limit of summation be a variable, to be assigned a value later when explanation for that value can be more logically provided. This might also help if others want to cite this method in future work, but want to use different numbers of states for their specific application. Also, please specify earlier on in the text what the upper limit of summation represents, as this was not clear in these equations and in section 2.4 in general. Moving the following text from p. 7 into section 2.4 where these limits are first introduced could help: "*The regime bounds depend on the resolution used, which is varied to establish the degree to which environmental factors must be constrained to accurately characterize sensitivity*".

We have added to section 2.5:

"Where the numbers for summation come from i.e. the number of regimes of LWP/EIS/RH."

"Where N_k is the number of observations of cloud state k"

"Where $N_{i,j}$ is the number of observations within each environmental regime:"

"Where $N_{i,j,k}$ is the number of observations within each environmental regime when constrained further by each of the state regimes k."

We have also replaced the 7, 10, and 10 with LWPs, RHs, and EISs in the summation equations to clarify what bins are being summed.

Further, we have added to section 2.4.2 "The number of cloud states can be varied. In our results, we evaluate the efficacy of increasing and decreasing the number of cloud states."

p. 6, l. 6: How is estimated inversion strength calculated? (note, some information is provided later, on p. 7, l. 2, but this information is not fully descriptive).

Added from Wood and Bretherton 2006 the equation for EIS to section 2.4.1 Environmental Regimes.

$$EIS = LTS - \Gamma_m^{850}(z_{700} - LCL)$$

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).

p. 7, l. 5: "*All observations within the 5% - 95% percentiles of both EIS and RH are partitioned into regimes*." As I understand it, one nice thing about taking the weighted mean is that you can use all of the data, and still get representative results. Thus, I don't understand why these data were excluded in the first place? (from the above statement, I believe the excluded data would equal between 10-20% of their subset of observations?) Was a similar procedure was not followed for LWP, and if so, why not?
The tail ends of the stability and humidity spectrums were removed because we found they biased the results to the extremes. A similar approach was taken for LWP by limiting it to 20 – 400 g/m2. These results still apply for the vast majority of warm clouds.

p. 7, l. 5: "*Environmental regime limits are defined such that there are the same number observations within each percentile of either EIS or RH. The regime bounds depend on the resolution used, which is varied to establish the degree to which environmental factors must be constrained to accurately characterize sensitivity.*" I am very confused by this part of the methodology. Please define in the text what is a bound and what is a limit. I am guessing that the "regime bounds" are the same thing as the upper limits of summation in equations 2-5? Is the "regime limit" the lengths of the [i,j,k, or l] bins in equations 2-5 or something else? I also think the wording of "each percentile," which in general usage implies 1 of 100 equal groups in a dataset, may also be incorrect because it does not seem consistent with the rest of the sentence and equations 2-5. Did the authors mean bins instead of percentiles? If so, what are these bins, specifically, how were they chosen, and how does the choice of spacing affect the results? I was also confused as to why one would want to group the same number of observations within each EIS or RH percentile [or bin?], if the results are going to be weighted later?
Section 2.4.1 Environmental Regimes has been edited for clarity. We have also added "For example, with 100 environmental regimes, the observations will be binned from by 10 percentile limits of both EIS and RH. Within each row of RH within the regime framework, there are the same number observations as within each column of EIS; however within each individual regime of both EIS and RH, the number of observations is dependent on the distribution of both EIS and RH."

p. 7, l. 19: Did the authors mean "...*warm, single-layer, marine cloud SW*..."? It doesn't appear that they looked at terrestrial clouds, and they stated that they excluded multi-layer cloud cases.

We have changed it to "single-layer, marine warm cloud" in multiple places throughout the text to clarify and remind the reader the results are for a subset of clouds only.

Fig. 1: Please describe in the Figure caption what the red lines and blue dots represent (blue dots are presumably l, but it is best to be completely clear). In the caption, l is referenced. To avoid confusion, please state which l is being referenced (so far l0, lLWP, lENV, lBOTH, and lALL have been defined, but no l without a subscript). Please also specify which l is being discussed in the rest of the paper as well as the symbol is used frequently. Please clarify that the R2 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.
All figure captions have been edited for clarity. In figure 1, we have added "with the red lines representing the standard deviation within each bin of ln(AI) and the blue dots representing the mean SW CRE for each bin." which also addresses how the red lines were calculated.
All lambdas have been subscripted with the correct identifier (0, LWP, ENV, BOTH, ALL).

Figure 2a: What cloud states are included here, and how were they derived and chosen? Some explanatory information is provided in section 3.2, but only after this Figure is referenced, which makes things confusing for the reader. It would be best if the figure could be a standalone item without requiring substantial reference to the text. The y-axis label for Fig. 2a seems to be missing, and only the units are provided. The text explaining Fig. 2a is not explicitly identified in the caption.
We have added to 2.4: "While the environmental regimes are established on a percentile basis, cloud state regimes are set by having an increasing number of bins for the lowest LWP clouds and a bin always set at 150 g/m2 to have a defined boundary between clouds which are extremely unlikely to precipitate (<150 g/m2) and clouds more likely to precipitate (>150 g/m2)."
We have added a better label to the y-axis of figure 2.
We have added more description to the caption of figure 2.

p. 7, l. 29: It might be helpful to reference which equation was used to derive the -13.12 value.
We have added where -13.12 came from.

p. 8, l. 4: "*Constraints on LWP limit these influences.*" This is already a well-known work that has previously established this finding.
You are correct. We have added a citation to work by Feingold on LWP constraints. Further citations are mentioned in the discussion as well already.

p. 8, l. 6: why were 3,7,11, and 23 divisions chosen?

We have clarified in section 3.2

> *"We will be using seven cloud states throughout our global analysis as it appears to capture the impacts LWP has on the sensitivity while allowing ample sampling for further division of observations throughout environmental regimes. The number of cloud states are steadily increased from 3 to 7 to 11 to 23 because those follow a progressive increase in the number of bin limits from 4 to 8 to 12 to 24 limits, respectively."*

Section 3.2: The term "cloud state" is commonly used throughout the paper, and it is the focus on section 3.2. However, cloud state is not explicitly defined in the paper, as far as I can tell, and this is very confusing for the reader. The data in section 3.2 mostly revolve around clouds binned by LWP. Is it possible to just use LWP, instead of "cloud state"? Another minor suggestion: the authors might consider changing "cloud regimes" to "LWP bins" (if this is correct). That would be a lot easier for a casual reader of the paper to understand.

We have changed Cloud Regimes (2.4) to Cloud States and added "Cloud states are defined as a range of liquid water paths, such that the liquid water path is held ostensibly constant."

Fig. 3a: Where is the caption text describing Figure 3a? Please clarify where the -11 value in the Fig. 3 caption comes from in relationship to these figures. Is it based on the weighted mean of the data in Fig. 3a? Where is the label for the z-axis in Fig. 3a?

Added to caption: "When weighted and summed following equation (3), λ_{ENV} is 11.Wm−2ln(AI)."

Also added to end of caption: "…where the red lines represent the standard deviation of the SW CRE within each ln(AI) bin and the blue dots represent the mean SW CRE for each ln(AI) bin"

Figs. 3b and 3c: Please state in the caption how moist and dry environments are defined. Are these figures examples of data within individual grid cells from Fig. 3a? If so, please state that. The red bars seem to suggest that there may be no significant differences between any of the ln(AI) values within Fig. 3b or Fig. 3c, including at very high ln(AI) values and very low ln(AI) values?

Added to figure 3 caption: "unstable (~1K), dry environment (< 10% RH)(b) and stable (~6K), moist environment (>30% RH)"

The red bars are the standard deviation within each ln(AI) bin, while the blue dots are the mean warm CRE for each ln(AI) bin, as now explained in the caption. The difference between high low ln(AI) environments is focused on the mean not deviation. There is ~20 W/m2 difference in the dry, unstable case between the high and low and ~ 35 W/m2 difference in the moist, stable case. The differences are significant enough to have slopes of 10 and -25 W/m2ln(AI) for each case respectively.

p.10, last line: "*To account for the local meteorology, warm clouds are separated into 100 environmental regimes...*" This method seems to closely parallel the methodology of previous work (e.g., Chen et al. (2014)). It would be appropriate for the authors to reference such work here.

You are correct. We have added a citation to this work here. "This approach is similar to other approaches taken to estimate the indirect effect such as by Chen et al. 2014." Chen et al. (2014) is also currently cited in both the introduction and discussion sections.

Section 3.3: Since some of the cells in the figure probably have much greater sample sizes in the natural environment than others, to me, the weighted mean is probably more meaningful than the findings of individual grids, and I think it would be appropriate to stress this more in the paper.

We have added to section 3.3: "The results focus on contrasting individual regimes, while the discussion focuses on contrasting constraints and the weighted, summed sensitivities."

Our discussion section focuses on contrasting the weighted, summed values while the results focuses on how the methodology can identify regime specific responses.

p. 11, l. 3: "*The highest sensitivity is observed in stable regimes (EIS > 5.0) with a moderately dry free atmosphere*." And p. 11, l. 8: "*Above 1 K, λ increases with increasing RH, while in less stable environments, RH plays only a secondary role in modulating the sensitivity*." In Fig. 3a, I don't see evidence so far of there being higher sensitivity in drier environments, or of the latter statement at all. Were Figs. 3b,c supposed to be referenced here? Please provide more information to substantiate these statements.

To highlight the differences in section 3.3 we have added "The less stable regimes in figure 3 exhibit almost no variation in unstable regimes, varying by only ~1 W/m2ln(AI) while more stable regimes can vary by >10 W/m2ln(AI)."

Fig. 4: In the caption, sensitivity of what? 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.

We have changed the caption beginning to "The sensitivity of the warm cloud CRE to aerosol found using equation 3 for environmental frameworks of…"

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.

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."*

p. 14, l. 3: "*Overall, the largest ln(AI) sensitivity is seen in stable, dry environments (Figure 6h)*." I don't see this shown in that figure.

We have added to section 3.4 "These environments are ~ 7K of stability and ~ 30% RH." to pinpoint the signal.

p. 16, l. 5: "*In the absence of constraints (top), λ exhibits larger variations in magnitude and sign than when cloud, environmental, or cloud and environmental constraints are in place (panels b and c and Figure 9).*" Was Fig. 8 supposed to be referred to here? I don't see a panel b and c in Fig. 9, but these trends are not evident in Fig. 8....

We have added references to appropriate figures in 3.5

And also "The unconstrained map (Figure 8 a) varies from -.53 to .77 compared the most constrained map where the sensitivity of warm cloud CRE to aerosol varies only from - .11 to .46."

p. 18, l. 1: It would be useful to also mention earlier on (e.g., methods?) that there were 1.8 million observations in the study.

You are correct and we should mention this earlier. We have added to the methods "Even with these starting constraints on LWP and height, there were 1.8 million satellite observations fitting these parameters within the time period."

References

Ackerman, S., Platnick, S., Bhartia, P., Duncan, B., L'Ecuyer, T., Heidinger, A., Skofronick-Jackson, G., Loeb, N., Schmit, T., and Smith, N.: Satellites see the World's Atmosphere, Meteorological Monographs, 2018.

Karlsson, J., Svensson, G., Cardoso, S., Teixeira, J., and Paradise, S.: Subtropical cloud-regime transitions: Boundary layer depth and cloud-top height evolution in models and observations, Journal of Applied Meteorology and Climatology, 49, 1845–1858, 2010.