Author responses to comments from Anonymous Referee #1 on acp-2022-374

Reviewer comments are in red and italicized, author responses are in black, and "text added to the manuscript is within quotes."

The paper is well written and I only have a few minor comments. The paper is thorough, which is appreciated as it documents the steps of the retrieval and the satellite comparison in a way that will be helpful to others.

The reviewer's comments are appreciated and a response to each comment is provided below.

Line 55: It seems like the authors are only discussing the first indirect effect and not adjustments. They could also cite work by others discussing aerosol cloud adjustments in the context of meteorological confounding variables and causal ambiguity (Gryspeerdt et al., 2019; McCoy et al., 2020). They may also wish to cite (Wood et al., 2012).

Thank you for pointing this out. To address this comment, the text has been edited as follows:

"However, ACI are often masked by meteorological conditions (Mauger and Norris, 2007), cloud adjustments to increasing N_a like invigoration (Douglas and L'Ecuyer, 2021) or non-linear Liquid Water Path (LWP) responses to changes in N_c (Gryspeerdt et al., 2019), and the modulation of aerosol properties by clouds and precipitation (Wood et al., 2012). These confounding influences can be addressed to some extent by constraining meteorological variables that affect LWP and comparing clouds with similar LWP or low precipitation rates (e.g., McCoy et al., 2020; G22)."

Line 146: What is the scale of the profile? Does this mean that on a research flight the max height at which cloud occurred and the min height were used? If the profile is too extensive it is not clear if this is a particularly good assumption and it is unclear why the median was not used. I may have understood what is being referred to and a small amount of additional information here might be helpful to readers.

The average cloud thickness (cloud top height minus cloud base height) was about 201 m (Gupta et al., 2022). The text describes the max/min cloud heights during each vertical transect through cloud rather than the entire research flight. The thresholds for in-cloud measurements are listed to describe how cloud top and cloud base were defined for each cloud profile. Gupta et al. (2021) described the procedures followed to ensure any observations of cumulus clouds above or below the stratocumulus clouds were not included in the observations.

For clarity, line 146 is edited to "For each individual vertical transect through marine stratocumulus, cloud top height….". The following line is also added: "During the ORACLES deployments, the average H was about 201 m (G22)."

Line 343: this compensating uncertainty is consistent with earlier studies such as (Painemal & Zuidema, 2011) and (Grosvenor & Wood, 2014).

Thank you for pointing this out. The following text was added: "as has been reported previously (PZ11; Grosvenor and Wood, 2014)."

Line 402: Assuming uncorrelated random errors would tend to overestimate the error since earlier the authors showed that there were compensating errors?

The compensation of errors is discussed in the context of calculating MODIS N_c since the variables can be in the numerator or denominator in Eq. 7. However, that should be independent of any correlations or relationship between the errors outside of the MODIS N_c calculation. It is unlikely the errors are correlated given the different sources and calculation procedures for the variables. For example, C_w, k, and α come from in situ data while R_e and τ from MODIS retrievals.

Line 431: Nit-picky, but since these distributions are non-normal (N is lognormal) the two sample t-test is not appropriate here.

This is a fair point. The discussion of 95% confidence intervals is removed and the differences between the average values for the variables are listed. For further information, the reader is referred to previous studies (Gupta et al., 2021; 2022) where the average values were listed for each variable in addition to the 95% confidence intervals to allow the reader to compare the two parameters.

Section 4: this section is interesting as it compares places where aerosols touch the cloud layer with places where they do not. The section is a bit excessively descriptive of the figures and could be shortened a bit. Rather than listing differences a table with (for instance) Mann-Whitney U-test statistics could be given. Giving values for differences between contact and non-contact is useful, but a bit hard to contextualize in that no information on the aerosol loading is given.

Table 5 was added to replace some of the detailed discussion from the text. Section 4 was then shortened to avoid any repetition of the information already provided in the tables/figures.

Aerosol loading should not have a direct impact on the comparisons between in situ and MODIS estimates of cloud properties – the primary focus of the current study. For a comprehensive analysis of aerosol-cloud interactions as a function of aerosol loading, the reader is referred to Gupta et al. (2021, 2022) where the topic is discussed in significant detail with additional regimes defined based on both above-and below-cloud aerosols.

Line 432: Why would SST, stability (either EIS or LTS- they are nearly identical in this region) be affecting N? It would only apply to tau and re I believe.

SST or stability could indirectly affect N_c through their impact on R_e or precipitation. Meteorological variables were compared between contact and separated profiles to limit the confounding influence of local meteorology (Gupta et al., 2022). However, the differences were small or statistically insignificant. These parameters were chosen because they can exert an influence on LWP for marine stratocumulus and can influence the assessment of aerosol-cloud interactions as the reviewer comment above (and references therein) highlighted.