We would like to first express our thanks to the **REFEREE #1** for his/her constructive comments. The responses to these are below after the reviewer points that are in bold.

This manuscript uses AERONET AOD products retrieved from the spectral deconvolution algorithm to 1) quantify the AOD enhancement in cloudy-sky conditions, 2) examine the change in Angstrom exponent due to cloudy-sky, and then 3) propose potential explanations that are responsible for the change in Angstrom exponent. While I find the scope of the work is interesting, I have some concerns.

1) The use of "Level 0", the data points that got removed from Level 1 and was not included in Level 2 due to cloud screening: The whole manuscript is based on the assumption (although the authors treated it as a fact) that the fine- mode retrieval that is in Level 1 but not in Level 2 represents aerosol properties in cloudy conditions. The problem is - those retrievals were removed at the first place because we don't know how good those retrievals are and how much they are contaminated by clouds. The authors argue that clouds affect coarse-mode retrievals but not fine-mode, and therefore, the use of fine-mode retrieval is OK. However, I don't see any evidence to show that those retrievals are valid and indeed representative. The paper they cited (Chew et al.) used level 1.5, not level 1, so Chew's conclusions shouldn't be applied directly without caution. In short, as the authors mentioned, it is somewhat surprising that these data have not been fully exploited, but there is a reason for that. It is not scientifically rigorous to use retrievals without checking if those retrievals are meaningful!

We provided a brief note already earlier during the open discussion regarding this reviewer point. Many of these clarifications/justifications are now included also in our revised manuscript to explain the quality assurance included in the AERONET measurement data set that we analyzed and thus to justify their use in our study. We also included some illustrative plots, both in the manuscript and in the Supplement, to indicate how the L1 fine mode AOD is indeed a meaningful measurement also in cloudy conditions.

Minor comment – Page 4: do the authors really mean "the latter for all-sky and the former for clear-sky"? I think it should the other way around.

This is right, it should have been the other way around. This has been now corrected in the revised manuscript.

2) The analysis of seasonal variation and significance:
It would be better to clearly describe the sample size used in each bin, and to include retrieval uncertainty into these analyses. While the data range (figures 5-15) in each month spreads quite widely, it would improve the manuscript greatly by providing more critical discussions about them, rather than simply focusing on means only. Also, the authors throw in something like "For cumulus clouds in the mid-Atlantic US, the AE ... " or "marine stratocumulus" for Lanai, which needs more care; these statements should be supported by some scientific evidence (a quick way will be to check weather state from ISCCP).

We included the sample size in each bin. Also, the discussion of the monthly plots is now more thorough.

3) Parcel model runs
I thought this part is interesting, but the current descriptions lack logical connections and are very dis-organised. I don't think readers can replicate simulations/results based on the current form, and I would strongly recommend rewriting this part. Here are some specific examples, which hopefully can help the authors understand why the current form could be quite confusing and unclear.
a) Could the authors make it clearer about the initial size and composition distribution used in the simulations? Like, the sentence on Page 14, 'For less hygroscopic aerosol composition …, e.g., the one we assumed for our Walker Branch simulations", which should be described clearly right in the beginning. What is the growth factor used for Walker Branch? Also, on Page 16, it is mentioned that a very narrow lognormal size distribution, but I don't recall what is used exactly in the control experiments? Perhaps it is mentioned somewhere in the manuscript, but these things should be introduced in a more coherent and organised manner.

The modeling section was strongly modified and re-structured. Therefore these points above, as well as the modeling related reviewer points below, are hopefully adequately addressed by our new revised manuscript.

b) Could the authors make it clearer how the total column AOD and AE are calculated in cloudy and clear-sky conditions? A lot of assumptions are made there and it is unclear where is the justification, and why this will be consistent to observations.

The model calculations are described in more detail in the revised manuscript.

c) Page 17: why using different combinations of wavelengths for Angstrom exponent calculations?

Good point. We agree that it is better to be consistent and changed the wavelengths in these runs. However, as expected, the pattern of AE as a function of size in this plot (and thus the conclusions) did not change.

d) The context of "For instance, for Walker Branch there is … for sizes above this limit, even close to the cloud top" on Page 17? Also, which part of Figure 18 helps conclude the last sentence of section 3?

These points are clarified in the revised manuscript.

e) Errors on Page 16, "WB@820 refers to the Lanai simulation", legend in Figure 16, and captions for Figure 18.

These are corrected.