This work estimates various cloud susceptibilities, including Nd-to-aerosol, LWP-to-Nd and precipitation-to-Nd, using long-term synergistic dataset of CALIPSO-CloudSat-Aqua/MODIS. A specific focus is on how the vertical co-location of cloud and aerosol layer affects the ACI estimates. This produced dataset that the authors will make it publicly available, would be very useful for the ACI community.

The manuscript is overall well-written and the methodology is sound. However, some conclusions appear somewhat too strong, and a few statements require clarification (see my detailed comments below). Despite these points, I believe this could be an interesting study for ACP once all concerns listed are addressed.

Major comments:

Introduction:

> I feel many important references were not mentioned in this study. Most points discussed in the Introduction have been already well-documented in previous review papers, e.g., https://doi.org/10.5194/acp-20-15079-2020 and more recent https://doi.org/10.1029/2022RG000799 and the references therein. Would be nice to acknowledge previous work.
>
> For the effect of retrieval bias, I don't think (Varnai and Marshak, 2009) really touched the effect on ACI, instead more about AOD error. A detailed investigation can refer to https://doi.org/10.5194/acp-22-7353-2022, which may be more relevant here. Regarding the aerosol retrieval issue in low aerosol conditions, a reference should be provided (see the discussion about this in above two review papers).

Figure. 2: Similar plots but showing sample size would be help here. Also, it's interesting to see negative signals in some regions, particularly in (b) this seems to be more visible in regions with strong precipitation. Any explanation about this? As I can understand these plots were for all clouds; would be interesting to look at non-precipitating clouds only. Similarly, for L233-237: The easiest way to investigate the effect of precipitation on ACI is making the similar plots as Fig. 4 but distinguishing non-precipitating and precipitating clouds

L213: maybe an explanation on Spearman correlation would help. It's confusing that the data in fig. 3a apparently are not concentrated around the regression line, but r_s are mostly larger than 0.95 and even being 1. Please clarify.

L216-219 (also the argument on 'S-shape' in abstract): I'm not sure how much I can be convinced by this statement.

- I feel the reason why we didn't see a clear 'flat curve' in high σ is the insufficient samples there; binning data into same sample-size bins induces a weak representativity where data are sparse. Even with sparse data, we still see the saturated Nd when σ starts going beyond 0.2-0.3. Thus, the analysis here is not sufficient to demonstrate the S-shape is non-physical.

- Even using boundary-layer SO4 (closer to σ here), the sigmoid shape is still quite clear (fig. 1b in https://doi.org/10.5194/acp-23-4115-2023). A recent study further provided the observational evidence for this sigmoid curve based on long-term trends (https://doi.org/10.1038/s41558-023-01775-5). These should be discussed.
- I'd suggest formulating it in a way that the non-lineaer behavior reported in earlier studies tends to be less pronounce when using cloud-base extinction than column AOD, instead of saying it's non-physical as the results presented cannot justify this strong statement.

L260-261: This is an interesting point. The authors could demonstrate this even clearer by making a panel (b) for Fig 7 but lumping all global data together.

Minor comment:

Since only the vertical co-location of cloud and aerosol layers is studied, the term 'Progress' in the title seems too broad and gives the impression of a review-like paper. I suggest removing it.

L26: Estimates -> Observational estimates

This work largely follows Painemal et al. (2020). The importance of using vertically collocated aerosol has been already well-justified. Would be good to explain what new message one could get beyond the existing literature.

L68: 'shortcomings': Since the text so far only highlights the benefits, it might be helpful to flag what shortcomings the readers can expect next.

' metrics of cloud susceptibilities of ACI' and 'cloud susceptibility' are the same, aren't they?

L80-82: this sentence is hard to read. Please explain what 'This choice of CALIPSO-based dataset responds to limitations of the standard CALIPSO product' means

L93: I personally think CTH is a better term than ZT for cloud top hight, which has been widely used. Would be easier for readers

L99: 'height': is it cloud top height?

Eq 1: Though the authors referred to (Albertcht et., 1990), it's good to provide the full formulation here along with all parameter values need in the calculation so that people can easily follow.

112-113: How it can categorize the low-cloud precipitation rate is not clear. I guess the authors put a 'raining' flag if Zmax?-15, otherwise 'non-raining', right?

L124: Could you clarify what you mean by "the closest CloudSat CPR pixels to the 25-km line"? What exactly does the 25 km line refer to here?

L134-136: it's a bit unclear if the cloud-top height is from MODIS or CALIOP as stated earlier? To match and 1x1 modis pixels I'd assume it's from MODIS right?

L140: the threshold is generally set to 4; could you explain why 2 is used here? Does it mean more optically thin clouds are included in this study?

Eq3: Only data with CF>80% are analysed. In this case, S_CF cannot reflect the real effect. would be nice to mention this limitation here though it appears quite later in the paper

L207: 'is constrained using AOD' what does this mean? Is σ vertically integrated into the value of AOD?

Fig.7: It would be easier to follow if the authors marked these 4 regions in Fig 6.

l261-262: I think the story in Arola et al. (2022) is quite different to the argument here. They attributed the invert-V to retravel errors. Citing this paper here seems a bit confusing unless the authors make this clear.

L266: It's Intuitive that the product of S_nd_lwp (fig. 6a) and ACI (Fig. 4a) should be negative as their signs are opposite, especially in Tropics; so it's kinda surprising that it turns to be negligible. Could the authors explain this a bit more?

L298: I'd avoid words like "novel" or "new," or anything implying the study is the first to show a particular conclusion. A more neutral phrasing would work better.

It would be easier for readers to follow the results if Fig. 4 were placed as a separate panel within Fig. 9; so that readers would not need to scroll back and forth. And why does the ACI(based on AOD) index appear to be negative here? It's overall positive in previous studies. Would be good to discuss.

L301-303: The authors stressed a lot on the difference in ACI between σ and AOD; but it's very important to mention here that in the end we care about anrgropogenic perturbation of Nd and forcing which also relies on PI-PD change in the utilized proxy, not the simple slope (https://doi.org/10.1029/2022RG000799).

L311-312: the use of AOD doesn't misguide the modelers as long as they are looking at AOD as well. I think this sentence can be dropped.

L311: I find the phrase "unlike previous assessments, but similar to Gryspeerdt et al. (2016)" a bit confusing. Gryspeerdt et al. (2016) is also a 'previous' study, so it might help to clarify what you mean here. For example, do you refer to a specific group of 'previous assessments' using a different methodology?