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Interactive comment on "A new statistical approach to improve the satellite based estimation of the radiative forcing by aerosol–cloud interactions" *by* Piyushkumar N. Patel et al.

Anonymous Referee #3

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In this work, the authors extend a previous study by Quaas et al. (2008) in several ways. First, an assumption in a fitted, sigmoidal relationship between planetary albedo and aerosol optical depth, cloud fraction, and cloud optical depth is relaxed, and allows the authors to compute the relationship using a non-linear approach. Second, the authors evaluate the relationship between planetary albedo and radiative forcing due to aerosol-cloud interactions (RF_{ACI}) derived from satellite data (using their fitting method) compared to offline radiative transfer calculations. Finally, the authors focus their analysis of RF_{ACI} to three small regions and the seasonal cycle of their monsoons. Compared to the offline radiative transfer calculations, using the non-linear approach tends to reduce the root-mean-square error in estimates of planetary albedo and im-

prove correlation relative to the original multilinear approach. Using the non-linear approach, the authors show that in the Bay of Bengal region, natural aerosol produce a smaller RF_{ACI} than anthropogenic aerosol, and that RF_{ACI} is generally much smaller in the monsoon season than at other times of the year.

0.1 General Comments

By extending the method of Quaas et al. (2008) to estimate RF_{ACI} and developing and employing a new evaluation scheme for it, this work contributes a useful analysis to the field of aerosol-cloud interactions. Some additional clarification is necessary, though, in order to document how exactly this work complements Quaas et al. (2008) and what advantages it introduces. Furthermore, the manuscript requires extensive copy-editing; as written, some results are hard to understand due to typographical errors and the manuscript is hard to follow at times. The following lists the key issues that must be addressed before publishing:

- I strongly recommend that the authors request copy-editing services from Copernicus to improve the quality of the manuscript. In the **Specific Comments** section I have tried to document typographical and grammatical errors which produce confusion in interpreting the results, but overall there are many such corrections that should be made throughout the document.
- The authors estimate N_d using an adiabatic liquid water cloud assumption. However, this assumption is invalid outside of stratiform clouds in the marine boundary layer, and similar estimates like Bennartz (2007) clearly indicate that this assumption is highly uncertain outside this type of regime. The authors should discuss the limitations of using N_d in their Central India (CI) region, and in seasons dominated by non-stratiform clouds (such as the monsoon one they analyze).
- Several clarifications should be made regarding the non-linear fitting technique.

First, it isn't clear on line 135 why a_5 should ever be set to 1; Quaas et al. (2008) does not seem to make this assumption - contrary to the assertion in lines 146-147 - and if the authors are suggesting this as an alternative formulation of equation (2), then some justification is necessary. For instance, in all except one of the nonlinear fits provided in Table S1, a_5 is an order of magnitude smaller than 1. Second, the authors should clarify what method is used to perform the non-linear fits with a citation if possible, even if it's something standard such as non-linear least squares, for the sake of reproducibility.

- In Section 3.2, the authors present an independent estimate of $\mathrm{RF_{ACI}}$ for validation purposes using a radiative transfer code. The authors should include some discussion of how this approach differs from those in the literature, such as Bellouin et al. (2013), and what its limitations are given the dataset and methodology employed. Furthermore, if the use of the radiative transfer code is so readily evaluated in conjunction with satellite data, then what advantage does equation (2) offer in terms of developing constraints for $\mathrm{RF_{ACI}}$?

- In equations (3-4) the authors require estimates of $\frac{d\ln N_d}{d\ln \tau_\alpha}$ but do not state where these come from. If they use the regression approach of Quaas et al. (2008), then this should be indicated.

- The discussion of uncertainty in the estimates of $\mathrm{RF_{ACI}}$ in Section 4.2 does not seem to follow from the results presented earlier in the manuscript. On lines 258-259 the authors suggest that the nonlinear fitting approach reduces uncertainty by 20%-25%, but it is not clear where this estimate is coming from. The authors' analysis of the reduction in RMSE of planetary albedo compared to the radiative transfer simulations is not a measure of uncertainty, if that's what this statistic refers to. This estimate should be removed, and the authors should instead expand their error-propagation analysis to justify the estimate of ± 0.08 W/m^2. For instance, in relation to the previous comment, how does uncertainty in the re-

gional and seasonal estimates of $\frac{d\ln N_d}{d\ln \tau_\alpha}$ influence the estimate of $\mathrm{RF_{ACI}}$?

0.2 Specific Comments

- Lines 12-15: This sentence is very awkward and partially repeats itself halfway through.

- Lines 18-20: Sentence needs to clarify what is being compared against with the correlation and error statistics.

- Lines 37-38: Following McComiskey et al. (2009), $\frac{d\ln N_d}{d\ln \tau_\alpha}$ is not computed using partial derivatives and is not calculated with LWP held constant; please remove this statement, or clarify how this relationship differs from the other ACI metrics that could be considered.

- Line 39: Need to define r_e as "droplet effective radius"

- Lines 40-41: Because they are column integrals, metrics like aerosol optical depth do not necessarily represent just the particles impacting clouds - just the total ambient aerosol burden, particularly with respect to larger particles. Please rephrase accordingly.

- Lines 68-74: The first sentence is something of a non-sequitur and could be removed entirely. The second sentence is awkwardly phrased; it would be better to point out that the aerosol mixture in this region is very heterogeneous in time and space with respect to size distribution and chemical composition.

- Lines 82-85: It would be extremely helpful to the reader if you included a figure that outlined where these regions are on a map.

- Lines 91-93: This sentence should be flipped with the following and the beginning of the paragraph re-written to emphasize that your data comes predominantly

from MOIDS and CERES; then you should dive into the details of which data product (and citation) you use for each specific derived quantity.

- Lines 128-131: Pursuant to the general comment about N_d, the authors should discuss the limitations of this method for estimating N_d
- Line 132: Where does this particular value for γ come from?
- Lines 144-152: At a minimum, this paragraph needs additional detail on what nonlinear fitting approach was used (non-linear least squares? some other method?) with a citation if applicable.
- Line 164: Before this sentence, it would be useful if the authors list the variables required to perform their SBDART computations.
- Line 194-185: Please clarify the difference between τ_α and $\tau_\alpha^{ant/nat}$. Presumably the first is the total AOD and the second is just the anthropogenic/natural contribution to AOD?
- Line 185 and Equation 5: I would recommend writing out explicitly $N'_d = N_d + \Delta N_d$ in both locations.
- Lines 202-203: "Weight" is the wrong word; according to Table S1, it's simply that the magnitude of the coefficients are different.
- Lines 225-227: Rephrase to avoid using terms like "satisfactory results" in preference for neutral language.
- Lines 229-231: The phrasing "... decreases RMSE by from 0.007 to 0.011 ..." is clearly a mistake; please delete whichever word is wrong and be clear about how the RMSE is changing.

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