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.

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Received and published: 2 February 2017

Response to the Anonymous Referee#3's comments

General Comments:

By extending the method of Quaas et al. (2008) to estimate RFaci 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.

Authors: We express our sincere thanks to the referee for his/her insightful and constructive comments and suggestions on this study. The comment/suggestions were to-the-point and very valuable for us to improve the scientific and technical clarity and quality of the manuscript. In the following, we itemize our point-by-point response to each of the concerns raised by referee.

Comment:1 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.

Authors: The given specific comments are seriously considered and the typographical and grammatical errors are corrected throughout the document with the help of an expert to improve the quality of the manuscript.

Comment:2 The authors estimate Nd 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 Nd in their Central India (CI) region, and in seasons dominated by non-stratiform clouds (such as the monsoon one they analyze).

Authors: The limitation of the computation of Nd from satellite retrievals, and the associated uncertainty along with its contribution to the total uncertainty is discussed in the revised manuscript as suggested by the reviewer. This also takes into account new results on the retrievals of Nd from satellites.

Comment:3 Several clarifications should be made regarding the non-linear fitting technique. First, it isn't clear on line 135 why a5 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, a5 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.

Authors: The assumption of a5 set to be 1 was not clearly indicated in Quaas et al., (2008), apologise for this by the second author of our manuscript! - but a subsequent study, Xiaoyan et al., (2014) reassesses the same study by employing updated satellite products, where it is clearly indicated that for this study a5 was set to 1. This second paper is now referenced in the present study for clarity about this point. Second, as per the reviewer's suggestion, more details about the nonlinear least square method are included in the revised manuscript.

Comment:4 In Section 3.2, the authors present an independent estimate of RFaci 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 RFaci?

Authors: As per the reviewer's suggestions the difference between the RT simulation of Bellouin et al., 2013 and in the present study is now explained in detail in the revised manuscript. Following Quaas et al., (2008), Bellouin et al., (2013) performed a similar study (in terms of radiative forcing due to aerosol-cloud interactions) making use of the MACC aerosol reanalysis data and they used a radiative transfer code including a Monte-Carlo method to obtain the standard deviation for the analysis of uncertainty. However, in the present study, RFaci is simulated using a radiative transfer code (SB-

DART) to evaluate the performance of the statistical approach used to compute the RFaci. The use of a simple, analytical expression to assess the aerosol-cloud radiative forcing has the advantage of being very easily understandable, accessible and applicable.

Comment:5 In equations (3-4) the authors require estimates of d ln Nd / 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.

Authors: We are thankful to a reviewer to point out the lack of sufficient information. The values of d ln Nd/ d ln $\tau\alpha$ is obtained here are the same as Quaas et al., (2008); derived using a linear regression. This is now clarified in the revised manuscript.

Comment:6 The discussion of uncertainty in the estimates of RFaci 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/m2. For instance, in relation to the previous comment, how does uncertainty in the regional and seasonal estimates of d ln Nd / d ln $\tau\alpha$ influence the estimate of RFaci?

Authors: We are thankful to a reviewer for his/her suggestion to expand the error analysis. According to reviewer's suggestion, we introduce the uncertainties involved in the present study due to different parameters considered in this study. The detailed information, including a table about the uncertainty budget is now included in the revised manuscript.

Specific Comments:

Comment:1 Lines 12-15: This sentence is very awkward and partially repeats itself halfway through.

Authors: The sentence is rewritten in the revised manuscript.

Comment:2 Lines 18-20: Sentence needs to clarify what is being compared against with the correlation and error statistics.

Authors: The sentence is rewritten as per the reviewer's suggestion.

Comment:3 Lines 37-38: Following McComiskey et al. (2009), d ln Nd / 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.

Authors: We are sorry to introduce the misleading information in the sentence and according to reviewer's suggestions, the sentence is removed in the revised manuscript.

Comment:4 Line 39: Need to define re as "droplet effective radius"

Authors: re defined as "cloud droplet effective radius" in the revised manuscript.

Comment:5 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.

Authors: The sentence is revised according to reviewer's suggestion in the revised manuscript.

Comment:6 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.

Authors: The suggested sentence is removed and others are rewritten in the revised manuscript.

Comment:7 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.

Authors: As per the reviewer's suggestions, a figure is included in the revised manuscript, which shows the study regions on a map.

Comment:8 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 MODIS and CERES; then you should dive into the details of which data product (and citation) you use for each specific derived quantity.

Authors: The sentence is revised as per reviewer's suggestions.

Comment:9 Lines 128-131: Pursuant to the general comment about Nd, the authors should discuss the limitations of this method for estimating Nd.

Authors: The limitation of Nd along with its contribution to the total uncertainty is discussed in the revised manuscript as per suggestion.

Comment:10 Line 132: Where does this particular value for γ come from?

Authors: The reference for the value of γ is added in the revised manuscript.

Comment:11 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.

Authors: As per reviewer's suggestion, the detail about the nonlinear method is discussed in the revised manuscript.

Comment:12 Line 164: Before this sentence, it would be useful if the authors list the variables required to perform their SBDART computations.

Authors: Following this suggestion, a list of input variables and their sources are tabulated as table-1 in the revised manuscript.

Comment:13 Line 184-185: Please clarify the difference between $\tau\alpha$ and $\tau\alpha$ant/nat. Presumably the first is the total AOD and the second is just the anthropogenic/natural contribution to AOD?

Authors: Yes, it is correct. The difference between both the terms $\tau\alpha$ and $\tau\alpha$ant/nat are clarified in the revised manuscript.

Comment:14 Line 185 and Equation 5: I would recommend writing out explicitly $N\_d\hat{}=N\_d+\Delta N\_d$ in both locations.

Authors: Terms are modified as per reviewer's suggestions.

Comment:15 Lines 202-203: "Weight" is the wrong word; according to Table S1, it's simply that the magnitude of the coefficients are different.

Authors: The word "weight" is replaced by "magnitude of the coefficient" as per reviewer's suggestion.

Comment:16 Lines 225-227: Rephrase to avoid using terms like "satisfactory results" in preference for neutral language.

Authors: The term "satisfactory results" is replaced in the revised manuscript.

Comment:17 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.

Authors: The sentence is revised and rewritten in the revised manuscript.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/acp-2016-680/acp-2016-680-AC2-supplement.zip

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