

Interactive comment on “A review of measurement-based assessment of aerosol direct radiative effect and forcing” by H. Yu et al.

Anonymous Referee #1

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Specific goals of this review paper as stated on p. 7652 are to 1) review measurement-based understanding of tropospheric aerosol and their direct effects, 2) estimate uncertainty associated with them through examining the differences among various estimates, and 3) explore the use of recent measurements to improve the performance of model simulations. The review provides thorough descriptions of the differences in estimates of AOD, DRE, and DCF derived by a variety of methods. In doing so, it provides bounds for these parameters as retrieved by satellites, models, AERONET measurements, and combinations thereof. Certainly the first stated goal of the review is achieved. In terms of goal #2, lower and upper bounds of AOD, DRE, and DCF are developed based on the comparison of different methods. This approach can provide

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mean values, standard deviations, and standard errors for the parameters resulting from the combination of satellite vs. model methods, etc. However, it does not provide the uncertainty associated with the parameters derived from the different methods or from a combination of methods. The paper would greatly benefit from an uncertainty analysis that 1) provides an estimate of the uncertainties for all methods involved, 2) identifies sources of errors for all methods involved, and 3) attributes differences observed between methods to specific factors or sources of error. Estimates of AOD and DRE retrieved from satellites, satellites integrated with models, and model simulations are compared. The general result is that satellites integrated with models deliver values somewhere between the pure satellite- and pure model-based estimates. Specific reasons for why the measurement- and model-based estimates are systematically different are not discussed for different regions or aerosol types. How does satellite integration improve the models and what does this say about specific factors that are limiting model accuracy? How can this approach be used to further optimize model simulations? In summary, the paper does provide a review of measurement- and model-based estimates of AOD, DRE, and DCF in that it defines upper and lower bounds of these parameters by combining results from all methods. It describes differences between methods and lists, in a general way, factors that may contribute to the differences. The paper would be strengthened by the addition of an uncertainty analysis that allows for the identification, quantification (if possible), and prioritization of uncertainties affecting all methods. Based on this analysis, the next steps to be taken could be listed and prioritized so that we can most effectively improve the accuracy of estimates of DRE and DCF. Certainly within “the list” would be an emphasis on the improvement of models through integration with measurements and how best to achieve this. Specific comments follow.

1) It is stated in the abstract (line 9) that this paper offers an assessment of AOD, DRE and DCF focusing on satellite and ground-based measurements supplemented by CTM simulations. On page 7652 (last paragraph) three specific goals of the paper are outlined. It would be useful to add these more specific goals to the abstract to

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better define the focus of the assessment (review measurement-based understanding, estimate uncertainties associated with measurement techniques, evaluate the use of measurements to improve model simulations).

2) What is missing from the abstract and throughout the paper is a clear agenda for the assessment. Is the goal to identify uncertainties in measurements and models? To prioritize the uncertainties and their remedies? The paper needs a clearer sense of how this assessment will contribute our knowledge of DRE and DCF.

3) Abstract, line 15: State accuracy of MODIS estimate of human contribution AOD based on fine mode fraction.

4) Abstract, last paragraph: Specifically, what does “these achievements” refers to? The first paragraph of the abstract points more to issues (uncertainty of aerosol absorption, difficulty in characterizing land surface reflection, discrepancy between measured and modeled estimates of DRE) than to achievements. The stated accuracy of satellite retrieval of AOD over land and water is impressive but the estimate of DRE seems much more problematic.

5) p. 7652, line 7: Does “overall forcing” refer to globally averaged? Direct plus indirect effects?

6) p. 7652, line 10: How is the stated uncertainty of DCF defined and how was it determined? Is this the range of values reported by IPCC?

7) Section 2: It might be more appropriate to call this section “Assessment of current abilities in characterizing tropospheric aerosols and estimating the aerosol direct effect.” The section does discuss progress but also mentions limitations in current approaches and identifies areas that need improvement.

8) p. 7656, line 26: Define absorption efficiency as it is used here.

9) p. 7656, line 28: Change to “Higher for AN internal MIXTURE than for AN external MIXTURE.”

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10) Section 2: Of the ground-based networks described, only data from AERONET is used later in the paper. This discussion could be omitted.

11) p. 7662, line 24: What does “quantitative aerosol size parameters” mean, i.e., what is the uncertainty associated with the retrieved parameters? 12) p. 7662, line 10: How much does the new low-light level calibration improve the accuracy of dark water MISR AOT retrievals?

13) p. 7664, line 2: “In conjunction with aerosol measurements from other sensors” This statement is a little vague. What measurements from other sensors?

14) p. 7764, line 10: “Some approaches should be employed” Again, vague. What approaches should be employed?

15) p. 7664, line 16: “Some scientific results are going to be published soon” Not really worth mentioning without including brief details of the results.

16) p. 7666, line 12: Define ABC if not done so earlier.

17) p. 7668, line 9: What is meant by “adequately calculated?” What uncertainty does the current ability to estimate ocean surface reflectance add to estimates of AOD, DRE, and DCF?

18) p. 7668, last line: The concept of global albedo is first mentioned here without an introduction to its significance.

19) p. 7670, second paragraph: Section 2 of the paper alternates between honest and thorough evaluations of our current ability to characterize parameters required for determining DRE and DCF and blanket statements of instrument ability with no supporting information about accuracy or uncertainties. For example, the section on surface albedo (2.2.1.) provides a good discussion of the importance of surface albedo, issues with its determination, and recent developments in its retrieval from MODIS and MISR products. This discussion fits with the goals of the paper that are stated in the introduction (p. 7652). In contrast, the section on clouds (2.2.2.) provides information on

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MODIS retrievals of cloud properties but offers no assessment of the uncertainties involved and how these uncertainties are propagated through the determination of cloud fraction, cloud optical depth, and, ultimately, DCF. The section would be improved by a more uniform assessment of how well current measurements characterize the properties described in each sub-section. Uncertainties associated with current approaches and specific limitations of current methods should be clearly stated.

20) p. 7670, line 6: Should this be “Almost ALWAYS”?

21) Section 2.3. provides a discussion of RTM and a brief description of methodology, limitations and causes of uncertainties. What about CTMs that provide input to RTMs? A brief discussion of their sources of uncertainties and how they affect aerosol loadings and optical depth would be helpful.

22) p. 7672, line 13: How was the clear-sky aerosol direct effect of various aerosol types shown in Figure 4 determined? The beginning of section 2.3. includes a broad discussion of RTMs and issues that limit their accuracy. The 4th paragraph discusses AERONET AOD values and corresponding direct effect but the transformation from AOD to DRE is not described. This description would help to pull together the first part of the section where RTMs are described and the second part where one is apparently used.

23) Figure 4. Figure 4a includes vertical bars which represent 1 standard deviation of the mean. Do the vertical bars in Figure 4b. also represent 1 s.d.? It would be very useful and would improve the discussion to include uncertainties as well as standard deviations. The former will inform how well we can measure these parameters and estimate radiative efficiency while the latter describes natural variability. The addition of an uncertainty discussion would provide more of an “assessment” context thereby fulfilling the goals of the paper stated in the introduction.

24) Figure 4a. With all parameters plotted on the same y-axis it is very difficult to see the significant regional differences. I recommend plotting each parameter (AOD, SSA,

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g) in a separate panel with expanded y-axes.

25) p. 7674, line 17: Are only the dust and sea salt components size resolved?

26) p. 7674, lines 10 -21: Readers not familiar with methods used in the assimilation of satellite retrievals and model simulations will need more explanation than this paragraph provides. Specifically, more detail is required in order to understand the approach of “combining them with weights inversely proportional to the square of the errors of individual descriptions” and “interpolation approach with the Kalman-Bucy filter.”

27) Table 3. What is the effect of different data years for different methods on the comparison?

28) p. 7676, line 1: What is the MOD04 aerosol product?

29) Figure 6: Is this MAM for the years listed in Tables 2 and 3? If so, how much of the difference is due to year-to-year variability? Reporting standard deviation or, even better, uncertainties would allow for a more meaningful assessment of differences in mean values between methods.

30) p. 7680, line 10: How much does the approach of filling in gaps in the MODIS and MISR retrievals with GOCART simulations affect the comparison? Certainly it makes those three methods dependent so that the comparison becomes biased toward GOCART.

31) p. 7680, line 19: How much reduced are the MODIS-MISR differences by the use of the improved low-light level calibration of MISR and land characterization by the MODIS retrieval? Does it eliminate all differences or are there other significant issues involved that affect the comparison?

32) p. 7681, line 27: define ADMs.

33) p. 7682, line 8: Standard error is defined here and used several more times in the

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paper. Is it calculated such that n = number of simulations summed over all methods? For example, in Tables 7 through 9, is n = number of methods used or the total number of simulations performed summed over all methods? Similarly, on p. 7685 (line 3), it is stated that the standard deviation accounts for about 15-25% of the average. Is this the standard deviation over the number of measurement methods used? It would be helpful to include standard deviations (or uncertainties) for each method in Tables 7 through 9 along with the overall measurement and model standard deviation. The reader could then compare reported mean values to the standard deviation instead of relying on one sentence in the text.

34) p. 7682, last paragraph: Possible reasons are given for large discrepancies in model simulations of DRE. The “laundry list” approach for explaining differences does not move us forward in our identification and reduction of uncertainties. What is preventing the analysis from being carried further so that model differences can be attributed to specific factors? It should at least be done for GOCART since it is the model that is used with satellite retrievals for deriving DRE over land. A goal of the paper is to “explore the use of recent measurements to improve the performance of model simulations”. This goal is difficult to achieve without an assessment of what is limiting model simulations. Along the same lines, is it possible to pursue the systematic difference between measured and modeled DRE such that the differences can be attributed to specific factors (other than satellite cloud contamination)?

35) p. 7683, line 6: Does the 60% refer to differences in measurement SFC and TOA cooling? Is 37% really only “slightly” smaller than 60%?

36) p. 7683: It is stated that DRE estimates over land rely on model simulations and satellite-model or satellite-AERONET integrations. The previous discussion on DRE over oceans indicated systematic differences between modeled and measured DREs. The reasons for this systematic difference were not specifically identified or resolved. Presumably these same differences exist over land. How, then, do they impact satellite-model assimilated DRE values?

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37) Figures 7 and 8 are confusing because not all of the methods discussed in the text are shown in the figures.

38) Section 3.3.1.: This section contains a description of the differences in regional DREs estimated by satellite retrievals, assimilated satellite - model retrievals, and model simulations. Methods are compared and differences are noted. In addition, the difference in regional DREs due to different aerosol types is discussed. However, specific reasons for why the different methods yield different results are not given. The assessment would be greatly enhanced if differences could be attributed to specific factors so that uncertainties in the estimate of DREs could be reduced. On p.7686 (line 20) it is stated that “model-satellite integration-based assessments in the region are generally higher than both the measurement-based and model-based estimates” but reasons are not given for the differences. In the discussion of Figure 9, differences are described but (beyond the post-launch high bias of MISR) not attributed to specific factors.

39) Figure 10: It is hard to see the different data sets as points land on top of one another. The use of smaller symbols or more unique symbols (open circles, asterisk, etc.) would help.

40) p. 7689, line 20: It is stated that “DRE estimates from GOCART and LMDZ-INCA simulations and the integrations of GOCART simulations and satellite data sets generally agree reasonably well with AERONET measurements at relatively weak cooling regimes” It would help to plot these in two panels for relatively weak vs. strong cooling regimes. As plotted it is difficult to see that the agreement is better for weak cooling regimes. In addition, this analysis would be made more quantitative if correlation coefficient and slope information were given for the weak vs. strong cooling regimes.

41) p. 7689, line 25: Does “around a year” mean “year round?”

42) p. 7689, line 26: “It is stated that “Such discrepancies may result from inadequacies in both model simulations and satellite retrievals. They would also come from the poor

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regional representativeness of AERONET stations due to the limited number of sites.” More probing into specific factors causing the differences instead of the laundry list approach would strengthen the analysis.

43) Section 3.3.3: Again, no attempt is made to attribute differences in methods to specific factors. On p. 7693 (line 14) it is stated that “Such differences may result from contrasts in aerosol compositions between the Arabian Sea (i.e. dust dominated) and the northern Indian Ocean (a mix of pollution and dust). Would it be possible to probe the MODIS and MODIS_A retrieval algorithms for the different aerosol types in the region and compare those to measurements made during intensive field programs, i.e., is this a problem with the retrieval algorithm? Or is it due to a difference in aerosol type encountered by AERONET during the intensive vs. that observed by MODIS? Is it possible to look at a narrower field of view with MODIS that better matches the AERONET measurements in time and space?

44) Table 18: How are the uncertainties calculated? Presumably these uncertainties represent a lower bound because the sources of error are assumed to be independent (see p. 7696, line 2). Please clarify.

45) p. 7696, line 21: Less constrained than what?

46) Section 4 discusses many factors contributing to the uncertainties in our current ability to estimate DRE and DCF. The paper would be strengthened if these factors were specifically linked to the many differences between methods that are discussed earlier in the paper.

47) p. 7703, line 3: Are three significant figures in AOD warranted here when the stated accuracy is +/- 0.03?

48) p. 7704, line 3: It is stated that “uncertainty in current aerosol single scattering albedo measurements constitutes the largest source of uncertainty in aerosol forcing and climate response.” Yet this review says very little about how the different methods

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handle SSA and how their parameterizations affect estimates of AOD, DRE, and DCF.

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