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Atmospheric Chemistry and Physics Discussions

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

J. Redemann (Referee)

jredemann@mail.arc.nasa.gov

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General comments

The paper by Yu et al. describes a review study of aerosol direct radiative effects, to be submitted as one of three aerosol-related reports for the Climate Change Science Program. The subject matter is of great scientific significance because it assesses current capabilities of observationally-based estimates of aerosol radiative forcing (although the authors reserve the term "forcing" for anthropogenic aerosol radiative effects). Hence, the topic is well suited for publication in Atmospheric Chemistry and Physics.

On the positive side, the authors deserve praise for an incredibly detailed review of every experimental method conceivably involved in the assessment of aerosol direct radiative effects. The paper is based on a very exhaustive literature review and the authors' detailed knowledge of ALL aspects governing aerosol direct radiative effects is evident throughout the paper. The authors need to be commended for their attempts to comprehend the details of every study that is referenced.

There are a few weaknesses to the paper, none of which should prevent the paper from being published. In particular, having read in detail a 114-page manuscript, a comment on its length shall be permissible. I would have simply rejected the paper solely based on its length if it was not for the submission to an on-line journal. Any paper of such depth should be considered for a monograph instead of a journal manuscript. In connection with this point, the authors should be asked to improve the structure of their paper. A table of contents and some sub-sections in chapters 2 and 3 could help. The simple fact is that the paper is very difficult to digest in its current form.

I would suggest that the paper "should be published in ACP after minor revisions". Because the weaknesses can be addressed rather easily and because of the importance of their review to the scientific community, the authors should be encouraged to revise their manuscript.

Specific comments

The authors should be asked to respond to the following question and comments regarding the contents of their paper:

1) My main scientific concern is the practice of "science by consensus" and the reporting format of uncertainties used in the paper. As an example, the small spread in "a number" of satellite-based assessments of the direct radiative effect is reported as a "standard error" along with the mean of the results in the form -5.5(+-)0.2Wm-2. For this quantity, the spread (or standard deviation) happens to be very small. Although sometimes it is explicitly stated that the second quantity in the reported format is the

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standard deviation, in many instances throughout the paper it is not, giving the reader the impression of a proper uncertainty analysis (instead of just the spread of a variety of results). Further, the paper states (page 5) that one of its explicit purposes is “to estimate uncertainty associated with (measurement-based assessments of aerosol direct radiative effects) through examining the differences among various estimates”. As a reviewer I question the notion that the variability of an ensemble of subjectively represented scientific results is an indication of the uncertainty of the mean. By “subjectively represented” I refer to the choice of spatial and temporal averaging of results. A good example is provided in the AOD assessments presented in the paper - the MODIS- and MISR-derived annual average AOD (land + ocean) are 0.188 and 0.199, respectively. Using the authors’ mean(+-)std-representation, the (MODIS+MISR) annual average AOD would be 0.194(+-)0.008, which would suggest a much smaller uncertainty than either MODIS or MISR can achieve. In conclusion to this point, I think it should be stated that the reported standard error does not generally denote a true experimental uncertainty, but instead the spread of results after extensive spatial and temporal averaging.

2) My only other major concern is the lack of rigorous definitions for the various quantities (e.g., DRE, ARE, DCF, ACF) in terms of physical observables (e.g., net irradiance), and statements about the exact spectral range for which results are reported. For instance, many of the forcing efficiency results in Tables 14-17 were originally reported for very different spectral ranges. Statements about conversions the authors may have applied (or applicable spectral ranges, if not) should be included.

3) Entire text, incl. title: Please check the use/omissions of definite and indefinite articles.

4) Page 5: Please indicate the organization that initiated CCSP and give a reference.

5) Page 14, end of POLDER section: A statement that the relatively poor spatial resolution has an effect on cloud screening could be added.

6) Page 15, MISR section: I assume “all kinds of ocean surfaces” refers to differing wind speeds, chlorophyll conc., etc?! Please clarify.

7) Page 20, 21 and Figure 2: I did not follow the discussion about black- and white-sky albedo, partly because the left panel of Figure 2 was not legible in my copy. Please reword and /or edit the figure.

8) Page 24 and Figure 4: this is the point where definitions are really required. There is no indication of the spectral range considered for the DRE results, whether these are averages of instantaneous or diurnally averaged results, etc. Please add these details.

9) Page 32: Some indication of how the cloud contamination of MODIS AOD was determined and validated would be helpful. Please add.

10) Page 33: You provide a fairly detailed discussion of aerosol-cloud interactions. Are the effects you describe really considered in all or even some of the global scale models? Please state.

11) Table 3: Please give a justification for comparing results determined for very different time periods, in particular when only 1 year worth of data is used. An indication how a given year compares to the climatological mean would help.

12) I may be missing a subtle difference, but I thought the annual avg. AOD reported in Table 4 should be identical to the annual avg. AOD in Table 6c (likewise for Table 5 and 6b). However, there are differences for the MODIS ocean AOD and MO_MI_GO over land AOD. Please explain or rectify.

13) Section 3.2.3: this section is hard to follow, because the differences in the various methods for determining DRE are only given once discrepancies are found. A more detailed description of the methods beforehand or more detail in Table 3 would help.

14) Table 9: A detail caught my eye. In zone 4 during MAM, MODIS_G derived DRE is larger than MISR_G. It is stated in the text that MISR AOD is generally larger than MODIS AOD over ocean because of MISR low-light level calibration issues. Obviously

in zone 4 during MAM, this is probably not the case. Is the reason for this anomaly the prevalence of dust at this time and location? Please comment.

15) Table 10b: it is not immediately obvious why Table 10b contains only a subset of the products in Table 10a. Please explain.

16) Table 12-17: I think the units of radiative efficiency should be Wm-2. The unit "per optical depth" is not a physical unit, but instead comes out of the normalization and hence should be made clear in the definition of "radiative efficiency". Please consider.

17) Page 47: You state that the assumptions underlying equation 1 "will introduce large uncertainties in regions where absorbing aerosols stay above clouds". I would argue that such a simplified equation will result in uncertainties in ANY situation where aerosols and clouds co-exist, regardless of their relative location or aerosol absorbing properties. Please consider rewording.

18) Table 18: with the limited description in section 3.4, some of the choices for uncertainties and their propagation are difficult to follow.

19) Section 5: I especially enjoyed reading the suggestions for future research. Putting the main suggestions into a table or creating short bullets in the text would emphasize the recommendations.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 7647, 2005.