

Interactive comment on “Estimations of Global Shortwave Direct Aerosol Radiative Effects Above Opaque Water Clouds Using a Combination of A-Train Satellite Sensors” by Meloë S. Kacenenbogen et al.

Anonymous Referee #3

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This paper presents a new estimate of the shortwave direct radiative effect attributable to aerosols above clouds. The paper builds on the previous literature by advancing a technique that applies globally and utilizes the depolarization ratio method applied to global CALIOP observations. Use of the depolarization ratio method improves upon a widespread underestimate of aerosol optical thicknesses in the standard CALIOP retrieval products, but seems to struggle to capture cases of dust over clouds. The paper does a nice job of summarizing the studies that have come before, the variety of methods that have been applied to this problem, and the problems that hinder the

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precise quantification of the global radiative effect. The paper also does a nice job of placing their quantitative results in the context of other estimates. I have only some minor comments. After addressing these, the paper should be suitable for publication in ACP.

1) The analysis is restricted to clouds that are determined to be opaque, but the method by which opaque clouds are distinguished from clouds that are not opaque is not clear. In the appendix it is noted that the “CALIOP opacity flag” is used. There should be a brief mention in the body of the paper of the physical basis for the “opacity flag”. Are there particular regimes where low clouds are prevalent but transparent? There is a vague reference to “clouds such as the ones reported in Leahy et al. (2012)”. A more specific description would be better.

2) Figure 1 indicates that the Southern Ocean is the most prominent place on the globe for uniform single layer clouds, but panel b suggests that they are not suitable for the depolarization ratio method. Perhaps it is not of great importance if most of this region has little appreciable aerosol above the cloud layer. Nevertheless, I was left wondering why. Is it a quality of the clouds? Or merely a lack of aerosol optical thickness?

3) Panel d of figure 1 shows a substantial underestimate of cases of aerosol above cloud compared to a similar statistic based on the standard CALIOP aerosol optical thickness product for continents and for oceanic regions dominated by dust plumes. This is discussed in a couple of places in the manuscript, but nevertheless I remained confused as to the cause. The only indication in the body of the paper on line 316 where it says “...filtering out of ‘unobstructed’ but potentially aerosol-contaminated OWCs.” The paper does not make clear what “obstructed” or “unobstructed” means in this context or why such clouds would be filtered. This sentence is in dire need of some plain English.

4) Another place where the description is so technical as to hide the point is in the discussion of the extinction-to-backscatter ratios in sections 3.2.2 and 3.2.3. My sense

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is that there is an important point in these sections and that differences in the probability distributions in figure 6 must be significant. But it was not clear what that point is or what the significance to the main result of the paper is.

5) Minor point: In the sentence beginning in line 308 the authors state “. . .negative (positive) values in blue (red) show the number of AAC cases that are missed (gained). . .” Way back in 2010 Prof. Robock pleaded with us to end this misuse of parentheses [Robock, A. (2010), Parentheses are (are not) for references and clarification (saving space), *Eos Trans. AGU*, 91(45), 419–419, doi:10.1029/2010EO450004]. My understanding is that one of the publishers in our field has specifically written it out of their style guide. I read pretty widely and the only genre of writing where I have experienced this application of parentheses is in the atmospheric sciences journals. I hope the authors will consider rewriting this sentence.

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