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Atmospheric Measurement Techniques Discussions

Interactive comment on "A novel method for estimating shortwave direct radiative effect of above-cloud aerosols using CALIOP and MODIS data" *by* Z. Zhang et al.

E. M. Wilcox (Referee)

eric.wilcox@dri.edu

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This paper provides a credible estimate of the radiative forcing by aerosols residing above clouds. It is a valuable contribution, which crucially provides a method that can make use of the pixel-scale observations from MODIS while providing the computational efficiency to perform global estimates. The paper should be suitable for publication after relatively minor revision.

The two other reviews have hit many of the major points that require some clarification in the paper. I will comment on two and raise one more.

Since the paper provides a quantitative estimate of the radiative effect, I feel the authors are obligated to provide a credible estimate of the uncertainty in the radiative effect. The authors are to be commended for acknowledging at least two important sources of uncertainty: the assumed independence of above cloud aerosol optical thickness from the cloud optical thickness below, and the bias error that is suspected in the aerosol optical thickness retrieval. Certainly there are others, some of which can be quantified and some that probably cannot. The best available quantitative estimates of these should be propagated through the analysis and provided with the radiative effect quantities.

One important reason to do this is so that estimates of the same quantity based on other techniques can be done. Inevitably, the values for the radiative effect will differ between the different techniques, but the uncertainties at least provide a means of determining whether the differences lie within or outside the estimated uncertainties. There are a few other published estimates and I think the authors should try to compare with them in addition to the Meyer et al. (2013) estimate. Others I am aware of include Costantino and Breon (2013), and Wilcox (2012) – there may be others. Sometimes differences in averaging and domain make a direct comparison to a published value impossible. I think in the case of my own estimate, the number I quote in the abstract of Wilcox (2012), 9.2 +/- 6.6 W mˆ-2, is comparable to the 6.63 W mˆ-2 reported on the last line of page 10004 of the current paper. So in this case the numbers differ, but only within the admittedly broad range of uncertainty I estimated in my paper.

The paper notes that it is assumed that the cloud optical thickeness beneath an aerosol layer is independent of the aerosol optical thickness above the cloud. Although I am on the record arguing that they are related (Wilcox 2010), at least over the Southeast Atlantic Ocean, I think the authors may make the assumption for the purposes of advancing the methodology. I suspect that the conditions under which above-cloud aerosol will affect cloud optical thickness through a cloud response to aerosol radiative effects is possibly a limited fraction of all cases of detectable aerosol over cloud,

although that is merely a guess – it is not clear that the assertion can be properly evaluated globally with the available data. I suggest that the authors clearly restate this assumption in the “summary and discussion” section of the paper.

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