Atmos. Chem. Phys. Discuss., 5, S1939–S1945, 2005
www.atmos-chem-phys.org/acpd/5/S1939/
European Geosciences Union

Atmospheric Chemistry and Physics Discussions

ACPD

5, S1939–S1945, 2005

Interactive Comment

Interactive comment on "Aerosol effect on the distribution of solar radiation over the clear-sky global oceans derived from four years of MODIS retrievals" *by* L. A. Remer and Y. J. Kaufman

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Received and published: 5 August 2005

In compiling data from the entire MODIS period of operation on both the Terra and Aqua satellites with respect to the question of clear-sky aerosol radiative effect (RE), this article makes a valuable contribution to the understanding of aerosol/climate interactions. As it stands, the paper is well-written, tightly focused, and succinct while, at the same time, it succeeds in providing a clear and forthright discussion of many of the important sources of uncertainty regarding both the MODIS aerosol products and the method used to calculate clear-sky RE. (An example is the discussion of cloud-contamination

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bias, and how this bias would not be revealed in the AERONET validation tests.) Nevertheless, the study has significant, correctable shortcomings, as described below.

1. The scientific issue at stake is top-of-atmosphere energy change induced by atmospheric aerosol as a result of the clear-sky forcing mechanism. This is dubbed the clear-sky radiative effect (RE) of the aerosol. An appropriate focus is the global distribution of seasonal-mean RE, as presented in Fig. 5 or the regional/seasonal means as in Fig. 6. The mode of calculation, however, omits consideration of clear-sky fraction and therefore fails to quantify the actual energy balance change. Rather, it quantifies the energy balance change per unit of clear-sky area. Since clear-sky fraction varies from region to region and season to season, Figs. 5 and 6 and, indeed, all of the quantitative results presented in this article with units of W/m2, are misleading. A prominent example is the feature seen in the Southern Ocean in Fig. 5 (DFJ and SON panels). Since clear-sky fraction is 10–20% in this region but more like 40% for the rest of the oceans, this feature is greatly exaggerated. A proper calculation of RE would take into account clear-sky fraction for each day and each 1x1 degree grid cell. That is:

$$\mathrm{RE\_local(x,y,t) = RE\_clear(x,y,t) * F\_clear(x,y,t)} \quad (1)$$

where RE_clear is the quantity currently reported in this article, F_clear is the clear-sky fraction, and (x,y,t) refers to one grid cell and one day. Maps of RE_local would be much more informative from a geophysical standpoint, and averaging RE_local to the regional/seasonal scale would be straightforward:

$$\mathrm{RE\_regional = 1/(A\_region * N\_days) * sum\{RE\_local * A\_local\}} \quad (2)$$

where A_region is the area of the region, A_local is the area of each grid cell, and N_days is the number of days over which the average is made.

The current article presents regional/seasonal means of the parameter RE_clear in Eq. (1), above – that is, the radiative effect per unit of clear-sky area. RE_clear does not represent an “effect" of the aerosol but is really a sort of efficiency parameter. It may

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have interest to aerosol scientists, but has little meaning, in and of itself, with regard to the global energy budget or climate science. In contrast, RE_regional, from Eq. (2) above, actually represents the amount of energy reflected to space by the aerosol over the region and time indicated. This average necessarily incorporates correlations between clear-sky fraction and amount of aerosol present. The regional average of RE_clear (as presented in the current article) does not consider such correlations and thus cannot be converted to RE_regional simply by applying regional-mean or global-mean clear-sky fraction.

There is no obstacle to calculating RE_regional via Eq. (2) in the current study. Clear-sky fraction is a parameter that is readily available within the family of MODIS products.

In making this comment, I fully realize that several previous satellite-based studies (Haywood et al., 1999; Boucher et al., 2000; Chou et al., 2002; Christopher et al., 2002, 2004; Loeb et al., 2002, 2004) have calculated the same quantity that is presented here – i.e. regional- and global-mean RE_clear. Thus, it is probably useful to report this quantity for the sake of comparison to those earlier studies. However, from the standpoint of climate science, it is time to take the next step, moving beyond this highly artificial quantity and presenting, in addition, the quantity that actually relates to energy balance change induced by the clear-sky aerosol effect.

2. The uncertainty analysis could be improved in several respects.

2.1 A particularly valuable part of the analysis is the comparison of results from the Terra and Aqua platforms. This comparison could be better exploited, however, by using it to formally quantify retrieval precision. Precision is an important part of uncertainty for any instrument or method. Under the assumption that optical depth and radiative effect do not vary systematically between 10:30 am and 1:30 pm, the average value of these quantities for a given region should be identical between the Terra and Aqua platforms. Any deviation from agreement is an objective measure of precision. Formal quantification (as a function of region, averaging scale, etc) would greatly

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improve the discussion of uncertainty.

2.2 There is a fundamental problem with the claim that flux estimates from MODIS are more accurate than optical depth estimates – namely, that optical depth can be validated while flux cannot. Therefore, the claim cannot be empirically tested. Moreover, it is unclear why the authors discuss validation of optical depth, if the retrieval of flux does not depend on the accuracy of optical depth. This problem could be ameliorated by comparing the MODIS flux retrieval to that of CERES (for the larger, clear-sky regions where such a comparison is possible). CERES does not measure the flux either, but it is at least an instrument designed for that purpose and one that senses the full solar spectrum and a much larger range of scattering angles than does MODIS. Adding this comparison to an independent retrieval of flux would greatly strengthen the uncertainty analysis and the paper as a whole.

2.3 The uncertainty analysis includes a calculation of RE sensitivity to uncertainty in ocean surface albedo (p. 5018). The uncertainty in ocean surface albedo is set to 0.01, but this value is never justified. In fact, ocean surface albedo varies far more than that as a function of solar zenith angle and wavelength. The appropriate value of ocean surface albedo uncertainty should be calculated and justified.

2.4 The uncertainty of converting to 24-hour average is nicely presented. I especially appreciate Fig. 4, which neatly summarizes the complexity of this problem.

2.5 The quantification of uncertainty on p.5024 makes reference to separate sources of error that are 2%, 3%, 3%, 10%, and 8%. It then claims that a quadratic summation of these errors yields 5.5%. My calculator gives 14% as the quadratic sum.

3. In several instances, the calculation method is justified by qualitative arguments, but these are not backed up by quantitative demonstrations.

3.1 p. 5011 line 7 states that using MODIS data with its 500 m resolution “reduces any unnatural bias to cloud-free conditions". High resolution data can always be converted

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to lower resolution. Thus, it should be possible to quantify this effect. How significant is it? Moreover, how much bias is likely to remain due to the fact that clouds exist at much smaller scales than 500 m? It should be possible to estimate this by looking at the much higher resolution data from the MODIS airborne simulator.

3.2 In that same paragraph on p. 5011 (and elsewhere) it is claimed that using MODIS radiances in a consistent way will produce a good flux estimate even if the intermediate quantities (optical depth, single-scatter albedo, and asymmetry parameter) are not very accurate. This is because errors in one parameter will be compensated by errors in the other parameters. It is alleged, for example, that errors in optical depth and single scatter albedo “will [completely] cancel, resulting in the correct value of derived flux, at least in the single scattering approximation." There are two problems here. First, this compensation needs to be demonstrated with a set of physical calculations. Second, the single-scatter approximation does not apply to any of the regions where substantial aerosol exists. Even at an optical depth of 0.1 (quite common), multiple scattering is important at high values of the solar zenith angle. Therefore, the argument, as it stands, is not convincing even at the qualitative level.

3.3 On line 16 of page 5014 it is stated, “Over the course of a month, MODIS views the same 1-deg square with a wide variety of view angles. This works to reduce the uncertainty in calculating flux from the individual angular observations." This makes sense, but it needs to be quantitatively demonstrated. The MODIS view angles are limited. How much reduction in uncertainty is obtained if one compares a flux estimate based on a single day to the flux estimate based on a month of observations (assuming, for the sake of the exercise, that the aerosol remains unchanged)?

4. Finally, it would be useful to present data on MODIS sample fraction. This surely affects uncertainty and, in any case, is an intrinsic characteristic of the measurement method that readers should be aware of. By sample fraction, I mean the number of valid pixels within a given domain (pixels actually used in the retrieval) divided by the total number of pixels. Further, it would be useful to separately quantify the fraction of pixels

that are discarded due to sunglint, cloud-contamination, and the retrieval procedure itself (i.e. discarding the brightest and least bright pixels).

Minor comments:

Abstract, Table 1, Fig. 3, Fig. 7, and Fig. 8 should state the wavelength of optical depth.

Fig. 4 is very useful conceptually. But it is labeled incorrectly in both the title and the caption. Figure shows ratio of instantaneous to 24-hour, not the other way around. (Instantaneous radiative effect has to be larger.)

Fig. 5 has no color bar or units.

In Eq. (3) and the surrounding discussion, the term “extinction" is incorrectly used where “optical depth" is intended. “Extinction" is the space-rate of light attenuation (with units of inverse distance) – a quantity that MODIS is not capable of measuring. In addition, the symbols “beta" and “lamda" are switched between the left side of the equation and the explanation, 2nd line below the equation.

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Interactive comment on Atmos. Chem. Phys. Discuss., 5, 5007, 2005.