Atmos. Chem. Phys. Discuss., 7, S1117–S1122, 2007
www.atmos-chem-phys-discuss.net/7/S1117/2007/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Statistical uncertainty of top of atmosphere cloud-free shortwave Aerosol Radiative Effect" *by* T. A. Jones and S. A. Christopher

Anonymous Referee #2

Received and published: 11 April 2007

This paper studies the relevance of the arithmetic mean and standard deviation used to describe a dataset of aerosol optical thicknesses or radiative effects. The authors argue that these quantities are misleading in many cases. Since the published mean and standard deviation are often used when comparing different studies, and are the numbers reported in high profile works such as the IPCC assessment reports, questioning their validity is important. However, while I agree with the points the authors make, I think the paper overstates the problem. I recommend major revisions.

General comments.

1. What is meant by "statistical uncertainty"?

This expression is not defined in the paper and I am unsure whether it is actually an uncertainty. Isn't it more a question of whether the arithmetic mean is able to adequately represent a non-normally distributed dataset? The authors put the skewness forward, yet this is an unfamiliar statistics for many scientists. Why not promote the use of the median, which is more robust than the mean for non-normal distributions? They also show probability density functions but fail to mention that it involves a binning of the data, which is often hazardous.

2. Uncertainty and variability.

In section 4.1, the authors write that "the large sample deviations [...] exceed all known uncertainties present within the data". Here, they are comparing standard deviations that do not quantify the same things. Standard deviations may measure:

- The variability within a dataset. For aerosols, the variability is large since both spatial and temporal distributions are very inhomogeneous.

- The uncertainty in a retrieved value.

The inhomogeneous aerosol distributions are due to the short aerosol lifetime. The fact that the variability is larger than the uncertainty is good news. An artificial increase in the variability due to the poor sampling of satellite retrievals would be bad news, but the test on random sampling made by the authors shows it is not the case.

3. Bias due to clear-sky-only retrievals.

I reckon this is the most important issue and I am disappointed by the authors' analysis. There is a clear positive correlation between the distributions of the shortwave radiative effect (Figure 2), its standard deviation (variability, Figure 3) and the pixel count (Figure 4) and a negative correlation with the cloud fraction (Figure 5). Is it surprising? Is it really a problem when working on clear sky only? What are the consequences when scaling the clear-sky estimates to all skies? A more in-depth analysis is really needed here, but it is complicated since aerosols can have an effect on cloud cover, reducing

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it through the semi-direct effect and increasing it through the indirect effects.

4. Inclusion or non-inclusion of missing aerosol components.

Due to the relatively short lifetime of aerosols, a given aerosol species cannot be present everywhere across the globe. The authors rightly state that one may therefore work only on those scenes where the aerosol species exists, or set its optical thickness and radiative effects to zero where it does not. Obviously, the resulting statistics are very different, since the datasets are different. Again, the authors make that sound like a problem before finally writing that "the matter really depends on what one is trying to show" (section 3.5). Indeed. There is no problem there and I fail to see why it takes so long for the authors to reach that conclusion. When working on a given aerosol species, only include those scenes where that species is present. When comparing different aerosol species, make sure the area taken into account in the study is the same for all species by adding zeros - and those zeros are physically meaningful.

5. Structure of the paper.

My opinion is that sections 3 and 4 should be merged, in order to go straight to the results and avoid unnecessary repetitions of the methods.

Specific comments

Introduction, third paragraph: Christopher and Zhang (2002) is cited as (2004).

Same paragraph: "the number of pixels can vary from a single pixel to over a thousand". Shouldn't a threshold on the pixel count be defined, in order to ensure that the grid cell average is representative of the aerosol variability? Could the authors offer guidance on the choice of that threshold?

Introduction, fifth paragraph: "mathematical limitations result in cases where one or two aerosol types may not exist" and "this is perfectly reasonable". There are no limitations, then, since in reality some aerosol types may not be present everywhere.

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Section 2.1, second paragraph: “clear-sky is defined as cloud and aerosol-free regions” followed by a criterion on cloud cover only. How is it ensured that those regions are also aerosol-free?

Section 3.1, last paragraph: “An aerosol pixel of a particular type with a certain reported AOT should have the same effect [...] no matter when or where the aerosols are located, assuming a uniform background such as the ocean.” This is not true. The direct effect has a strong dependence in the solar zenith angle (which depends on the location, day of the year and hour in the day). Typically, the same component aerosol loading will have a larger direct effect at mid-latitudes than in the Tropics. The ocean reflectance is also very dependent on the solar zenith angle. See sections 3.4 and 3.5 of Boucher et al. (1998).

Section 3.3., first paragraph: The authors write that gridding the data forces a “spatially homogeneous dataset”. I can’t see how. The pixel count will still vary between grid boxes. Am I misunderstanding the authors’ definition of “spatially homogeneous”?

Section 3.4: Instead of taking only a small, fixed number of pixels to create the grid-box averages, couldn’t the authors take a fixed percentage of pixels? They would avoid the increase in variability for cells made out of many pixels.

Section 4.1, first paragraph: “AOT are biased towards smaller values”. It is not really a bias in the statistical sense, since the real AOT distribution is very likely to include more small AOTs than very large one. This is because those events associated with very large AOTs (some mineral dust and biomass-burning events, typically) happen less often and span a smaller area than the background optical thickness of sea-salt, for example. It is also important to state that those smaller optical thicknesses are associated with larger retrieval uncertainties.

Same paragraph: It resembles a “Gamma distribution” with a small shape parameter.

Section 4.1, second paragraph: “Figure 2 shows gridded data”. I’m confused: Gridded

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data will only be studied in section 4.2.

Same paragraph: “globally-averaged MODIS cloud fraction for all (clear and cloudy) data”. The cloud fraction is obviously given for all scenes. They are classified as clear or cloudy using that cloud fraction.

Section 4.1, last paragraph: When looking at Figure 7, it is hard to tell whether a linear relationship exists between AOT and SWRE. Plotting the point density would show whether most of the data points are indeed along the fitting line.

Section 4.2, last paragraph: “Still, it is the latter that best agrees with previous research”. That does not prove that the other value is wrong.

Section 4.3, last paragraph: It is very unclear. Do the authors mean that the Kaufman et al. (2005) method is not internally consistent? How exactly?

References: Bellouin et al. (2005), Fan et al. (2005a), Li et al. (2004) are referenced but not cited. Kaufman et al. (2005) is cited throughout the paper as 2005, 2005a or 2005b. Which of the two Zhang et al. (2005) is 2005a?

Figure 1, 6, 8 and 9: What do the numbers and the vertical lines represent? They do not seem to match.

Figure 1: The caption should state that symbols are used for the PDF and the solid line represents the ideal normal distribution.

Figure 10: In the text we are told the figure shows the distribution of the anthropogenic optical thickness, and there is indeed one plot. However, the caption reads “Dust and anthropogenic component AOT and SWRE”?

References:

Boucher, O. et al. Intercomparison of models representing direct shortwave radiative forcing by sulfate aerosols. J. Geophys. Res., 103, D14, 16979-16998, 1998.

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Interactive comment on Atmos. Chem. Phys. Discuss., 7, 3557, 2007.

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