

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

T. A. Jones and S. A. Christopher

Received and published: 17 May 2007

Overall comments: The manuscript “Statistical uncertainty of top of atmosphere cloud-free shortwave aerosol radiative effect” by Jones and Christopher discusses the statistical uncertainty that is related to datasets of aerosol optical thickness and shortwave (SW) aerosol radiative effect. In their analysis the authors question the validity of the arithmetic mean and the standard deviations that are widely used as a reference when comparing different studies and they mention the importance of evaluating additional statistical measures in describing global data sets. The manuscript is quite well written, although repetitions between sections 3 and 4 exist. Overall, the manuscript is publishable in ACP if a more in depth discussion regarding the clear-sky retrievals and their biases will be included.

We thank the reviewer for his comments concerning this manuscript and have used them as a guide to improve its overall quality. Section 4 has been revised to remove some overlap from Section 3. Also some additional discussion on the ramifications of the use of only clear-sky data.

Detailed comments: 1. A significant amount of discussion is spent on the “statistical uncertainty”; however this is never defined. A detailed description of how exactly they define this phrase would be very useful.

The term “statistical uncertainty” has been replaced with the more fitting term “statistical variability”. We define statistical variability as the robustness of globally averaged statistics relative to data distribution. This is now stated explicitly in the abstract.

2. Page 3560 - line 4: It is not clear to me what the authors mean when they say that: “...gridding ensures a uniform distribution”.

We meant to imply that gridding produces a data set in which each grid cell carries uniform weight in space and time. As a result, statistics calculated from gridded data are not as regionally biased compared to using raw data. We do note and analyze the underlying issue that the number and distribution of data within each grid-cell can vary wildly.

3. Page 3560 - line 28: The authors state that “...mathematical limitations in this algorithm result in cases where one or two aerosol types may not exist.” This has nothing to do with mathematical limitations but with reasonable representation of the reality. Please elaborate.

Under some circumstances, the Kaufman algorithm can produce negative values for either dust or anthropogenic AOT, which is clearly a non-physical result. Thus, the assumption is made that all negative AOT values are equal to 0. This point has been clarified in the manuscript.

4. Page 3562 - line 6: The authors state “Clear-sky is defined as cloud and aerosol

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free regions...” that are determined by the cloud fractions. I do not understand how the cloud fraction implies that these regions are aerosol-free. Please clarify.

The CERES-SSF product includes the MODIS cloud fraction parameter which indicates the proportion of a MODIS pixel covered by clouds. To remove the effects of clouds from SWRE estimations, we use only data where cloud fraction is less than 1%. This is now discussed in greater detail in the text.

5. Page 3565 - lines 2-5. I strongly disagree with the authors' statement. It is well known that the direct effect of aerosols on SW flux, even of the same species, depends on the solar zenith angle. Please revise appropriately.

We regret this misstatement and have revised accordingly. We have also added a brief discussion of the observed variability in efficiency from other research conducted by the authors.

6. Page 3565 - line 19: Could you please clarify what do you mean by “ To overcome the problem of spatial inhomogeneities, raw data pixels binned into a uniform grid ”?

7. Page 3565 - lines 23-24: It is not clear to me what the authors mean by saying that: “... it forces a spatially homogeneous dataset from data that was previously nonuniformly distributed.”

We were trying to indicate that gridding the data, at least on the surface, removes the large spatial biases present in the raw data. We do realize that each grid-cell is made up of a different number of grid-cells and therefore each has a different statistical meaning. This is discussed further in the text.

8. Section 3.5: I don't really understand where the problem with the inclusion or not of missing aerosol components is. Indeed, inclusion or not of such components will result in different statistics. However, this decision is a strong function of what one is trying to find, as also pointed out by the authors. I think this section overstates the significance of such decisions. Please elaborate.

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The discussion concerning missing aerosol components has been shorted to some degree, but remains a key point of the results. One of the primary reasons this issue is discussed in such detail is that many other works report anthropogenic (or dust) statistics without mentioning which assumption they use. As a result, we decided to quantify its importance as a guide to future researchers.

9. Page 3569, section 4.1: Since the analysis of the gridded data is done in section 4.2 why do the authors refer to gridded data in section 4.1?

The discussion of Figures 4-7 has been moved to the “Gridded Data” subsection

10. How do figures 2-5 compare to each other? A more detailed discussion is very important to be included here.

Figures 2-5 are now Figures 4-7 and additional discussion concerning them, especially in terms of the implications of using clear-sky data, are now present in the text.

11. Page 3569, line 14: The authors state “...MODIS cloud fraction for all (clear and cloudy) data”. Please clarify.

The MODIS cloud fraction plot was created using all available MODIS data, not just clear-sky data. Data used for this figure did not have a cloud fraction threshold applied. This is now stated explicitly.

12. Section 4.3 - last paragraph: It is really hard to understand what the authors mean. Please elaborate.

This paragraph has been completely rewritten to improve clarity.

Minor comments: 1. Page 3559, line 25 - ...“Christopher and Zhang (2002)” rather than ... “Christopher and Zhang (2004)”. Done 2. Page 3560, line 18 and elsewhere - Kaufman et al., 2005 is referenced also as Kaufman et al., 2005a and Kaufman et al., 2005b. Please make it consistent. Done 3. Please provide references for Bellouin et al. (2005), Fan et al. (2005a) and Li et al. (2004). Done

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4. Figures 1, 6-9: What do the vertical lines and the values noted on each plot represent?

Vertical lines represent mean values for the respective distributions. This is now stated in the figure captions

5. Figure 1 - ...“Probability density functions (open symbols) of non-gridded, global AOT (a) and SWRE (b) with idealized Gaussian (normal) distributions (continuous line) overplotted” ... rather than ...“Probability density functions of non-gridded, global AOT (a) and SWRE (b) with idealized Gaussian (normal) distributions overplotted”.

Figure 1 caption revised.

6. Figure 10 - Please make the text consistent with the figure.

Values in figure now reflect those in the text. This error was introduced during the initial revision when new figure were made from a data set using an improved Fclr definition.

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

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