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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A critical look at spatial scale choices in satellite-based aerosol indirect effect studies" *by* B. S. Grandey and P. Stier

Anonymous Referee #1

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The paper deals with a very important question related to cloud aerosol analysis form satellite data. The authors seek to know what is the correct scale for a joint analysis of aerosol and clouds' properties. On one hand in order to see if there is significant causality reflected by significant trends over the large natural variability, one would try to analyze datasets that are as large as possible and to increase the samples number. For that, one has to use long time series and preferably large spatial domains. On the other hand meteorology is a key player in the cloud-aerosol system and natural climatological trends (meteorology) may create apparent relations between aerosol and clouds that are not based on "cause and effect". The authors compare two methods of trend analysis, one by using all the data in the box (region), collecting all times and all pixels together, and one that calculates regression (slope) for each pixel separately using the pixel's time series and then average the slopes (weighted by their significance) in order

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to estimate the regional one. They compare the two methods and show how the results deviate as the box size gets larger. I think that the ideas presented in the paper are important and should be explored but I miss serious discussion on few key topics and found few inaccurate descriptions of references.

Specifically: 1) The problem of meteorology vs. true aerosol effect is known to be important for quite a while. There are several approaches to tackle this problem. The approach that is described in this paper refers to gradients. If clouds and aerosols have significant gradients in their averages (meteorology) these gradients may create apparent correlations driven only by these gradients. If we will take as an example the Atlantic subtropics off the Saharan coast, we expect the aerosol loading to be higher near the African cost than downwind in the ocean. Gradients in cloud properties are also expected to occur as a function of the distance to Africa, as upwelling cold water in the east part defines the distribution of the marine stratocumulus. As the SST gets warmer and with the increase in the MBL height westwards the clouds gradually transform from close cells to open ones and later to sparse cumulus clouds near the American coast. This is all meteorology, but scatter plot between aerosol optical depth and cloud properties will show trends. In order to decouple meteorology form aerosol effect there were many attempts to slice the data per given meteorological conditions. Figure 2. In Kaufman et al, (2005) shows clouds' properties as a function of the distance from the African coast. The meteorological effect is shown as the overall trend for all data groups while the aerosol effect is shown as the difference between the polluted to the clean data per longitude. Moreover great part of Kaufman (2005) is devoted to estimation of the true aerosol effect vs. meteorology by using reanalysis model data as a proxy for meteorology. In Koren et al (2005) they first classified the data according to cloud types and focused on convective ones only. Then the convective clouds data was sliced by clouds' height and they selected modeled updrafts as a proxy for convergence vs. divergence conditions. The cloud aerosol analysis was for each subgroup. Sometimes, to minimize meteorological trends, areas with small meteorological variance are chosen. For example the dry season over the central Amazon (away from the shore and the Andes) exhibits very small changes in the day to day atmospheric conditions (profiles) (This was the case in the cited Koren et al, 2008 and in many other papers). Restricting the meteorological conditions using modeled (or measured) atmospheric properties (such as AIRS measurements) has advantages because it can classify meteorological conditions even in cases where there are no significant trends in the averages. It adds information and allows classification in smaller scales in time and space. The authors do not discuss such attempts to reduce the meteorological variance and to classify the data according to meteorological conditions. Nor they do not talk about selection of times and areas with no significant meteorological trends. Therefore some of the references are presented in a wrong way. In the revised version they should add the discussion (in the context of the relevant cited references).

2) By taking 10 years of data for all seasons (most of the paper's figures) the authors do in time what they do not recommend to do in space. In theory seasonality may create very strong apparent correlations between clouds and aerosols. The authors should base their core analysis on shorter times within a given season.

3) Using shorter time intervals will reduce the significance of the pixel by pixel analysis dramatically. In such case most of the pixels may exhibit insignificant trends. In such case collecting data from larger boxes will be essential. And the discussion of how to reduce the meteorological variance mentioned in (1) will be relevant.

4) Following the previous points, the paper misses serious discussion on the one pixel analysis option. Is it always better? What are the weaknesses? Does the (weighted) average of the means of the trends reflect in the right way the regional trend? The authors compare the trends of the gridded analysis to the regional one assuming that the gridded is the correct one. Is it always true? Intuitively in places where there are no significant joined gradients, analysis of larger box should reflect the trends better.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 15417, 2010.