

Interactive
Comment

Interactive comment on “Application of spectral analysis techniques to the intercomparison of aerosol data – Part 4: Combined maximum covariance analysis to bridge the gap between multi-sensor satellite retrievals and ground-based measurements” by J. Li et al.

J. Li et al.

jl2862@columbia.edu

Received and published: 29 May 2014

We thank the reviewer for raising many critical questions and offering helpful suggestions to improve the manuscript. We respond to the point-by-point comments below and have revised the manuscript accordingly.

General comments: This paper demonstrates an effective way of analyzing the com-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



bined data sets of AOD data field from multi satellite sensors (i.e., MODIS, MISR, SeaWiFS, and OMI) and AERONET ground observations simultaneously for studying spatio temporal variations. This is well written and seems to be a first attempt (to my best knowledge) to look at multiple AOD data sets using PCA and SVD techniques, which addresses a relevant topic for the journal of AMT. However, this manuscript needs further clarifications and explanations to be a separate 4th paper discernable from the previous three papers which also analyzed the same topics with similar techniques (i.e, PCA, MCA, and CPCA). A fundamental question is why we need this technique (Combined Maximum Covariance Analysis: CMCA) to analyze the spatio- temporal variations of AOD, if we can achieve nearly same results from other previous techniques. Another comment is that the authors tend to overemphasize the advantages of these techniques and not mention limits of those at all. Pros and cons of methods should be well balanced and documented for user community for future applications.

Thank you for the comment. In the introduction, we expanded the discussion of the reason to use the CMCA technique, and its advantage over the previous techniques. While the results seem quite similar to those from the previous paper, CMCA is much more efficient. MCA only allows comparison between two datasets. Therefore, if we are faced with multiple satellite observations and the ground truth, we must first perform MCA separately for each dataset. However, the comparison across satellite datasets will become difficult. On the other hand, CPCA achieves parallel comparison of multiple datasets. However, we cannot simultaneously evaluate the results for different datasets, especially places with disagreements because CPCA does not apply to the more scattered station data used as ground-truth. We also added a discussion of the shortcoming or limitation of this technique in the end of Section 3, although some of these limitations apply to spectral decomposition techniques in general.

Specific comments: Page 3503, Title: reconsider the change of title and be shortened. I disagree that the content of this paper is enough to support that the CMCA technique is able to bridge (and explain) the gap between satellites and AERONET observations.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

We have changed the title to “Synthesized Analysis of Multi-sensor Satellite and Ground-based AOD Measurements using Combined Maximum Covariance Analysis”. In our original title the phrase “bridging the gap” was meant to emphasize that this spectral analysis technique allows us to incorporate both multi-sensor satellite datasets and ground based measurements into a single spectral analysis and comparison, rather than compare them separately. We apologize for the confusion.

Page 3504, lines 1-14: these descriptions might be better fit in introduction than in abstract. Overall, this abstract is lack of specific results and conclusions.

The abstract has been revised. The introductory part has been compressed and more results were added.

Page 3505, line 21: put the acronym (OMI), same as those for other instruments.

Corrected.

Page 3507, line14: clarify the Angstrom relationship. Did you use the Angstrom exponent parameter from MODIS, MISR, and SeaWiFS to obtain the 500 AOD or use linear interpolation as described in Page 3509, lines 16 and 28?

We changed it to “Angstrom power law” as suggested by the other reviewer and added a description that it is a linear interpolation on logarithm scale.

Page 3511, line 9: list wavelengths (two UV and five visible channels).

The wavelengths of the UV and visible channels have been added.

Page 3512, lines 13-19, Verify this technique by omitting two or three month data from a few selected sites having a full data record and reconstructing a full time series for comparison. These results should be shown in Figure 3.

Thanks for the suggestion. We revised this section by conducting a cross validation of the interpolation using 8 stations with less than two missing data. The results of the cross validation is summarized in numerical form and the experiment using time

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

series at IER_Cinzana is shown instead in Figure 3. Of the 8 stations, this stations has relatively high variability and the results for the other stations are better or at least comparable.

Page 3513, line 5 and section 3.2: need more literature review and add more references on these techniques, not only the author's two papers listed here but also other references for other applications because these techniques are commonly used in many fields to deal with multivariate data sets. And also discuss thoroughly advantages and disadvantages (or limits) of these techniques.

Thanks for the comment. We added more references to the previous application of these methods. However, our previous papers are the only ones that use these methods in the intercomparison of multiple datasets. We discussed the disadvantage of these methods at the end of Section 3.

Page 3513, lines 14-15: might be helpful to put actual numbers of dimensions (e.g., $n = 6 \text{ years} \times 12 \text{ months} = 72$, $m = 360 \times 180 \text{ grid cells} = 64800$ for each satellite; $m = 58$ AERONET locations).

The numbers of the dimensions have been added.

Page 3514, lines 4-7: the assumption of “equal weight” for each AOD data set mapped to the same spatial resolution (1 x 1 degree) for this analysis may not be adequate, (though mathematically enough), especially for monthly AERONET gridded data. Does it have representativeness in space and time to be comparable to those from satellites? We have been observing some discrepancies between satellite and AERONET point measurements over some locations even with daily matchups. As the spatial and temporal window of AERONET increases, the importance of AERONET as a ground truth will become lesser in the comparison with satellites. Different sampling issues among satellites should be also discussed how they can affect the covariance and results.

The “equal weight” assumption only applies to the fields that are being combined, i.e.,

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

the satellite data fields. This part of the text (page 3514, line 4-7) only describes how the satellite data fields are combined, i.e., only X_MODIS, X_MISR, X_OMI and X_SeaWiFS are combined into the large X_sat matrix (equation 2). This assumption is not required for AERONET, as SVD does not have such assumption for the two data fields. With respect to different sampling issues, if every sensor is measuring the truth then they should have the same spatial and temporal variability and produce the same spatial and temporal modes, because they are measuring the same quantity. The difference in sampling does not affect covariance matrix but will lead to differences in the spatial modes. These differences are exactly what we are expecting and looking for using the CMCA technique. For example, the insufficient representation of the Russian fire by SeaWiFS and OMI due to cloud screening or row anomaly. We added a discussion to the end of this paragraph about the sampling issue.

Page 3514, line 10 and 13: equation (3) and (4), also useful put actual numbers of dimensions.

The numbers of the dimensions have been added.

Page 3516, lines 7-9: Remove (“we choose not to dive into...), not necessary for results. Removed.

Page 3516, lines 15-17: provide a summary of the previous studies and highlight conclusions; discuss clearly advantages/disadvantages of the previous techniques compared to CMCA. Justify the need of CMCA with results.

A summary of previous studies has been added here. The advantage of the CMCA technique over the previous techniques was further discussed at the end of Section 4.2.

Page 3518, line 16: typo (“anomly”).
Corrected.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Page 3519, lines 2-4: rewording; I believe that direct comparisons (satellites vs ground observations) are the most reliable approach to understand the sources and types of aerosols in space and time with prior knowledge and information on those.

This sentence has been changed to “It is also an efficient way to provide insights into possible into possible problems and highlight regions with the most uncertainty.” What we originally meant is that CMCA is an efficient way to locate the agreement and disagreement between datasets and to identify the most uncertain regions, and to suggest possible causes through the analysis of the spatial distribution and temporal variability revealed through the analysis (e.g., whether it is related to dust or biomass burning, and whether it results from seasonal variability or events). This provides a focus for further examination using direct comparison. However, without any prior knowledge or the decomposition results, it would require direct comparison for all regions and all time periods to locate disagreements between the datasets.

Page 3520, lines 5 – 10: these are true for OMI and relevant to mention here. These two factors (i.e, crude cloud screening scheme due to a large footprint at nadir of 13 x 24 km² and row anomaly issue) are associated with instrumental design and issues and cannot be much improved by upgrading the OMI algorithm. Therefore, it is not necessary to state and emphasize this in the caption of Figure 12.

Agreed. It has been deleted from Figure 12 caption.

Page 3520, line 23: typo (“Genetic”).

Corrected.

Page 3521, lines 1-8: difficult to discern the colors (blue or green?) and magnitudes of two dots in the mode 1 over this region; same for the mode 2. Reconsider a way of presenting these to support discussions by adding a separate table or line plot.

The numerical value of the magnitude for the signals for these two dots (Kanpur and Gandhi College) have been added to the figure caption. Gandhi College has a stronger

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

signal in Mode 1 but weaker in Mode 2 than Kanpur, although the differences are not much which results in their close appearance.

Page 3521, line 7 and 15: rewording, (“problematic” and “problems” in satellite sensors); “difference” found in this analysis does not necessarily mean a sensor is wrong or problematic. Differences can be found for many reasons.

The “problematic” does not refer to the sensors but to the dataset itself, i.e., the seasonality not well represented in the datasets. We have modified it to “may not be correctly represented by”.

Page 3522, line 23: the techniques (i.e., PCA and SVD) in this study are widely used in many applications and it’s difficult to say it is a “new” technique.

Here we mean the CMCA technique is new. We agree that the PCA and SVD are not new and the CMCA is a further step of development. We changed it to “improved”.

Page 3523, lines 1-3: explain specifically what useful insights into the underlying physics of the problem can be obtained from this analysis. I disagree that this kind of data analysis technique (i.e., eigen analysis with the covariance) can provide it.

Here we mean that the comparison is not purely statistical but could have physical meanings. For example, many regions identified by the decomposition represent major aerosol source regions (e.g., South America, North Africa, etc.), therefore, disagreements over these places are likely attributed to the capability of a datasets to characterize these aerosols. We changed this phrase to “which associates the comparison to real physical phenomena.

Page 3524, line 8: typo (“in accurate”), should be one word (inaccurate).

Corrected.

Page 3533, Figure 1: add NDVI time series to confirm the seasonality of each plot. Why are there gaps for the plot of Bratt_Lake site?

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

The NDVI time series is added by the side of each AOD plot. We examined the winter gaps in the Bratts Lake time series and found that in most Januaries, neither DT or DB has retrieval here, likely due to polar night, as the latitude of this station is high (50N). In practice we tried to fill the gap using nearby grids as much as possible, although a few grids still have missing data and are thus removed (see MODIS data mask in Figure 4).

Page 3535, Figure 3: add more plots before and after interpolation over some sites as suggested in Page 3512, lines 13-19.

We completely revised this discussion by replacing the original figure with experiments using IER_Cinzana site which has a full data record. The cross validation results are shown in the text as numerical values. Overall, the interpolation at one or two data gaps only results in less than 4% uncertainty compared with the variance of the original time series. The third mode of the anomaly dataset accounts for 7% of the variance, therefore we believe this 4% should not have much impact on the dominant modes. The IER_Cinzana site shown here has relatively high variability, and the results for those with lower variability, such as GSFC, are much better. Although the interpolation is still not perfect, it performs better than straight interpolation on the time series with the seasonal cycle left in.

Page 3539, Figure 7: Why did you put the triangle marks (AERONET sites) on the plot? If not necessary in this Figure, remove them.

The reason that we included the triangle marks for the AERONET stations sites on Figure 7 is to emphasize for the discussion that some places with high discrepancy between the satellite datasets do not have qualified AERONET data, such as East Asia and Borneo, and that these places should be the focus of ground based site deployment.

Page 3543, 3544, Figure 11 & 12: those intense wildfires of unusually large scale in Russia cannot be missed by any satellite instruments. In Fig 11 &12, weak signals

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

from SeaWiFS and OMI should be most likely due to cloud screening schemes in the process of AOD retrievals. In fact, OMI aerosol index maps clearly show those events in August, 2010 even though some missing data observed due to the row anomaly (refer to: Witte, J. C et al.,: NASA A-Train and Terra observations of the 2010 Russian wildfires, Atmos. Chem. Phys., 11, 9287–9301 doi:10.5194/acp-11-9287-2011, 2011). In particular, OMI with a large footprint can be quickly contaminated by cloudy scenes of thick smoke plumes and difficult to retrieve reliable AOD under those conditions. The authors do not have to describe all the details on the captions of Figure 11 and 12. However, it is not necessary to point out that SeaWiFS and OMI do not capture this event well in the captions, neither.

We apologize for the inappropriate wording. As noted by the other reviewer, the Russian fire event was not missed by SeaWiFS and OMI. They are just not as evident in the SeaWiFS and OMI datasets as in the MODIS and MISR datasets. We revised the caption for Figure 11, to “ the distribution of the fire signal in SeaWiFS and OMI is not as extensive as MODIS and MISR. And in Figure 12 caption, the detailed description has been removed.

Page 3547, Figure 15: these are the most critical results to confirm/support discussions on Figure 13 and 14. Should include comparisons for all four stations (or at least two stations over the Gangetic region). I also would like to look at similar time series plots but using the grand mean of the five instruments as a proxy of the truth (x-axis) instead of the monthly mean of AERONET. The reason is that SeaWiFS shows some missing data during the summer months and OMI has significantly reduced samples due to the row anomaly issue since 2008, and MISR has lesser samples than those of OMI and MODIS due to a narrower swath, and AERONET monthly AOD at such a coarse resolution of 1 x 1 degree grid cell may not be representative for comparison with those of satellites. Under those tricky conditions, the grand mean of all instruments might be more reliable as a “reference” than that of any other single instrument. In addition, it's also interesting to look at other regions such as the Sahel demonstrating a large

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

uncertainty in the spread maps with at least 3-4 AERONET stations (Figures 7 & 10). Why are the authors asking readers to look into other interesting regions with these techniques (Page 3523, lines 26-28) without showing results you can do easily here?

We added comparison for the Gandhi College station. The agreement for Thar Desert is good, and the AERONET measurement at Pune is not complete for the first three years (this station is added manually to account for full spatial variability). We also added the grand mean of all five datasets and compared each satellite time series against it. See detailed discussion in the second last paragraph of Section 4.3.

In Li et al. (2014b), we examined the Sahel region and a few other regions that were not presented in this study. The results were mentioned from Page 3517 line 27 to Page 3518 line 2 of the discussion paper. In this study, we have tried not to repeat the previous analysis but instead emphasize new results such as Russia and India. However, we do believe there will be other interesting regions specific to some researchers but we may not realize. Those are the places that we encourage the readers to explore further. Overall, the main purpose of the paper is to introduce and demonstrate this new technique with a few typical examples, rather than a thorough data validation.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 3503, 2014.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)