Answers to anonymous Referee #1 comments, received and published on 24 February 2015, on the manuscript:

"Using the OMI Aerosol Index and Absorption Aerosol Optical Depth to evaluate the NASA MERRA Aerosol Reanalysis"

We thank the reviewers for providing comments that helped to improve the quality of the paper. The detailed responses to comments are listed below (text in black shows comments from the reviewers, and the text in blue is our answer):

General comments:

In this interesting paper, the authors present the validation of their aerosol data assimilation result with OMI aerosol products and other independent aerosol observations. The instrument OMI was launched with the aim of measuring the ozone layer, so that its aerosol products have been treated as secondary products. It is also partly due to the physical difficulty in interpreting the OMI aerosol products. The authors attempted the novel and challenging validation in this paper. Their attempt should be appreciated whether or not the attempt became completely successful. Actually, their attempt was successful in Saharan dust validation and unsuccessful in smoke and sulfate validation. The overall presentation of this paper is well structured and mostly clear except for the classification of figures between "in the main body" and "in the supplement". The manuscript is worth being published in ACP after some minor revisions.

Specific comments:

1) page 32188 line 1: The monthly mean distribution is shown in Fig. 1, but the time (month and year) is not specified in either the main body or the figure caption. Even if it is an example, the time should be indicated.

The time "July 2007" will be indicated in the main body and in the figure caption.

2) page 32189 lines 20-24: No wonder the simulated AOD has a very good correlation with the assimilated observations MODIS NNR. It is meaningless to compare "the correlation with assimilated observations" and "the correlation with independent observations". The really-required scores are "simulation with data assimilation versus simulation without data assimilation", "analysis versus forecast", or "analysis versus independent observations".

Here we compare MERRAero and MODIS NNR (the assimilated observations) after first interpolating the model grid box to the observation location; in this way, we are able to assess representativeness error. We do, however, agree that providing the correlation between "simulation with data assimilation versus simulation without data assimilation" would add additional information in the paper. However, we do not have such an experiment available at this time to perform this analysis. We do include the comparison

between MERRAero and MISR, an independent, non-assimilated observation, as an example of "analysis versus independent observations."

3) page 32192 lines 5-7: The refractive index at 354 nm is modified here but the imaginary part value is not specified in the text. The value should be indicated to show the test-retest reliability of this experiment.

A value of the imaginary part of the refractive index at 354 nm will be added to the text.
P 32192 lines 5-7: "Using the observation-based dust optics, we now try to improve the AI comparison by increasing the imaginary part of the refractive index at 354 nm (changed from 0.0053 to 0.007) while keeping the refractive index at 388 nm constant (0.005)

3) page 32193 lines 22-24: The same as comment #2.

Same as comment #2.

4) page 32194 lines 23-25: This presentation is very confusing. The authors are ignoring marine layer aerosols but it is not described in the main body text. Without the description, "the maximum attenuated backscatter coefficient" seems to appear in the marine layer.

This part of the text will be modified in the new manuscript.
Page 32194 lines 23-25: "The maximum attenuated backscatter coefficient in the MERRAero smoke plume is shifted during the daytime, peaking between 5E and 12E while CALIOP maximum values for smoke are between 25E and 30E. During the night, the maximum attenuated backscatter coefficient for smoke occurs for both CALIOP and MERRAero over the continent between 15—25E. The GEOS-5 smoke plume is displaced to the west of the continent, descending gradually over the near-surface marine aerosol layer from 15 to 0E. In contrast, elevated aerosols are not found in the CALIOP profiles west of 12E."

5) page 32197 lines 3-9 and page 32201 line 16: Torres (2011) indicated that OMI AOD is good when AAE is between 2.5 and 3.0. However, what the authors showed here is that MERRAero AI is good compared to OMI AI when all of the OC SSA is modified to always yield its AAE between 2.5-4.0. Therefore, this sensitivity test showed only that the default optical property of OC was not appropriate, didn't it? Plus, Fig. 13 indicates that the model bias is resolved (= the scatter distribution is just parallel-shifted) but the broad scattering remains unchanged. Is this an improvement?

In the UV, BC has an AAE ~ 1 and OC has an AAE > 1. In the OMAERUV algorithm, biomass-burning aerosols (which include BC and OC) have an assumed spectral absorption contrast between 354 and 388 nm. Assuming this spectral contrast for biomass burning aerosols, they have found better agreement between OMI AOD and AERONET AAE.

In MERRAero, BC and OC are two separate tracers with their own fixed set of optical properties. When we constrain the total column of aerosol in MERRAero during the assimilation process, the aerosol mass is adjusted using the assumed aerosol optical properties for each tracer. Thus, our aerosol masses in MERRAero were constrained using our original optical properties for OC and BC, which are derived from OPAC. Now, having observationally constrained our aerosol mass distributions, the goal of the current analysis is to make comparisons with observation (for this case OMI AI) in order to revise/improve our optical assumptions if needed. Indeed, we have shown that by considering spectral aerosol absorption for OC in UV, we get a better agreement with OMI-AI, suggesting that our optics table for OC should be revised in the future in UV.

In Fig.13, we think that an increase of the correlation coefficient from 0.09 (baseline simulation) to 0.65 (updated simulation), associated with a decrease of the bias (-0.55 to 0.08) and the STDV (0.55 to 0.48) is certainly improvement.

6) page 32200 lines 14-15: The authors say that the MERRAero AI is reasonable. Yes, it is good in Saharan dust region. But, it is bad in smoke and sulfate regions. Please describe them honestly.

This sentence will be extended in the new manuscript as follows: "Baseline monthly mean comparisons showed that MERRAero simulated AI were reasonable compared with OMI AI with better agreement over the Saharan dust region than over the biomass burning region in southern Africa."

7) page 32200 line 26: I believe that the phrase "particularly over dust sources" is not well discussed in the paper.

This part of the sentence will be removed in the new manuscript.

8) page 32201 lines 11 and 20: Not only SSA but also AI is largely scattered.

AI does indeed have scatter, however the correlation between observations and MERRAero is high compared to the correlation between SSA observations and MERRAero. AI is sensitive to aerosol properties such as aerosol height, aerosol absorption … The AI scatter is likely due differences in optical properties, particularly if they are a function of relative humidity. In converse, SSA is sensitive to aerosol properties such as aerosol size and mixing state; the GOCART model is unable to simulate the observed variability in SSA because it only considers external mixtures and assumes dry particle sizes for each aerosol type. This is why the SSA comparison in Figure S1, for example, appears quite flat along the abscissa.

9) Figs. 1, 2, 3: In these figures, the color of lands is green, blue, or red completely. But oceans are not colored mostly. This means that all of the land areas are covered with observation data and oceans are mostly full with no-data. Is the contrast of OMI data so strong?

This lack of data over ocean is due to the criteria "quality flag = 0" chosen in our simulations to avoid cloud contamination.

10) Figs. 6 and 11: The expression of longitude is "plus and minus" in these figures. It is confusing because they are mentioned with "W and E" in the main text and the other figures. The expression should be unified.

These figures will be updated with "W and E" in the new manuscript.

11) Supplement: I have no idea what is the authors' criterion to distinguish supplement figures from main body figures. Of course, this manuscript contains a lot of figures in the main body (I believe the number of figures can be reduced somewhat. . .), but Supplement should be only additional or serviceable information.

We agree that there are a lot of figures in the main text, however we believe that they are necessary to thoroughly illustrate the skill of MERRAero. We believe that the supplemental figures provide additional information for the reader to evaluate the skill of MERRAero, and we decided to put them in a supplement so as to not increase the number of paper figures further.

Technical corrections:

1) page 32194 line 26: The longitude 15-25W is right? It isn't 15-25E?
Correct, thank you.

2) page 32200 line 14: ". . . in the process fine tune the aerosol optical properties. . ." is an error in grammar? I'm not sure because I'm not a native speaker. But I think "tunes" is grammatically correct.
Thank you. We have corrected this to:
"This paper uses independent observations to validate these aerosol diagnostics, and in the process we fine tune the aerosol optical properties assumed in the model.

3): The authors are using two terms "South Africa" and "southern Africa". If they are intended as the same meaning, the expression should be unified.

Thanks; we've unified this to "southern Africa" in the new manuscript.