

We thank all three reviewers and the editor for their useful and constructive comments and suggestions concerning our manuscript, # acp-2012-166. Below, we detail how we have addressed these comments in the revised version of the manuscript. Reviewer/editor comments are in italics with blue color, and our response in black regular type.

#### Responses to review#1

*"I recommend that the paper is accepted as is."*

Thanks very much.

#### Responses to review#2

##### 1 Main comments

*"As listed by the authors in the introduction, change in sampling can affect the trend analysis. In section 4.2, we are told of changes in sampling affecting AVHRR trends. Is SeaWiFS sampling of optical depth stable? Are there no regional trends in clear-sky pixel counts over the period?"*

In order to address the question as to whether any changes in the SeaWiFS clear-sky pixels counts occur over these 13 years, we have calculated the temporal variations of SeaWiFS retrieval fractions (number of days with retrieved AOD relative to the total number of possible days) for each 1 deg x 1 deg grid over the whole globe and the results are now included in the new figure 10. We also compare the retrieval fractions from SeaWiFS with those from MODIS, which has maintained a stable equator crossing time around 10:30 am, during the overlapping years (2000-2010). Our analysis indicates that the patterns of AOD retrieval fraction differences between early years (2000-2003) and later years (2007-2010) are similar and no significant bias between these two sensors. Therefore, any changes in the clear-sky pixel counts due to cloud cover changes in SeaWiFS data is probably not primarily attributed to the slight drift of the SeaWiFS orbit. We have included all these additional discussions in Section 4.2.

*"It would be useful to replicate Figure 9, but for a measure of interannual variability, for example the standard deviation of the time series corrected for the linear trend. Such a map would highlight regions of large interannual variability more clearly than statistical significance."*

This is a very good point. Since the reviewer#3 also has similar comments regarding this, we have added the trend calculations following the Weatherhead et al. (1998) methodology, which accounts for temporal variability (standard deviation of the time series) and autocorrelation of the time series in determining the statistical significance. So compared to the previous version of the manuscript, the calculations of statistical significance associated with the annual trends are now improved in our revised manuscript in all the tables and figures. Relevant texts are also modified to include these discussions in Section 3, 4.2, and 4.4.

## 2 Other comments

***“Page 8468, lines 16–18: A reference would be useful to support that statement. Note that it is not valid over land, although it does not affect the point the authors want to make.”***

The reference of (Gordon, 1997) is now added in the text.

***“Page 8468, line 24: For the reader to judge on the "extraordinary level" of SeaWiFS calibration, can you give corresponding numbers for a less exceptional instrument?”***

We are trying to avoid offending any calibration teams by pointing out specific numbers for each sensor. But according to Franz et al. (2008), by comparing the trends in water leaving radiance for Terra/MODIS with those for SeaWiFS and Aqua/MODIS from July 2002 to January 2007, they found a residual error in the temporal response for Terra/MODIS radiometric calibration of order -1.4% (decreasing) for the blue bands TOA radiance in January 2007, relative to the calibration in July 2002.

Their study also pointed out that a 1% error in TOA radiance at 551 nm would give rise to a 6-15% error in water leaving radiance at 551 nm, with the largest fractional error occurring in clear (blue) water, while a 1% calibration error in TOA radiance at 443 nm would produce a 6-15% error in water leaving radiance at 443, with the largest error in eutrophic (green) water.

So the radiometric calibration with long-term stability of 0.3% over the 13-year mission for the TOA radiances from SeaWiFS is pretty outstanding.

(Reference) B. A. Franz, E. J. Kwiatkowska, G. Meister, and C. R. McClain, “Moderate Resolution Imaging Spectroradiometer on Terra: Limitations for ocean color applications,” *J. Appl. Remote Sens.*, vol. 2, p. 023 525, Jun. 2008.

***“Page 8483, line 4: Since trends nearer the biomass-burning sources are not significant, does that imply that there has been a change in transport pathways?”***

It is hard to say. Since there are usually larger variability is AOD near the sources and often with frequent cloud cover in many biomass-burning regions, the corresponding statistical significances ( $|\omega/\sigma_\omega|$ ) are often smaller and not able to meet the 95% confidence level. Perhaps this could lead to another future paper for graduate students.

## 3 Technical comments

***“Page 8474, line 16: Remove extra “between”***

Done.

***“Page 8484, line 5: “decrease in”***

Done.

***“Figures 3 and 9: 'Dots indicate significance at 95% confidence level.' Dots are not easily visible on those maps.”***

We have re-calculated the 95% confidence points following Weatherhead method and make dots more legible for both figure 3, 8, and 9.

***“Figure 7, bottom panel, and Figure 10: Could you add the zero-line on the anomaly time series? “***

We have now included the zero-line for both figures.

### Responses to review#3

***“This paper and narrative do not presently demonstrate such rigid quantitative evaluation and consistency. Yes, there is discussion of statistical methods in Section 3. Yes, in Table 1, there are trend and std error (is that stddev?) reported. The authors do refer to thresholds for 90% and 95% significance at numerous spots. But, they never say how they actually derive this, or why its relevant. “***

***“Error bars are not given with Figs. 7, 10 and 12. Black dots, corresponding with a 95% confidence level, are not legible in Figs. 6, 8 and 9, and its really not clear in either of those figures how the result relates to the statistical confidence threshold anyway.”***

After consulting with Dr. Betsy Weatherhead and Dr. Jianglong Zhang (the first authors of the two primary papers quoted in reviewer# 3's comments), we have revised the calculations of annual trends and associated statistical significance following the methodology of Weatherhead et al. (1998) and Weatherhead (2003). The texts in Section 3 and 4 have been modified to reflect this. In order to report and compare quantitatively the values of global land/ocean trends and different regional trends, their uncertainties, and associated statistical significance, we have also added 2 new tables (i.e., table 1 and 3 in the revised paper) and 1 new figure (i.e., figure 9 in the revised paper). Additional discussions related to these new tables and figure are also provided in the revised texts.

The symbols representing regions with statistical significance at 95% confidence level in the trend and correlation maps now become bigger and more visible compared to figures 6, 8, and 9 in the previous version. The error bars are also added in Figure 10. For figure 12, we only include the error bars for the zonal mean of the annual trend in the revised manuscript; otherwise this figure becomes way too busy and distracting.

During the peer-review stage we discovered an inconsistency between the time periods uses for trend calculation for SeaWiFS and AERONET island/coastal sites in Table 1 of the previous manuscript, which has been rectified in the revised manuscript. Additionally, we added the AERONET site of Venise, which provides a long data record but we had previously not considered, to the table, where both SeaWiFS and AERONET detect a decreasing trend in AOD

*“One final point. I disagree with your statement at the end of Sect. 4.1. Trend determination is a function of signal versus noise. If you’re saying that climatic forces impart uncertainty, then this is reflected in the standard deviation. Physically, climate forcing is not doing any such thing that you guys state here, and I believe this to be a very important sentence in the paper. The variability that you are describing acts to lower the significance of your analysis, in the absence of an overwhelmingly large sample. Make the statistical argument, not the qualitative/forcing argument here and elsewhere in the narrative.”*

This is a very good point. The texts in Section 4.1 and 5 have been revised according to this comment.

Finally, we have gone through the comments listed in the annotated text provided by reviewer#3 and make all the necessary changes accordingly.

Thanks very much for your consideration,

Christina Hsu