RC – Referee comments are in Black
AR – Authors response(s) are in Blue

We sincerely thank the reviewer(s) for providing detailed comments and suggestions wherever applicable that helped make substantial improvement of the manuscript. Major changes made in the second-round review of the manuscript are summarized below.

- Included sensitivity tests of O_3 and NO_2 gas absorption through UV-Visible spectrum to estimate the error incurred in the retrieval of aerosol SSA.
- Applied corrections for the aerosol SSA to the entire data set due to NO_2 gas absorption to obtain the corrected aerosol SSA (Revised all figures and tables).
- Included the estimates of errors in the retrieved SSA due to uncertainty in variability of AOD from satellite observations over the 50-km radius at the AERONET sites.
- Clarification on the aerosol-typing scheme employed in this work. Renamed subtitle 'Biomass burning aerosols' as 'Carbonaceous aerosols' throughout the manuscript.
- Revised sentences wherever applicable in response to other specific comments.

Review Report – 1

General Comments

The revised version of this manuscript is much improved from the original, with a more complete section on the estimated uncertainty in the SSA retrievals from the new satellite-AERONET algorithm and better and more complete references to the published literature. However, the uncertainty in AOD is underestimated in the calculations of SSA uncertainty since the AERONET point source uncertainty is assumed for the entire 50 km radius area of the input satellite data. Therefore, the authors have assumed exactly homogeneous AOD over a 100 km diameter circle on earth, which is physically unrealistic. The authors should address this issue in a second revision. More details on this issue are given below in my specific comments. Also, a related problem is the computation of the uncertainty in AAE in this revised manuscript. Again, the uncertainty in AOD is assumed to be zero and the authors have only accounted for the effects of uncertainty in SSA on the computation of AAE uncertainty. This is particularly important for the AAE(354-388 nm) since a small error in spectral AOD can cause a large error in AAE for such a narrow wavelength interval. More details are given below in specific comments. This aspect of uncertainty in AAE also needs to be addressed and discussed in a 2nd revision of the manuscript.
Other issues that the authors should address in a revised manuscript are given below in 'Specific Comments":

Specific Comments:

Line 144: Please add this after 'almucantar plane': (plus hybrid scans to lower solar zenith angles)
AR: Done.

Line 201: Why not use OMI measurements of ozone or a realistic latitude dependent

climatology of ozone? Did you show that ozone amount does not matter in the retrieval? Did you use NO2 measurements from OMI, or what NO2 amount did you assume in the RTM?
AR: As mentioned in section 3.1, the RT model used in the current work accounts for ozone absorption and assumes a constant ozone concentration of 275 Dobson Unit (DU) for all sites. We have now included errors in retrieved SSA due to ±50 DU ozone amounts based on its variability (not shown here) for all the sites considered in this work. To determine the variability in ozone amounts we use the data provided in AERONET (AOD) product, which is derived from long-term (~25 years) monthly average climatology of the total column ozone retrievals from TOMS data gridded at 1.00 x 1.25 deg spatial resolution.

The RT model employed in this work does not account for NO_2 gas absorption. However, we estimate the optical depth of NO_2 and applied correction for our retrievals to obtain the corrected aerosol single scattering albedo. The revised manuscript now includes the corrected aerosol SSA after accounting for NO_2 gas absorption corrections. Details on the effect of NO_2 gas absorption on retrieval of aerosol SSA and correction applied are provided in section 3.3.4.

Line 229-231: This spatial and temporal averaging would certainly increase the difference in AOD between the AERONET point measurements and the AOD that exists in the 50 km radius plus 2-hour difference. Additionally, the delta in AERONET AOD versus the actual AOD in the satellite pixels will increase as a function of increasing AOD since AOD in general becomes less homogeneous in space and time as AOD increases.
AR: The revised manuscript now includes (section 4.1.5) estimate of error incurred in the derived aerosol SSA due to the variability of AOD within the 50 km radius of the point measurement (from AERONET).

Lines 242-243: These Angstrom Exponents are not computed from only two wavelengths as suggested by the authors. These are computed from 3 to 4 wavelengths of AOD with linear fit in logarithmic coordinates. The first three are 3-wavelength values (i.e. 380-500 uses 380, 440 and 500 nm AOD data) while the 440-870 AE uses the 440, 500, 675 and 870 nm AOD data to compute the Angstrom Exponent.
AR: Replaced 'wavelength pairs' as 'wavelength ranges (340-440, 380-550, 440-675, 440-875, etc.)'.

Lines 259-260: It should be noted that urban aerosols have a wide range of absorption; this is not exclusively a weakly absorbing category (see Dubovik et al. 2002 and Giles et al. 2014). Likewise, biomass-burning (or carbonaceous) aerosols exhibit a very wide range of absorption (see Giles et al 2014 and Eck et al. (2003 GRL)), depending largely on the relative contributions of the two phases of combustion (flaming and smoldering). There is extensive overlap in absorption between the two categories of urban and biomass burning. This needs to be discussed in this manuscript as the labels of urban and biomass burning (sometimes called 'carbonaceous' in your paper) often does not make sense in the way your classification system works.
AR: We have now clearly mentioned that fine mode particles with ($\alpha_{440\text{-}870} \geq 1.2$) consists of both carbonaceous and urban types of aerosols and exhibit wide range of absorption.

Lines 269-272: Therefore, this is climatology of aerosol layer height and should be clearly stated as such here. Climatological versus actual ALH can vary significantly for any given observation. Was this variation from climatology quantified in the uncertainty of ALH? It would also be expected to vary regionally, as some regions have greater variance in ALH both seasonally and day-to-day.

AR: Yes, we have now clearly mentioned that ALH used here is a global monthly climatology (1 x 1 deg) derived from 30-month long record of OMI-CALIOP collocated data set. Considering the limited samples of CALIOP over 1 x 1 deg gird (16-day overpass cycle) and the day-to-day variation of ALH the uncertainty in the derived monthly (NOT seasonal) ALH climatology is estimated to be within ±1 km.

Lines 286-287: Six-hour surface pressure from NCEP/NCAR reanalysis at 2.5-degree lat-long spatial resolution is interpolated to each AERONET site location and altitude and is provided with the AERONET files of AOD. It would have been much more accurate to have used those values of surface pressure rather than compute it from station altitude.

AR: As described in the section 3.3.3, we use surface pressure provided in the OMAERUV and MODIS aerosol products. These products use high-resolution digital elevation models (≤ 90 m) to compute surface pressure and are provided as ancillary data in the respective products. While OMAERUV product directly provides surface pressure for each ground pixel, MODIS aerosol product provides surface elevation, which are converted to pressure at standard atmospheric conditions.

Thanks for the suggestion on NCEP/NCAR reanalysis six-hour surface pressure data set; we will keep this in our thoughts for the future upgrade of our retrievals.

Line 360: Why stop at AOD(440)=0.4? It would be useful to show estimates at higher AODs also. The uncertainty in SSA retrieval will decrease significantly at higher AOD levels.

AR: Yes, we agree it would be useful to show estimates of higher AODs as well. However, it is well known that retrieval uncertainty decreases with increasing AOD, and our purpose is to determine the minimum AOD where SSA uncertainty is still reasonable within ±0.03-0.05 in the UV-Visible spectrum.

Line 371-373: This is the uncertainty in measured AOD at the AERONET site. However, you have used input satellite data over a 50 km radius and +-2-hour interval from the AERONET site location. Therefore, the variability in AOD over space and time certainty exceeds the point measurement uncertainty at an AERONET site by about a factor of ~50% to 100%. I therefore believe that you have underestimated the uncertainty in your SSA retrievals due your assumption of AOD uncertainty that is not representative of a 100 km diameter satellite average AOD.

AR: The revised manuscript now includes error estimates in our SSA retrievals due to variability in AOD over 50 km radius around the site and ±2 hours of the satellite overpass time (section 4.1.5). To estimate the error in our retrieved SSA due to this assumption we initially estimate the variability in AOD derived from OMAERUV and MODIS-DB AOD products, and ±2 hours of

AERONET AOD from the satellite overpass times. Based on the variability of AOD (not shown here) for the pixels within ±2 hours and 50 km radius of all sites considered we use a perturbation of ±0.2 for λ < 400 nm and ±0.1 for λ > 400 nm to determine the error in our SSA retrievals.

Lines 394-395: It is well known that fine mode particle size increases as AOD increases in many regions due to aging processes of coagulation and condensation (see Dubovik et al. 2002; Eck et al., 2010; Eck et al., 2012). Therefore, your errors due to the use of climatological size distribution averages will be biased as a function of AOD.
AR: We used seasonal climatology of particle sizes to develop LUT radiances. To estimate the error incurred in our SSA retrieval we used perturbation of ΔVMR = 20%. The value of ΔVMR was chosen based on the examination (not shown here) of particle sizes over all sites as a function of AOD and includes the effects of aging processes of coagulation and condensation.

Lines 414-415: Are all pixels assumed to be cloud contaminated or a fixed percentage of pixels assumed to be cloud contaminated in these calculations?
AR: For the sensitivity tests on cloud contamination, we developed LUT for each aerosol type assuming a cloud layer of 0.5 optical thickness in the RT simulations. Therefore, all the pixels are assumed cloud contaminated.

Lines 422-423: I cannot agree with your statement of minimal absorption in the UV from trace gases since NO2 absorption peaks at 380 and 440 nm. Also, NO2 column abundance varies tremendously across the globe and also seasonally. In winter in East Asia (China and South Korea) the NO2 amounts are very high and result in significant absorption in the UV and 440 nm. It appears as though you are basically computing SSA due to aerosols plus NO2 in eastern China and Korea thus overestimating aerosol absorption in these regions. AERONET utilizes a global monthly climatology of NO2 at 0.25-degree resolution derived from OMI data in order to correct the AOD and sky radiances for NO2 absorption effects. This bias in your SSA retrievals, which are maximum in China and South Korea, need to be discussed in the text.
AR: The revised manuscript now includes correction applied for the SSA retrievals to account for NO_2 gas absorption (section 3.3.4). We use NO_2 concentration provided in the AERONET AOD product (determined from monthly climatology of the total column NO_2 retrievals from OMI measurements gridded at 0.25 x 0.25 deg spatial resolution) and absorption coefficients from Vanadele et al 1998 to determine τ_{NO2}. The obtained spectral τ_{NO2} is used to estimate the actual aerosol SSA (shown in the below equation) as demonstrated by Krotkov et al 2005.

$$\omega_a = \omega(no\ NO_2\ corr).\left[1 + \frac{\tau_{NO}}{\tau_a}\right]$$

Where, ω_a is the true aerosol SSA,

ω is the aerosol SSA unaccounted for NO_2 absorption,

τ_{NO2} is the optical depth of columnar NO_2 amounts, and

τ_a is the aerosol optical depth after correcting for Rayleigh, and trace gases including NO_2.

Lines 456-457: However, it seems like you ignore this significant component of atmospheric variation in pressure due to meteorology in your calculation of uncertainty.
AR: Figure 1b in Colarco et al 2017 reports the differences in OMAERUV (static) and MERRA-2 (6-hourly) surface pressure. This study suggests the differences in surface pressure employed in the two data sets are mostly found over mountainous terrain (up to ±15 hPa) and oceans (> ±15 hPa). Therefore, the assumed uncertainty of ±100 m terrain height (±12 hPa) in our sensitivity test in the retrieval of aerosol SSA accounts well for these effects.

Lines 477-479: It seems you have neglected a significant source of uncertainty in your computations of AAE uncertainty. The uncertainty in AOD is also a significant factor especially when the wavelengths are close together such as for AAE(354-388 nm). Therefore, your uncertainty estimates can be considered minimum values since it has been assumed that spectral AOD have zero error in your computations.
AR: This is not true. The uncertainty assumed in SSA here corresponds to the overall error in the retrieval of SSA. We have now mentioned it clearly.

Table 3 presents uncertainties in the computation of AAE for ±0.01 intervals of $\Delta\omega_o$. In the main text (section 4.2), only ΔAAE values associated with $\Delta\omega_o$ = ±0.04 are reported which corresponds to the combined error from all variables involved in the retrieval of ω_o at UV wavelengths as determined in the previous section. From our sensitivity test it is noted that a perturbation of $\Delta\omega_o$ = ±0.04 yields an error in AAE associated with the 354-388 wavelength pair within ±0.13, ±1.3 and ±0.7 for carbonaceous, dust and urban aerosols respectively.

Lines 496-498: It can be expected that this is the more typical situation since the SSA retrievals are independent at each wavelength. Therefore, I would expect the AAE uncertainty to be very large, much higher than your previous analysis that assumes the same bias in SSA in both wavelengths.
AR: For the case where $\Delta\omega_o$ is perturbed only at one of the tail ends of the wavelength pairs the uncertainty in AAE is much higher for 354-388 ranges. This is clearly demonstrated in our results. A small perturbation of $\Delta\omega_o$ = ±0.01 at one tail end of the pair yields an error in AAE up to ±1.2 at which is equivalent to a perturbation of $\Delta\omega_o$ = ±0.04 at both tail ends of the 354-388 wavelength pair.

Line 508: What uncertainty in AAE do you assume here?
AR: For the conversion of our retrieved SSA at 388 nm to 440 nm (matching AERONET wavelength), we use AAE determined from 388-366 wavelength pair. The uncertainty in $AAE_{388-466}$ (not shown here) is relatively less than those found at $AAE_{354-388}$.

Lines 526-527: Any ideas on why the difference is so large for the Lake Argyle site? Small sample size or surface reflectance uncertainty?
AR: Although additional investigation is required, we believe the large difference in SSA is attributed to the surface reflectance uncertainty. The same sample size at other sites produced much better agreement as evident from the figure 9 (revised version).

Lines 529-531: However, you should also note in the text of this manuscript that the surface reflectance is a relatively small source of error in the AERONET retrievals with upward viewing sky radiance measurements.
AR: We have now mentioned clearly that for AERONET SSA surface reflectance is relatively small source of error.

Line 540: The Figure 9 y-axis labels need to be clarified and/or changed, since it is not possible to know what is being plotted without reading the text first. The current y-axis labels just give a wavelength and a satellite name, so it is impossible to interpret by itself.
AR: Revised y-axis title for clarity.

Lines 545-547: This could be partially explained by more sensitivity to the variability in particle size for fine mode aerosols coupled with significant departures on some days from the climatological values used in the retrievals. For dust there is much less sensitivity to particle size.
AR: Thanks for pointing this out. We added a sentence to mention this point.

Line 565: However, the surface reflectance is only a significant source of error in AERONET for low AOD magnitude, ~ <0.2 at 440 nm.
AR: These sentences briefly describe the fundamental difference in AERONET and our SSA retrieval techniques irrespective of the degree of uncertainties of the variables involved.

Line 567: Add this after almucantar plane: (or hybrid scan).
AR: Done.

Line 569 & 571: replace 'weak' with 'relatively strong' in both lines.
AR: Done.

Line 578: I suggest that you keep consistency in your labeling/categorizing of the aerosol type that you often call 'carbonaceous'. Immediately below in section 6.1 you call this type 'biomass burning'. It would be clearer to the reader if you consistently used the term 'biomass burning' throughout the manuscript.
AR: The section title is renamed as ‘Carbonaceous aerosols’.

Lines 596-597: September is also a month of significant biomass burning smoke in Missoula, while June typically has a very minor amount of smoke. Please correct this statement.
AR: We have now mentioned fires are common ‘June through September’.

Lines 606-607: Should include country for each site name i.e. Brazil and Bolivia in this case.
AR: Done.

Lines 611-612: Are these UV values of SSA for the JJA or SON months? Please clarify this

sentence.
AR: The regional average SSA reported for the JJA, and SON months correspond to 466 nm.

Line 619: Please include the country names: Zambia and South Africa.
AR: Done.

Line 634: It is well known that there is always some dust present in the Sahel and Sudanian zones in the dry season. This is the reason for the relatively low AE of 1.3 since these are mixtures of fine and coarse mode particles. The presence of dust is the reason for the relatively flat spectral SSA at Ilorin. If these were all fine mode biomass burning particles with much black carbon then the SSA would decrease with increasing wavelength.
AR: Revised the sentence to mention these are mixtures of fine and coarse mode particles.

Lines 646-648: Cairo is a very large city, metropolitan area population of 21 million, with many emissions from industry and traffic. It is well known for very high levels of pollution from industry and vehicles. It is not possible to isolate the properties of the aerosol from agricultural burning alone. Mixture of biomass burning plus urban aerosols is inevitable. Please convey this in the text. It is quite odd to even include this site in the Biomass Burning section of this paper.
AR: The section title is renamed as 'Carbonaceous aerosols'. We have now clearly mentioned at the beginning of section 6 that our results do not represent a robust characterization of aerosol types, neither we intend to tag any site as 'biomass burning', 'dust', 'urban' or 'mixed' category. The aerosol typing scheme employed in this work based on UVAI and particle sizes are only to guide our algorithm to include ALH in the SSA retrieval procedure.

We also mentioned in the paragraph that Cairo is one of the megacities with high pollution levels throughout the year and emissions from agricultural waste burning adds additional aerosol burden during September through December.

Line 653: Here is another example of the confusion that your 'carbonaceous' aerosol classification causes. These are dominantly urban/industrial aerosols at Beijing and XiangHe, not predominantly biomass burning aerosols. Your inclusion of this site under the section "6.1 Biomass Burning" is wrong and therefore very misleading.
AR: It is well known that aerosol sources at Beijing and XiangHe are dominantly urban/industrial throughout the year and biomass burning emissions are noted only during a season. Therefore, mixtures of biomass and urban/industrial aerosols are inevitable. We have now mentioned it clearly. The section title is renamed as 'Carbonaceous aerosols'.

Line 655: Do you mean these are AAE values here, if so then clearly state it.
AR: Added '$AAE_{340\text{-}646}$'.

Lines 662-663: Please also mention in the text the other significant aerosol sources in the Indo-Gangetic Plain region in northern India such as brick kilns that burn coal and therefore emit much black carbon, power plants that burn coal and also heavy vehicular traffic in the cities

such as Kanpur. You give the impression in the manuscript that there is only biomass burning going on in the region, which is both false and misleading.
AR: Added sentence to mention other aerosol sources.

Lines 754-756: Also, it is likely that a much smaller sample size in MAM contributes to the difference with JJA since a few unusual cases in spring season may significantly affect the average.
AR: Revised the sentence to mention sample size.

Lines 764-768: Please note that NO2 absorption has not been adequately accounted for in your retrievals in the urban regions where NO2 column amounts are highest. Therefore, it is important that you mention that your retrieved SSA are likely biased low especially at 380 and 440 nm where the NO2 absorption is highest. Maps of NO2 from OMI and TROPOMI clearly show high NO2 amounts over urban regions therefore affecting all of your retrievals for all urban sites, but especially so for China.
AR: Revised manuscript now includes correction for the SSA retrievals to account for the NO2 gas absorption.

Lines 785-786: This probably due to biomass burning aerosols in SON mixing with urban aerosols, not due to a change in the urban aerosol absorption as you seem to be implying. Please rephrase this sentence.
AR: Revised the sentence to mention mixing carbonaceous and urban aerosol samples.

Lines 793-794: The AE=1.3 strongly suggests a mixture of fine and coarse mode aerosols. Even 10-20% of the AOD from coarse mode particles (soil dust, etc) can result in substantial flattening of the SSA spectra. This seems a more likely explanation than your suggestion of a mixture of black carbon and organic carbon, which does not seem to make much sense since both are fine mode particle types.
AR: Revised the sentence to suggest mixture of dust and carbonaceous aerosols.

Lines 796-797: Averaging 13 sites together in Europe over a vast geographic area is not a very rigorous approach. In fact you point out at the end of this paragraph that three of these sites apparently had significantly higher absorption in SON than the other sites. I would suggest some further discussion of the range of SSA values over these 13 sites.
AR: Like the other regions, we added a brief description on the aerosol sources over the sites spread across the Europe. Our results indicate high aerosol absorption during SON for the sites at the Ispra, Modena and Rome located over Northern to Central Italy. It is likely that the increase in aerosol absorption noted during SON is caused by mixture of pollution and carbon amounts from wood burning for domestic heating. However, we do not have retrievals over other sites in the Europe for SON and DJF months to compare the range of values.

Line 805: change 'observing' to absorbing' here.
AR: Corrected as ‘highly absorbing’.

Line 806: There is always a mixture of organic and black carbon from fossil fuel combustion, so this sentence essentially tells us nothing.
AR: Revised the sentence.

Lines 812-813: Yes, aerosol humidification is summer results in a large shift in the fine mode particle size to larger particles relative to winter. Since you only apply a yearly mean aerosol size distribution it seems likely that you are underestimating the winter-summer difference in SSA since the larger particles in summer scatter light much more efficiently.
AR: We use seasonal average climatology (NOT yearly mean) of particle sizes for the LUTs as mentioned in section 3.1. Our results indicate wintertime (DJF) aerosols noted over NE China are more absorbing than those noted during summer (JJA) – this is consistent.

Lines 840-841: This is incorrect. The cerrado vegetation type dominates as a source of biomass burning aerosol only at the Cuiaba site, not 'at most sites considered here'.
AR: Corrected the sentence for cerrado vegetation at the Cuiaba site.

Lines 853-854: It should also be mentioned here that the uncertainty of AAE for such a narrow wavelength interval of 354-388 nm is very high. It is higher than your estimated values in Section 3 since you assumed that AOD in both wavelengths was perfect (no error in AOD was assumed).
AR: Revised the sentence to mention that uncertainty in our computation of AAE354-388 is high. As mentioned in the above responses, uncertainty in AAE is estimated using the ensemble of uncertainties in the retrieval of SSA, which includes AOD and several other variables.

Line 861: This can be true for China where the fine mode particle size is very large due to aging and humidification processes (thereby reducing the AE value) but for the Sahel the reason for the AE is mixing with coarse mode dust.
AR: We have revised the sentence, added mixing of dust in Sahel and humidification processes in NE China as examples.

Line 940: I cannot see a valid justification for such a large and polluted megacity as Cairo to be included in the Biomass Burning section. Even when biomass burning occurs near Cairo there is certainly a mix of aerosol types since the urban aerosol sources remain strong producers of aerosol throughout the year.
AR: The section title is renamed now as ‘Carbonaceous aerosols’.

Lines 948-950: Again, I find that sites in NE China are well known to be dominated by urban/industrial pollution. Including this Chinese region in the Biomass Burning section is very problematic. Your classification system between biomass burning (that you call carbonaceous half the time) and urban is dubious at best. This ambiguity in these two classifications needs to be discussed in the text so that the reader can be aware of the large overlap between these two aerosol types in your analysis. Even if there is a month or season with biomass burning in

the NE China region the overall aerosol could only be described as mixed since the urban aerosol loading is still very high.
AR: We agree, the sites over NE China are dominated by urban/industrial pollution throughout and biomass-burning emissions are adding an additional aerosol burden only in a season. We clearly mentioned the aerosol typing used here is not robust.

Line 968: Replacing 'carbonaceous' with 'biomass burning' here would be clearer for the reader, especially since you sometimes classify urban aerosol as carbonaceous (see comment above).
AR: Subtitle 'biomass burning' is now replaced with 'carbonaceous aerosols' throughout the manuscript.

Line 973: Please provide urban site names here.
AR: Added site names in the sentence.

Line 975: Also provide site names here.
AR: Added site names in the sentence.

References

Krotkov, N. A., Herman, J. R., Cede, A. and Labow, G.: Partitioning between aerosol and NO2 absorption in the UV spectral region, in Ultraviolet Ground- and Space-based Measurements, Models, and Effects V, edited by G. Bernhard, J. R. Slusser, J. R. Herman, and W. Gao, p. 588601., 2005.

Colarco, P. R., Gassó, S., Ahn, C., Buchard, V., Dasilva, A. M. and Torres, O.: Simulation of the Ozone Monitoring Instrument aerosol index using the NASA Goddard Earth Observing System aerosol reanalysis products, Atmos. Meas. Tech., 10(11), 4121–4134, doi:10.5194/amt-10-4121-2017, 2017.

Vandaele, A. C., Hermans, C., Simon, P. C., Carleer, M., Colin, R., Fally, S., Mérienne, M. F., Jenouvrier, A. and Coquart, B.: Measurements of the NO2 absorption cross-section from 42,000 cm-1 to 10,000 cm-1 (238-1000 nm) at 220 K and 294 K, J. Quant. Spectrosc. Radiat. Transf., 59(3–5), 171–184, doi:10.1016/S0022-4073(97)00168-4, 1998.

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AR – Authors response(s) are in Blue

We sincerely thank the reviewer(s) for providing detailed comments and suggestions wherever applicable that helped make substantial improvement of the manuscript.

Review Report - 2

I would like to commend the effort made by authors in addressing the comments and restructuring the analysis based on the review of the previous version of manuscript. Overall, the quality of presentation and scientific soundness of the analysis has improved a lot. Before the publication, please make sure that the texts do not mask the data points in the figures.

I have a few minor comments and is given below.
L#78: L_0 is not defined
AR: L_0 is atmospheric path radiance. We have now defined in the text.

L#84: Where is the dependence of omega_0 in Eq. 1
AR: In general, the solution of RTE is obtained by expressing the radiance terms as a product of single scattering albedo, phase function and optical depth through (Fourier series of polynomials) cosine function of zenith and azimuth angle of light propagation.

L#408-409: The latest estimation of TOA radiance measurement uncertainties for MODIS on Aqua exists. Just a comment.
AR: Added appropriate reference.

L#455: From the data, it is evident that there exists a spectral dependency, even though the slope is small.
AR: Removed 'spectrally invariant' and revised the sentence accordingly.

L#805: Do you mean predominant? or a typo for highly absorbing.
AR: Corrected as 'highly absorbing'.

Figures
F5: Explain the acronyms used in the figure labels in the caption. Also, explain what dashed and solid lines are.
AR: Revised the caption and legend for figure 5.

F6: The legends in the first subplot are blocking the data points in the figure.
AR: Revised figures to show all data points and legend clearly.

F7: Several data points are missing due to the range of y values plotted. Would you mind making sure that data is not masked when you replot the figures for final publication?
AR: Revised figures to show all data points and legend clearly.