Dear Authors and Editor,

This is a well-written paper, presenting clearly an important subject. I enjoyed reading it and appreciated particularly the finding that the presence of clouds leads to increased observed AOD by clear-sky instruments. However, even though the manuscript is generally scientifically rigorous, towards the end (Section 4) it produces some unfounded and arbitrary results. I see that this has been pointed out also by another reviewer. In my view some parts should be either removed or preferably further explored. I think a major revision is necessary before proceeding with the publication process.

Moreover, very little is said on the MATCH assignment of aerosol types and its agreement with observations. I understand that you would like to address the aerosol types in another publication, but a brief presentation of dominant types by region would be useful here. Then the reader would be able to understand better the possible effects of the aerosol types on the fluxes.

We would like to thank this reviewer for the close reading and interest in our paper. We appreciate the reviewer's comments, questions and suggestions and have incorporated several of the reviewers' comments along with our responses into the paper. However, we are not entirely sure which results the reviewer finds either unfounded or arbitrary. Within section 4 we discuss the discrepancy between clear sky calculations and observed surface irradiance. At line 523 we state clearly "…we cannot identify the cause of the discrepancy….", and subsequently propose possibilities. While again, perhaps not entirely satisfying from our or a reviewer's perspective it is encapsulates our current understanding of our results.

Regarding the second paragraph above we have expanded our description of the aerosol types in the MATCH model, their optical properties, and how those types and properties are transferred into the radiative transfer calculations. As to an analysis of aerosol type by region, our comparison of AOD to AERONET observations is broken down regionally which, generally identifies dominant aerosol types as stated in the paper. Both reviewers requested such an analysis and in our we show that given the information currently available to us, attempting to extract estimates of fine and coarse properties from MATCH is inconclusive at best. This is shown in the following discussion (which is repeated for both reviewers):

We have at our disposal no simple method to separate out fine vs. coarse mode aerosols from the output retained. Though we note that the distinction between fine and coarse is somewhat arbitrary as it is based solely on the observed AERONET size distribution. Thus, a fine aerosol might be 0.1um at one site and 1.0um at another. (Figure 9 below, taken from Dubovik et al. 2000.)

DUBOVIK ET AL.: ACCURACY OF RETRIEVED AEROSOL PROPERTIES

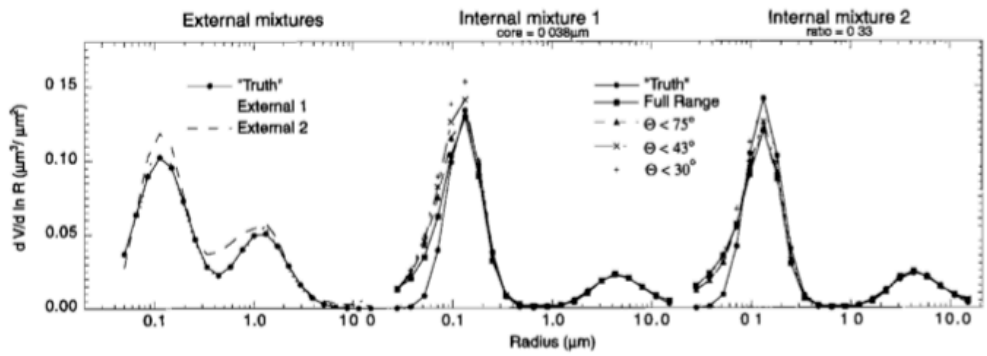
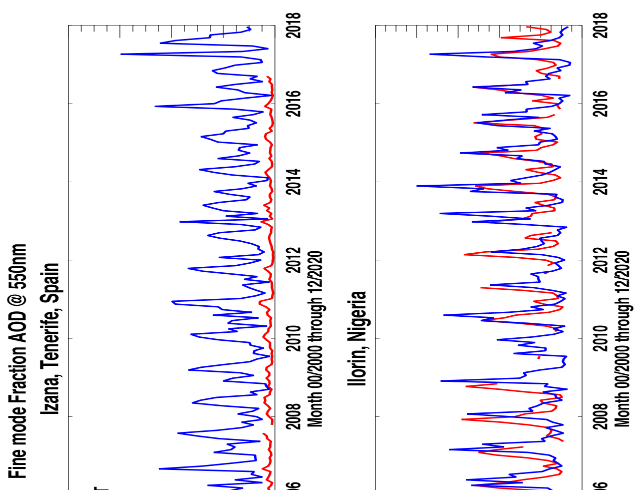
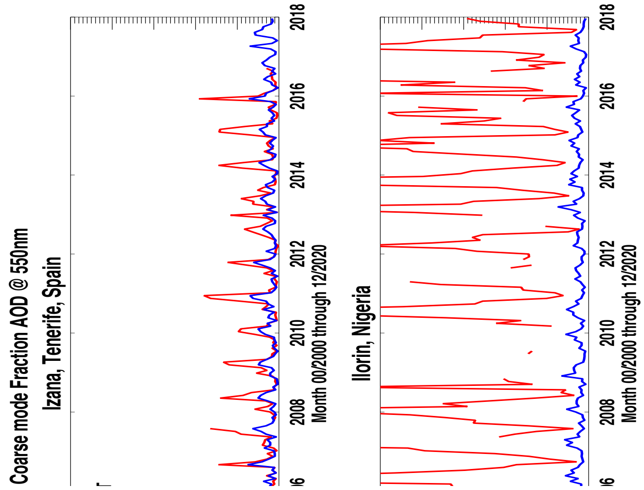


Figure 9. Volume size distributions of inhomogeneous aerosols (externally and internally mixed) retrieved using the model of scattering by homogeneous spheres.

None the less we did attempt such a comparison simply using large and small dust particles as defined by MATCH and plot them here against fine and coarse mode observations from AERONET at two sites likely to be affected by dust, Ilorin, Nigeria and Izana, Tenerife, shown below.



AERONET Fine mode fraction of AOD compared to MATCH average of dust particles < 1um. At Ilorin there is some correlation and similarity in magnitude, but not at Izana.



AERONET Coarse mode fraction of AOD compared to MATCH average of dust particles >1um. At Ilorin there is some correlation and large disparity in magnitude while Izana, there is a good correlation and similar values.

The point of these plots is that the correlation between the Fine and Coarse mode AERONET observations with, in this case dust aerosols from MATCH, is somewhat random. True fine and coarse modes would require a detailed extraction of all the aerosol profile data, after water uptake by soluble aerosols had been taken into consideration. These values are not retained and would require significant code changes and re-running of the product.

Please see below for more specific comments (main and trivial intermixed and shown by "order of appearance")

1) Line 82: In this manuscript you say little on aerosol optical properties other than AOD. For example (*t*)here (*is*) no mention is made on the single scattering albedo (SSA) and asymmetry parameter g, or equivalently on aerosol types.

For the aerosol types used in the running of the Langley Fu & Liou radiative transfer (RT)model we have added a plot (now Fig 1) of SSA and g at 550 nm. Hygroscopic aerosols are shown as a function of relative humidity. The relationship between the MATCH aerosol types and those used in the RT model is explained in line 150-160.

2) Lines 106-107: "The MATCH model is described in Section 1. In Section 2, we explain the aerosol transport model briefly." MATCH is the aerosol transport model, and Section 1 is the introduction. Should the sentence "The MATCH model is described in Section 1" be removed?

Thank you catching this mistake, the line has been removed.

3) Line 124: The breakup of types is not the same as in Table 1. The division between soluble and insoluble is not explained, does it refer to organic particles? Is fine dust a different type than coarse dust? If yes, this opens possibilities for a later (here or in another publication) comparison of the two types to the AERONET fine and coarse dust.

The reviewer is correct. That list of aerosol types represents those found in the Langley Fu & Liou radiative transfer model. The list has been changed to show those types and we have added discussion along with a table and plot to better define how aerosol properties are treated in the models. Table 1 shows the transition from MATCH to Langley Fu & Liou constituents and Figure 1 shows the distinction between insoluble aerosol (given as constants) and soluble (plotted as lines.)

4) Lines 124-125: "Model physics... of soluble gases and aerosols." Two sentences are saying practically the same thing.

We have changed the sentence to read "Model physics included in MATCH are parameterizations for convection and boundary layer processes that include…." to clarify that processes mentioned in the second half of the sentence detail the 'physics' mentioned in the first half of the sentence.

5) Table 1 and lines 145-148: So carbonaceous and sulfate particles are climatological, while sea salt and dust are dynamically generated. This, together with the lines 126-127 saying that MATCH parameterizes "the scavenging of soluble species", confuses readers

Table 1 states that the <u>source</u> of sulfate and carbon are climatological. Lines 126-17 and 145-148 state that those aerosol fields subsequently evolve through advection and other processes.

6) Line 171: "we do not use a quality assurance confidence score". Later you claim that the use of QAC from MERRA in convective regions creates a bias (wrt MODIS) opposite in sign but similar to the bias from MATCH, supporting the view that QAC is not necessary. This argument is a little superficial. The cause of the different biases between MERRA2 and MATCH is not sufficiently attributed to the use of the QAC. Moreover, a better check for the usefulness of the QAC would be to compare MATCH results with and without low QAC values. I am not saying that the authors should do this, but merely ask for an explanation why the QAC scores are not used in MATCH.

The failure to use the Quality Control flags was an oversight during the development of the code. It will be corrected in the Edition 5 data product release.

7) Figure 1 and relevant text: If you want to show the effect of AOD assimilation, why show the differences between consecutive hours and not the differences between assimilation and non-assimilation runs? Are the latter not available?

As the reviewer has surmised, the latter are not available.

8) Figure 1 and relevant text: I guess that the highlighted 15 deg bands is where local noon occurs. Still, I am not sure why and how the assimilation is taking place there and then. Is it because local noon is between the 10:30 and 13:30 overpasses? Are the Terra and Aqua AOD averaged and assigned to local noon?

We agree that the introductory sentence to the paragraph is not as descriptive of the process of assimilation as we had hoped. It has been replaced with the following:

"The assimilation process begins by combining the dark target and deep blue aerosol retrievals from MODIS (both Terra and Aqua when available) and creating global daily averages for the month. As MATCH progresses through time the data at local solar noon are assimilated by taking a 15° longitude width of retrieved AOD from the daily mean maps."

9) Lines 189-193: So if a volcano is captured by MODIS, does MATCH amplify AOD for all aerosol types? MERRA2 had the same problems for the Pinatubo eruption, with the aerosol speciation being wrong in the post-Pinatubo years.

We only assimilate the MODIS AOD. The aerosol mass is then distributed spatially (and vertically) but the aerosol type is mapped (per Table 2) into the available species in the Langley Fu & Liou RT code. Subsequently, yes, the process developed here will amplify all aerosol types (and so speciation may be incorrect) defined by MATCH for a given grid box where the MODIS data is assimilated. We point out this fact in lines 201-205 (new version of paper.)

10) Line 199: MERRA 2 assimilates AOD also from AVHRR for your first two years of your comparison (1979-2002) and MISR over bright desert regions (2000-2014)

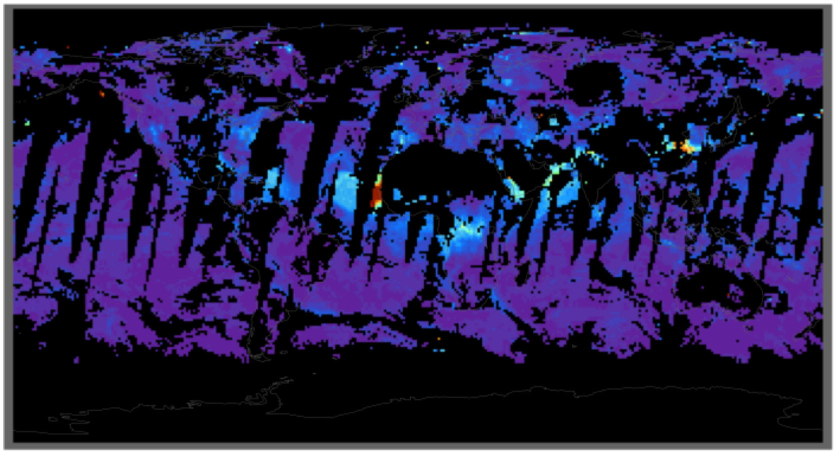
Thank you. That information has been added. The sentence now reads: "MERRA2 also assimilates surface observed AOD by AEROENT and ship born AOD observations as well as AVHRR and MISR retrievals for the years 2000-2002 and 2000-2014 respectively."

11) Figure 2: Which MODIS product is MOD08Mdy08? I couldn't identify it anywhere. It has to be daily, L3, DT and DB combined, right?

We use two MODIS products, the MOD08_D3 from Terra and MYDO8_D3 from Aqua. We have changed the plot to read simply "MERRA-2 – MODIS" and "MATCH – MODIS" and clarified the product definitions in the text.

12) Lines 207-211. I would like some clarifications. MODIS AOD comes from the clear-sky pixels in its 1x1 cell. Each day with at least some clear-sky pixels in MODIS, the whole all-sky 1.9x1.9 MATCH and 1x1 MERRA-2 cells are used. So at each cell you are comparing the clear-sky MODIS data with the all-sky data from the whole cell, aren't you? You address the problem with the MATCH and MERRA2 daily average data vs. the MODIS only-overpass data, but if I understand correctly, the clear-sky MODIS vs. all-sky MATCH/ MERRA2 problem would be present even for instantaneous values at the overpass times.

When MODIS clear-sky AOD is available for a grid box, the assimilation will replace MATCH AOD regardless of the cloud fraction in the MATCH grid box at the time. For this analysis we create monthly average maps from daily mean MODIS AOD maps created for the assimilation process into the MATCH model. However, even with both Aqua and Terra data, daily mean MODIS maps are not complete due to clouds. (See image below showing a daily mean AOD map from Terra.) These clear sky MODIS daily averages are then used as a guide for when and where to sample the MERRA and MATCH models across a given month.



With 20 years of data, the maps are eventually filled completely though sampling does vary from grid box to grid box. Figure 2) shows the differences between the mean MODIS map and the MERRA2 and MATCH, sampled at the day/place where MODIS has values.

13) Fig. 4 and relevant text: Again, I would like some clarifications. If clear-sky AOD is available, why not compare it against MODIS? Besides, how can you get clear-sky AOD from all-sky AOD by weighing with the clear fraction? Also, the mean MATCH clear-sky AOD in Fig 4bottom is 0.136, while the mean clear-sky AOD in Fig 3b is 0.160. Apparently, the term "clear-sky" means different things here, but I am not sure what is the difference exactly.

The plots shown in bottom of Fig 4 (now Fig 5) are not true clear sky AOD. They represent an average, over 20 years, of monthly mean MATCH and MERRA2 AOD that used the fractional clear sky within a 1degree grid box as a weight for the averaging process. We have changed the text in lines 257-264 (new version of paper) and in the caption to Fig 5 and added the reference to Loeb et al. 2020 where this technique was explained.

14) Fig. 4 and relevant text: In l. 244 you mention "March 2000 through February 2020", which also seems to agree with the color bar text. However, in the figure caption you mention "January 2020". Judging from the maps, the former seems to be correct, but please correct the inconsistency.

Thank you for the close reading. These maps are 20-year climatologies as you note. 'January 2020' has been removed both within the picture caption and earlier in the text the term 'monthly mean' was replaced with the words 'climatological mean'.

15) Lines 263-264: "MATCH ... larger... than MODIS". Even though this seems to be the case in Fig. 2b, Fig 3 gives the MATCH global average as 0.16 vs the MODIS 0.174. What am I missing? Area-weighted averaging?

That comment was left over from a previous edit. The reviewer is correct that both are smaller than MODIS. We have corrected the sentence to read:

"The above results indicate that both MATCH AOD^{clr}_{MODIS} and MERRA2 $\overline{AOD}^{clr}_{MODIS}$ are generally smaller than MODIS $\overline{AOD}^{clr}_{MODIS}$."

16) Line 294: I guess the two channels are the 500 and 675 nm and interpolation is used to get the AOD at 550 nm.

We use the angstrom exponent as given in AERONET data files to derive a value at 550nm, generally, from the 500 nm & 675 nm channels if available.

17) Line 299: "45 sites". More than 45 sites are shown in Fig. 6 and Table 2. Also, Fig. 6 and Table 2 disagree in the numbers.

Thank you for finding that error. The total number of AERONET sites used 55 as listed in Tables 2 and 3. The count has been corrected in the text and in Figure 6. (Revised manuscript Figure 7.)

18) Line 316: I think it should read "MATCH clear-sky AOD for the Brazil group is biased high by 0.02"

Thank you for finding that error. The text has been corrected to "0.02."

19) Line 392: "fixed aerosol sources in time". This goes only for carbonaceous and sulfates, right?

Correct. These types are listed as such in Table 1 as 'monthly climatological'.

20) Line 440: I think it should read "scale nearly linearly with AOD between"

Yes. We have added the words 'with AOD' to the sentence.

21) Line 440: It should be Fig. 10 instead of 8

Correct. Thanks for finding that error. Text has been changed accordingly.

22) Line 468: Moreover, the polar irradiance is small, so it makes sense to see the smallest overestimations.

That sentence has been removed.

23) Line 469: Or overestimation of the asymmetry parameter? Or underestimation of ozone? Maybe a general bias of 1-2% for the Fu-Liu model? There is a known overestimation in the model-calculated surface SW fluxes, still under examination (e.g. https://doi.org/10.1007/s00382-018-4413-y). My point here is that the analysis is not deep enough to provide a definite answer and that these findings are not robust.

The findings are robust in so much as the data available will allow. For that reason, we state in line 468: "This points to the possibility…". We do not make a definitive statement on the source of the error, only suggest a possible reason for the overestimation of DSW in the model. As the reviewer points out, there are other possibilities.

24) Fig. 12: Instead of StdDev, it would be better to use RMSE, similarly to Fig. 13

We agree with the reviewer in that it is good to be consistent so have replaced the figure with values of RMSE instead of standard deviation.

25) Line 477: "increase the observed downward shortwave irradiances". Do you mean by reflection on the clouds?

Yes, or any increase in the diffuse observation due to clouds not identified by the satellite. We have changed the text to be more specific, new version of the paper, lines 480-484.

26) Line 493: Maybe include a brief description of the Kato et al. (2018) methodology for the tuning and possibly justification for these specific adjustments? Otherwise, these adjustments seem arbitrary. I understand that you are trying to make the atmosphere less transparent and more reflective and that the algorithm works by adjusting AOD, albedo and atmospheric water. However, the desired changes in both TOA and surface fluxes may be due to problems in aerosol misclassification, errors in the optical properties of each aerosol type, the neglect of aerosol vertical profiles, etc. If you cannot exclude these sources of error, I think it is premature to assign corrections to AOD, albedo, and water vapor to fit the CERES fluxes. It is useful as a sensitivity study, but its usefulness ends there.

We understand the reviewer's concern. Water vapor affects TOA clear-sky shortwave flux. But it also affects TOA LW flux. Therefore, the adjustment of water vapor amount is constrained by longwave. Clear-sky shortwave adjustment is, therefore, dependent upon how we distribute the difference between surface albedo and aerosol properties. For ocean surfaces that occupy 70% of Earth, the uncertainty of surface albedo is small compared with the uncertainty in the aerosol properties, provided there is no sea ice and wind speed is known. TOA flux is sensitive to single scattering albedo and scaled optical thickness. However, the variability in the optical thickness is much larger than the variability in the single scattering albedo and asymmetry parameter. This justifies, to a first order, adjusting aerosol optical thickness over ocean. Note that we do not have vertical profile information because only passive sensors are used in CERES data production. To further clarify our intent, we have added the following to briefly describe the adjustment process:
"After known biases are taken out, the adjustment of temperature and specific humidity profiles, surface and aerosol properties are derived based on their pre-assigned uncertainty and the difference of computed and observed TOA shortwave and longwave irradiance using the Lagrange multiplier approach."

27) Line 502: You probably mean Table 4

The reviewer is correct it should be Table 4. (In the revised manuscript, now Table 5.)

28) Line 510: You probably mean the top left plot of Fig. 12

The reviewer is correct it should be Fig 12. (In the revised manuscript, now Fig 13.)

29) Line 511: Here too, Fig. 12

The reviewer is correct it should be Fig 12. (In the revised manuscript, now Fig 13.)

30) Line 511: You write "decreasing AODs for the desert group by 0.02", but Table 4 shows that the AOD adjustment for the desert is +0.02

This was poorly worded, and the paragraph has been updated. We state in this paragraph that in order to better match observed TOA reflected SW up, the EBAF-surface product adjusts the AOD upward, on average, globally, by ~0.02. Further we state this seems reasonable for mid-latitude and Asian regions based on our comparisons with AERONET (Table 2). Lines 511 through 513 point out that this increase in AOD, however, is inconsistent with results shown in Table 2 for North Africa where we are already biased high relative to AERONET observations. We have changed the sentence to:
"The positive bias found in the downward shortwave irradiance for the North Africa group (Fig 12c) is not consistent with the positive bias of aerosol optical depth shown in Table 2." (Lines 21-523, new version of paper.)

31) Line 567: This is maybe subjective, but for clear-sky fluxes I would not call the role of aerosol optical properties minor. Definitely not for the AOD, but even for the SSA.

We have removed the word 'minor'.