Once again, we wish to thank these two reviewers for their interest and close reading of our paper. Based on suggestions by Reviewer 1 we have reorganized Section 4 of the paper, dividing it into three parts. Section 4.1 now addresses the impact of AOD on surface SW calculations. We added Section 4.2 which deals with LW surface calculations and the dependence of the LW and SW surface irradiance on dust particle size. (Section 4.2 includes a new table and three new images. To reduce the paper's length, we replaced Figure 14 with a much smaller table as suggested by Reviewer-1.) This new section allowed a segue into a brief discussion of LW aerosol effect and a comparison between observed and MATCH AOD as a function of fine/coarse particle sizes in terms of canonical monthly means. Regarding temporal variability and MATCH's ability to retrieve such, we focused on three desert sites where dust is found seasonally. All substantial changes within the manuscript are highlighted in red. We believe we have answered the reviewer's questions and concerns, and this has resulted in a greatly improved work. Included in this file are our responses (in red) to both reviewer's queries, reviewer 1 first and reviewer 2 second.

Reviewer – 1

Review of the revised paper:

Evaluation of aerosol optical depths and clear-sky radiative fluxes of the CRERES Edition 4.1 SYN1deg data product *by D.Fillmore et al.*

Comments to the author's responses

All my comments were addressed – although those involving extra analyses were unfortunately denied - so if not in this paper ... then hopefully in a future contribution. When the authors state as the goal "to evaluate aerosol optical depths for irradiance computations", then also the clear- sky LW fluxes need to addressed, especially near dust sources. Typical mineral dust aerosol radii near sources are 3-6um and even transported dust has effective radii of about 1.5um. Their given justification to ignore LW effects assumes too small (ca 0.5um) radii for mineral dust. In that context also the last sentence of the abstract ('... are unknown.') is very unsatisfying.

We do not disagree with this reviewer's desire to investigate more fully the dependence of the SYN1deg irradiance calculations on aerosol optical depth and type. However, in the context of the paper, as written, it is difficult to extend the analyses to individual site locations without greatly increasing the length of the text.

Our argument to exclude discussion of LW irradiance in the paper (and in our first response) was based on three factors. 1. Errors in LW down due to AOD are less significant than errors due to water vapor and temperature profiles, 2. Past comparisons indicate GEOS5.4.1 atmospheric profiles (used SYN1deg radiative transfer calculations) capture total precipitable water (PW) and temperature profiles quite well, and 3. Differences between observed and calculated LW down at the surface are smaller than *errors predicted by differences in PW and surface air temperature. Thus, error due to AOD in LW down is small. However, we have added section 4.2 to briefly discuss LW down with respect to AOD and there include the reviewers desired comparison of fine and coarse mode AERONET AOD with MATCH equivalent aerosol types.*

For the dust inconsistency problem also possibly poor choices of AERONET references contribute, as (1) column properties (like AOD) of regions should not be compared to mountain site data (as Izana) and (2) months when other aerosol types dominate should be excluded (as for DJF in western Africa).

Besides the increased length, individual sites may, or may not be representative of larger regions. (A point made by the reviewer in his third comment below.) We note too that we did not include the Izana site in the statistics given in the paper. However, we did indicate it as a site in the North Africa group in Appendix A. That is an error on our part and the site has been removed from the list in the table.

And if AERONET is applied to reveal biases for AOD, then also the offered AOD split between fine-mode/coarse-mode AOD should be applied. I reject the notion that MATCH does not contain (approximately) this size information with its component processing, where dust and seasalt basically define the coarse mode AOD and (organic and black) carbon and sulfate define the fine-mode AOD. And for cases, where/when relevant AERONET sites are missing (as for northern Africa, central Africa or ocean regions), comparisons at coarser temporal (e.g., monthly, seasonal) resolutions (as offered by top down climatologies or other approaches such CERES, SRB 4, ICAP) are (as in my initial review) encouraged for insights on MATCH tendencies.

We attempted to show in our first response the somewhat arbitrary definition of fine and coarse modes from AERONET being the result of a bimodal distribution, where the two modes can vary in particle size depending on the site location. However, to address the reviewers concern we have included a new section (4.2) to the paper to consider the impact of particle size on the SYN1deg irradiance calculations. We use the reviewer's suggested groupings of carbon and sulfates for small particles and large dust and sea salt for large.

The strength of MATCH AOD data are high temporal resolution AOD maps (as needed in modeling) to include short-term regional anomalies, but that does not mean that global multi- annual (average) maps are correct. The most important section is chapter 4. So there should be more weight (and analysis) and less weight on the first 25 pages. In particular, the detailed comparisons to MERRA distract, as it is only a different model interpretation with quite different results and MERRA results are even left out in the comparisons at TOA fluxes (CERES) and surface fluxes (BRSN) of chapter 4.

We have split section 4 into three parts, adding 4.2 "LW Comparisons". There we show LW irradiance calculations compared to observations along with a short discussion of the impact of particle size on the LW aerosol direct radiative effect at the surface and *incorporate canonical monthly means of coarse and fine mode AODs, both observed and from MATCH to show seasonality in both.*

On responses to my minor points of the initial review ...

- The demonstrated smaller AERONET AOD at cloud-free conditions at the CART/Bondville sites may be meteorology (air-mass) related. These statistics will not necessarily apply to many other AERONET sites, for instance at dust dominated sites where higher AOD will likely be associated with less clouds.

While true the observations in the central United States cannot be applied across the globe, they should be representative of large agrarian and midlatitude plain regions that do represent a significant portion of northern hemisphere land types. We cannot include analyses at all the individual sites, however, we have included plots in Section 4.2 at three desert sites to more closely look at dust dominated areas.

- The assumed 1um mineral dust size (0.5um eff radius) is very small, even for transported dust. Thus, the associated reasoning for "LW impacts are not important (to the surface irradiance)" is not convincing at all. Also with larger (up to 10um) dust sizes the effective solar absorption potential (e.g. 1-SSA) quickly increases so that for the same AOD the solar surface irradiances will be lower (dust size in MATCH could be possibly included via a proportional link between dust AOD and dust effective size, as sizes near dust sources are usually larger).

We have divided Section 4 into 4.1 Shortwave and 4.2 Longwave discussions. In section 4.2 we address concerns regarding incorrect particle size and fine/coarse mode comparisons at desert African sites.

- in Figure 1 there are not the promised maps for SSA and g ....?

We added the spectral properties of the aerosol types used in the Langley Fu & Liou Radiative Transfer code. We did not add maps showing their distribution globally. (See our first response to Reviewer 1, question 5.)

- I agree with the authors and retract from my initial conclusion of a "likely stronger fine-mode absorption in MATCH compared to MERRA", because Figure 4 compares model simulated AOD maps. Still, the larger MATCH AOD values in regions, where fine-mode aerosol (SU, OC, BC) types dominate are concerning – possibly helped by the fact that MATCH assimilations only constrains the total AOD and not local component mixtures.

See section 4.2 for added discussion of fine/coarse mode aerosols with respect to AERONET and MATCH.

- I still suggest in AOD assessments to separate fine and coarse mode AOD contributions when comparing to MERRA and AERONET. For AERONET inversions detailed (ambient) 22 bin size classes a separation at the 0.528um size-bin boundary is recommended and in modeling the separation by components with combining BC/OC/SU (fine) and SS/DU (coarse). AERONET also offers simple fine-mode fractions via the AOD spectral dependence (and the SDA method) from direct attenuation data. The fine-mode effective radius can certainly vary (as demonstrated in a figure by the authors) but there are always a minimum at aerosol radii of about 0.5um (unless there is a major volcanic eruption with effective sulfate aerosol radii near 0.5um).

In the new section 4.2 we have added a plot showing canonical monthly means of MATCH aerosol types (as suggested by this reviewer) compared to the fine mode and coarse mode aerosols determined at several west Africa AERONET sites.

- if you use high altitude mountain AERONET sites (e.g. Izana) then you get underestimates in comparisons to regional averages (bad choice!). Also West-African sites (e.g. Ilorin) are biomass dominated in NH winter and dust dominated in NH summer, which complicates a type association unless a seasonal separation is done. You also present fine-mode and coarse mode distributions but I suspect that fine-mode is not dust (as fine-mode dust is secondary to coarse mode dust).

We do not include the high altitude Izana site in our statistics. It was mistakenly included in the table in Appendix A1 and has been removed from the table.

Comments to the responses

36 these maybe convective regions ... but a much more important element is that these are regions with fine-mode aerosol maxima (by wildfires and pollution).

45 make sure to pick regionally representative AERONET dust site (exclude mountain sites).

The Izana site was not included in the original paper's statistics. However, Table A1 has it listed as a member of the North Africa group. It has been removed from the Table A1. (The map of AERONET sites, Figure 7, shows the Cape Verde site which is stated as being 60m above sea level.)

47 if AOD is correct, but the dust size (and solar absorption) is underestimated, then a model yields too high solar irradiances at the surface

We have added a small table and discussion in the new Section 4.2 showing the dependence of SW and LW irradiance down at the surface on dust particle size.

152 OPAC is parts in outdated (e.g. too much dust absorption, too little BC absorption) ... but more importantly, how is OPAC applied? I assume (if so state that) that MODIS AOD are compared to simulated AOD and differences are expressed via component mass corrections by applying OPAC aerosol type based Mass Extinction Efficiencies– assuming locally/monthly fixed aerosol component mixtures.

Table 2 shows how the aerosol properties defined in the MATCH model are transferred to the Langley Fu & Liou radiative transfer code (LFLRT). The OPAC properties are defined (as tables in LFLRT) spectrally for the indicated constituents. MODIS aerosol optical depths are assimilated into the MATCH model using modeled mass and OPAC mass extinction/scattering coefficient. Then MATCH optical thickness is scaled by the ratio of MODIS/MATCH AOD. This scaled AOD is passed into the radiative transfer code and used as a weighting function for the properties defined in the LFLRT. Optical properties of aerosols in the radiative transfer modeling are also computed by those listed in Table 2.

158 Sinyuk (2003) dust RFI-imag values are much better than those in OPAC ... also you may note that in his paper the coarse mode dust effective radius (in Table 1) is ca 1.5um (number mode radius of 0.63um and stddev of 1.72).

We use the Sinyuk properties for dust in the Langley Fu & Liou Radiative Transfer model.

182 as from AOD differences aerosol component mass is adjusted ... this of course requires mass extinction coefficients for aerosol type (which also include water uptake). Are these coefficients based on OPAC and if so have they been checked for realism?

Yes, those are from OPAC, d'Almeida, and Sinyuk et al. We check "realism" and use d'Almeida and Sinyuk et al. for Sea salt and dust (please see Table 2).

205

222

errors in the captions: "thickens" and "left)" and "right)" ... and eastern EU / west Asia

Thank you, that error has been corrected.

230
there is no plausible explanation why MERRA is so much smaller than MODIS and why MATCH over many land regions is even larger than MODIS (based on Figure 2 differences I am surprised to see in Figure 3 that the global average of MATCH is smaller than MODIS? Also, in Figure 4 clear and all-sky MATCH-MODIS differences are positive, which is inconsistent with Figure 3 means).

We used MERRA-2 for all-sky AOD comparisons because MODIS can only provide clear-sky AOD. The values in Figure 2 are a bit deceptive. Looking closely at the color bar one finds that the light green color is, in fact, slightly negative and light green dominates much of the globe. So, while the large positive (red and orange) biases catch one's eye, the slightly negative values dominate in the global average

265 I suggest to replace 'convective' with 'fine-mode AOD maxima'

The term “convective region” is a general term. We kept ‘convective’ in the revised version.

283 Anomalous high AOD by MATCH at near overcast conditions (as shown in Figure 5) will not matter much for solar irradiances at the surface, when clouds reflect most of the solar energy... or? So for solar irradiance all-sky AODs (and ‘averages’) are less meaningful that clear- sky AODs.

In the SYN1deg product we compute both clear-sky fluxes (cloud-removed) and cloudy sky (aerosol removed) to assess aerosol effect on clouds. Therefore, cloudy-sky AOD does matter.

I do not see that “Match-Modis differences are smaller than Merra-Modis differences”. And

This is the case in the global mean values.

340 Many aerosol types have, if not monthly, so at least seasonal maxima, so an evaluation to AERONET (in Table 2) on a seasonal basis would be much more insightful ... even better with a separation into fine-mode and coarse-mode AOD.

This has been done at three desert sites in section 4.2.

387 ... so what is the conclusion? Spell it out. MODIS AOD are likely overestimated without QAC so that MATCH is relatively high compared to MERRA. But this does not answer the question why MATCH is significantly larger than MODIS over many continental regions as shown in Figure 2.

Added the statement “, likely increasing MATCH AOD overall.” To the text in section 3.

393 If MATCH (via MODIS) misses large aerosol events, why are AOD values over continents (incl. wildfire regions) so large?

As described in the text, MATCH will not assimilate an episodic event until after it has been observed by MODIS. It does not necessarily miss the event but there can be a time lag depending on clouds. These episodic events are not considered to be the main reason why MATCH AOD values are large over continents.

422 the AOD data presented in Figure 9 are maxima (if I read the captions correctly). Why are not median and interquartile-range values are presented instead? And is there a direct sample link between AOD and PV of just max statistics?

The values are not ‘maxima’. The AOD and PW values have been scaled to the largest value between observed and modeled values to place the points on the same plot and keep relative magnitudes between values accurate.

426 explain 100% post and neg error in the captions. If the guess is 0.2 ... then at 100% the lower bound is 0.1 and the upper bound 0.4, which would give an asymmetry not shown in Figure 10. Also, it would be nice to show results for a more absorbing (dust or wildfire) aerosol, because the error max at sza near 0.5 will vanish and will more simply decrease with insolation. Still, I do not see the value of this figure as not even an AOD value is given in the captions (as usually AOD uncertainty wants to be translated into W/m2 irradiance error). Why do I have to read the text to get the info and with those definitions even negative AOD are possible.? Figures and captions should be self-explanatory! But do we need section 3.1 other than stating AOD error as function of aerosol type and SZA... all of which could be placed in a table.

We have changed the legend on the plot to be more descriptive and rewritten the text in the figure caption.

486 CERES detects SW (and LW) upward radiation (and not solar 'irradiances' as at the surface).

Correct, irradiances at the surface are the result of radiative transfer calculations, not observations.

493 Aside from AOD and precipitable water there are other knobs for tuning such as (aerosol absorption, aerosol (dust) size and aerosol elevation, surface albedo). Just tuning with global averages may provide globally the correct result for the wrong regional/seasonal regions. Thus, a regional evaluation is strongly encouraged. Later surface albedo changes are included ... so are these not elements in the global adjustments, as they seem to be higher everywhere in Table 4

The adjustments discussed here are done for the CERES EBAF-surface product. We include the discussion as AOD is one of the variables that may be tuned in the process and thus adds insight into the aerosols coming in from the MATCH model. These adjustments are done regionally (1deg by 1deg) for a monthly time series and so indicate larger, gross characteristics of the data product.

500 to increase the shortwave reflection in terms of aerosol properties also less aerosol absorption and a smaller aerosol type (relative more fine-mode AOD) helps

We do not have the capability to adjust aerosol particle size and type independently in the tuning process, only total column AOD.

508 MATCH AOD is already high at mid-latitudes ... even to MODIS, so an additional 0.02 AOD seems to go the wrong way.

MATCH is higher than MERRA-2 over high/mid latitudes but comparisons with AERONET show near zero or slightly negative comparisons. It is within the context of the AERONET comparison that we make the statement that additional AOD (as found in the EBAF-surface tuning) is not unfounded.

510 there is no 'top right' in Figure 10

Thank you. The text should have referenced Figure 12 in the original manuscript (now Figure 13). It has been corrected.

519 MODIS AOD over oceans are already likely too high, so that further increase cannot be justified.

Adjustments through tuning are regional, not global, for the EBAF-surface product. We do not know the reason for the reviewer saying that MODIS AOD over oceans are too high.

534 I miss in Table 4 the clear-sky upward MATCH based SW (and LW) uncorrected upward fluxes (to compare to CERES based TOA obs) and the clear-sky downward MATCH based SW (and LW) uncorrected downward fluxes (to compare to BSRN/buoy data).

Table 4 (now Table 6) shows results for EBAF and EBAF-surface fluxes along with adjustments made during the EBAF-surface tuning process. It is not meant to be a validation table but to add insight into the AODs provided by the MATCH model. SW and LW irradiance bias/standard deviations at surface observation sites are found in figures 13 and 14 where we now cover the surface validation in sections 4.1 and 4.2.

Reviewer – 2

Thank you for your response and please forgive my delay in re-reviewing. The authors have replied to each one of my comments, covering most of them, even though not as thoroughly as I'd like for some of them. The revisions to the manuscript were clearly as limited as possible, but passable. Please find below my comments on the revision, where "Points" refer to the point numbering from the authors' response to my earlier suggestions and "Lines" refer to the new version line numbers. I consider the unreferenced earlier "Points" as covered satisfactorily.

Point 20) Previously I suggested that l. 447 should read "...scale nearly linearly with AOD between...". Or maybe I was mistaken, and the initial meaning was that the scaling was linear with cos(sza)? Anyway, the authors said that it is fixed, but it is not really.

The calculations scale nearly linearly with cos(sza) as the reviewer says. We have added "with cos(SZA) " to the text.

Lines 472-473 and line 532: Now the biases changed to 3 and 15 W/m2

Thank you for catching that. The numbers have been corrected to the values in Figure 13.

Point 22) The sentence is not removed, even though the authors say it was.

Thank you. That paragraph has been rewritten to reflect the flow of the paper.

Point 26) I think that the authors' point would benefit from including parts of their response to my comment in the manuscript. Even though the authors' response has assuaged somewhat my concerns, I still think that their approach here is more of a sensitivity study than a rigorous proposal for AOD, albedo, water vapor corrections to their product. I am not convinced that possible errors in aerosol types should not be included in the adjustments. My suggestion here is to convey more strongly to the reader that these modifications are one possible solution between many and not the most probable one.

First, we do not suggest the results of the tuning solution as a rigorous solution for the input parameters and so in a sense, the reviewer is correct, it does provide an idea of the sensitivity of the TOA irradiances to the inputs. Secondly, each tuning variable requires, for the Lagrange Multiplier (LM) solution, an estimate of the uncertainty of that parameter, essentially on a global scale. Aerosol types vary dramatically from region to region of the globe. This would greatly complicate the LM solution and it is also not clear that such estimates of aerosol type uncertainty exist.

Point 28) Although the authors claim to have fixed this, it is still wrong. Not only the fig. number but I think they mean "left" instead of "right". Also, the bias now is 11 and not 12 W/m2

Thank you, these errors have been corrected in this latest version.