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Comment on acp-2021-283

Anonymous Referee #3

Referee comment on "Evaluation of aerosol optical depths and clear-sky radiative fluxes of the CERES Edition 4.1 SYN1deg data product" by David W. Fillmore et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-283-RC2, 2021

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.

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

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

ll. 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?

l. 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.

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

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

l. 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.

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?

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?

ll. 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.

l. 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)

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

ll. 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.

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.

Fig. 4 and relevant text: In l. 244 you mention "March 2000 through February 2020", which also seems to agree with the colorbar 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.

ll. 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?

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

l. 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.

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

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

l. 440: I think it should read "scale nearly linearly with AOD between"

l. 440: It should be Fig. 10 instead of 8

l. 468: Moreover, the polar irradiance is small, so it make sense to see the smallest overestimations.

l. 469: Or overestimation of the asymmetry parameter? Or understimation 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.

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

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

l. 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.

l. 502: You probably mean Table 4

l. 510: You probably mean the top left plot of Fig. 12

l. 511: Here too, Fig. 12

l. 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

l. 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.