Reviewer #2

We would like to thank the Reviewer for his/her constructive comments that helped us to improve the quality of our work as well as to clarify misleading points. Our replies (regular font) for each comment (bold font) are provided below.

This manuscript describes the methodology to obtain a dust aerosol optical depth data set from MODIS total AOD combined with the use of MERRA-2 to determine the dust fraction in the AOD. This opens nice perspectives, offering specificity to the aerosols retrievals and this with global coverage once per day, nice horizontal resolution and a long time series.

Although the concepts at the basis of this work, the goal and the obtained data set are scientifically very good, the manuscript itself needs major revision (and I am willing to review the revised version if the Editor finds it needed). The manuscript is very long and some parts are pretty difficult to read, being very descriptive with many numbers. Some parts do not bring a lot to the manuscript, while being quite long. Also, there is a lack of consistency in terms used to refer to the products (see below), which renders the reading a bit difficult. Ideas to improve this can be found in the different comments.

We would like to thank the Reviewer for his/her positive opinion about the scientific contribution and the importance of our study. Regarding the issues raised here, we would like to inform that we have made a major effort to reduce the length of the manuscript and shrink parts of the text in which many numbers are given. Moreover, a better clarification of the used/obtained datasets has been made thus addressing inconsistencies mentioned by the Reviewer. Our detailed replies are given in the relevant comments listed below.

I have some major general comments then addition specific major comments, then some minor comments (editing / suggestions).

Major general comments:

1. **There is no mention of the thermal infrared (TIR) based DOD data (SEVIRI and IASI - for IASI data is available in the climate data store). These are very interesting as the TIR is only sensitive to dust, but gives DOD at TIR wavelength which needs to be converted to visible (step that includes some assumptions on particle size and properties, but also a bunch of assumptions are needed in this work). I am not saying the study should be redone with a full comparison with TIR dust data (although that would be pretty interesting, see also a further comment on the comparisons undertaken in this work), but that when trying to obtain pure dust AOD one should at least mention the TIR DOD. For example, after lines 110-113 it would be nice to have some sentences describing what MIDAS data brings in addition to the TIR-based DOD (for example IASI is also long-term, global twice per day instead of once, and 12km ground resolution at nadir). To be perfectly clear, this is not me being skeptical about the scientific interest of this work, but I think that some information on other methods to obtain DOD from satellites should be added to the manuscript.**

We have added in the revised manuscript the missing information about DOD retrievals operating at TIR wavelengths, as correctly pointed out by the Reviewer. We find very interesting the idea of comparing MIDAS DOD against those provided by IASI and SEVIRI. Actually, it is a very nice perspective for a further exploitation of the MIDAS dataset!

2. **Data from CALIOP and MODIS are used, but there is a confusion as to which data exactly. Indeed, the authors use for CALIOP either the "official" CALIOP product from NASA, or the LIVAS product that some of the authors have previously developed, but both are referred to as "CALIOP", making it pretty difficult to keep track of things. It is a little bit the same for the MODIS "official" AOD product and the MIDAS here developed product, it needs thinking to be sure which one is referred**

to in the manuscript. I recommend to use the product names everywhere in the manuscript, to avoid any confusion: wherever referring to the "non-official" product, please use consistently LIVAS and MIDAS, while keep the instrument name for the "official" products. This also includes the plot titles, legends and caption.

All the necessary replacements, as suggested by the Reviewer, have been made throughout the revised manuscript (text, plots and captions).

3. **For the MERRA-2 dust fraction, please always use the acronym defined (MDF) or at least the same words, avoid using dust "portion" or other terms, for consistency and clarity.**

We think that it is pretty clear to the reader that MDF and MERRA-2 dust portion (or fraction) have the same meaning and there are not consistency or definition issues. However, in the revised manuscript the number of MDF "instances" has been increased at the expense of those of "dust portion (fraction)" trying at the same time to avoid the usage of this term very frequently which makes difficult, to our opinion, the readability of the text.

4. **Why do you use only the MODIS data from Aqua (and not Terra)?**

The obvious reason is that Aqua and CALIPSO are flying in the A-Train constellation which means that MODIS and CALIOP retrievals are almost coincident in temporal terms. This ensures that time departures between these two spaceborne sensors are not affecting our results in contrast to Terra which flies three hours earlier than Aqua. Nevertheless, the MODIS-Terra L2 data currently are processed and the derived MIDAS netcdf files will be uploaded as soon as possible in the same repository.

5. **Why old versions are used both for CALIOP and AERONET while the new versions exist for some time now?**

A same (similar) comment has been raised by the Reviewer 1. We are copying our reply below.

In the revised manuscript we have used the MODIS-Aqua C061 data as well as the AERONET Version 3 retrievals. Moreover, the temporal availability of the MIDAS dataset has been extended from 2007-2016 (10 years) to 2003-2017 (15 years). Therefore, the major comment raised by both Reviewers has been addressed adequately to our opinion. For the evaluation of the MDF we have used the CALIOP data which have been post-processed from our group and are provided via the LIVAS database (Amiridis et al., 2015). In the submitted manuscript, they are stated (Lines 248 – 250) the published works describing the methodology for the derivation of the pure dust product (accounting for dust plus its portion from dust mixtures; Amiridis et al., 2013) as well as the series of filters applied in order to analyze only the quality assured CALIOP profiles (Marinou et al., 2017). The aforementioned techniques are also briefly discussed in our manuscript (Section 2.3). The in-house developed LIVAS database has been built using CALIOP V3 data and its temporal availability spans from 2007 to 2015. Currently, we are working on the development of the updated LIVAS database, spanning from 2006 to 2020, in which the CALIOP V4 profiles are used.

6. **I think that there are too many descriptions of different data sets and of different comparisons, each time with a long description of the geographical features. This makes the paper a bit difficult to read.**

In the revised manuscript, better clarifications are given in order to avoid any confusion to the reader. In our study we have used four datasets (MODIS-Aqua, CALIOP, MERRA-2 and AERONET) which are utilized as follows. MODIS-Aqua AOD and MDF (i.e., MERRA-2) are combined in order to obtain MIDAS DOD. The evaluation of MDF is made against LIVAS, which has been developed based on the CALIOP profiles. MIDAS

DOD is compared versus MERRA-2 and LIVAS DODs while it has been evaluated against AERONET DODs, extracted according to the methodology described in Section 2.4.

7. **Many numbers are given with too many digits. Please try to only provide significant digits.**

We have reduced the number of decimal digits.

Major specific comments:

1. **Line 33: "ground-truth AERONET-derived DODs" -> There is no "truth", any measurement has uncertainties and biases. In particular, here the DOD derived from AERONET is a complex product with a number of assumptions and no-one should see it as "the truth".**

We agree that the word "truth" is not appropriate and we have remove it.

2. **Section 2.3 on CALIOP: It is a bit unclear to me how the CALIOP subtypes are used in LIVAS. I have the feeling that LIVAS is a different retrieval, not using the CALIOP "official" features/retrievals and therefore I would recommend to only mention here what is really needed to understand LIVAS. Otherwise it is a bit confusing.**

Section 2.3 consists of two paragraphs where in the first one, a brief discussion about CALIOP retrievals and products is given while in the second one the main steps for the derivation of the post-processed CALIOP-related pure dust product, available from the ESA-LIVAS database, maintained and hosted at the National Observatory of Athens (NOA), are intentionally briefly described because all the relevant publications (Amiridis et al., 2013; 2015; Marinou et al., 2017; Proestakis et al., 2018) are already provided. A short comment that may can help in the clarification of any misleading points. LIVAS is the outcome of the post-processing of CALIOP profiles by applying: (i) "corrections" on dust lidar ratio (Amiridis et al., 2013), (ii) a discrimination technique for "extracting" dust aerosols from dusty mixtures, (iii) a series of quality control filters (Marinou et al., 2017) and (iv) aggregation into Level 3 outputs (Tackett et al., 2018).

3. **Section 3.1 on the methodology: Do you see a discontinuity in the MODIS DOD linked to changing of MERRA-2 grid cell?**

The spatial variations found in MIDAS DOD are attributed to those of MODIS AOD since the MERRA-2 grid cell has not been changed for the derivation of the fine resolution dataset. For the intercomparison among MIDAS, LIVAS and MERRA-2, all datasets have been regridded to 1° x 1° pixels for consistency reasons without noticing pronounced features, discontinuities and abrupt changes in the DOD geographical patterns.

4. **Lines 330 to 332: "Our approach avoids on purpose the inclusion of additional optical properties providing information on aerosol size (alpha) available from MODIS and absorptivity (Aerosol Index) from OMI that are characterized by inherent limitations". -> This is probably very unclear for the non-specialist reader and comes a bit out of the blue. If OMI is mentioned, the authors should at least explicit why using OMI would make sense when looking for dust aerosols and what are the potential drawbacks with it, other than data availability.**

We have added a reference (Torres et al., 1998) describing the theoretical background. We think that this clarifies the published approach that positive AI values are associated with the presence of absorbing mineral particles. Even higher AI levels are found when biomass particles are probed. There are numerous studies which have been relied on AI thresholds (usually above 1) for the monitoring and tracking loads of absorbing particles. The theoretical background of the UV retrievals is given in Torres et

al. (1998) while updates of the applied OMI algorithm are described in Torres et al. (2013). Likewise, a nice overview of the OMI products can been found in Torres et al. (2007).

5. **Lines 359-361: "Here, we are using the same equations replacing AERONET AODs with those given by MODIS. This relies on the fact (results not shown here) that their averages are almost unbiased." -> A bit unclear. Average of MODIS AODs unbiased wrt AERONET? If yes, this feels a bit short and at least a reference to the MODIS validation should be given and this should be discussed. A quick search (https://doi.org/10.1016/j.atmosenv.2018.12.004) showed me that indeed on average along many years and globally the MODIS mean bias wrt AERONET is particularly low. However, at regional scale this is not always true. In particular, over the dustiest regions (N Africa and Middle East) it seems that there are more outliers in the comparisons linked to a more difficult AOD retrieval from MODIS. I am not saying that the MODIS AOD should not be used to estimate the uncertainty on the AOD, but that it should be discussed a bit more.**

We have rephrased this sentence as follows.

"*This relies on the fact (results not shown here) that their averages from a global perspective are almost unbiased; however, at regional level, small negative or positive offsets (lower than 0.05 in absolute terms) are recorded in the vast majority of AERONET sites, thus supporting our argument.*"

6. **Equations 4 and 5: Those do confirm my thoughts in the previous comment. Levy et al (2013) write that (for DT land) "69.4% of MODIS AOD fall within expected uncertainty of ±(0.05 + 15 %)." This is, I guess, the origin of equation 5 here (and something similar can be found for equation 4). This means, to me, that DT land has a mean bias of 0.05 wrt AERONET, in contradiction with the sentence above saying that there is no bias.**

Please see our previous reply.

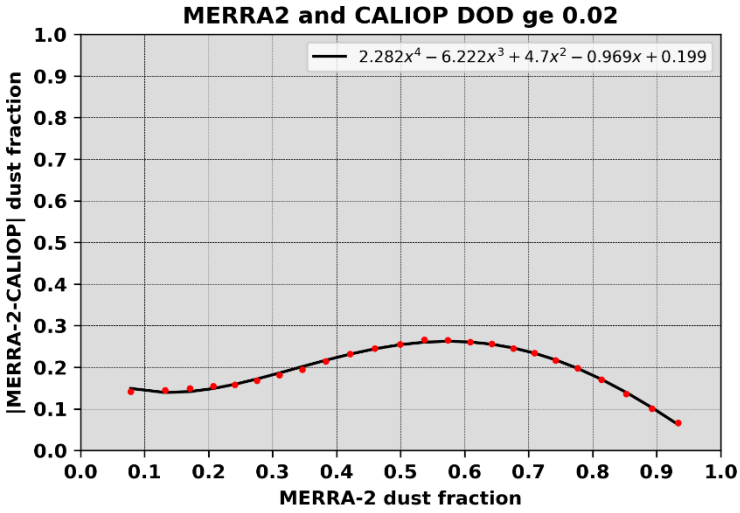
7. **Equation 8: I would appreciate a plot here (of the data that lead to this equation), both to show how good the fit is (does it really need such a complex polynomial curve?) and show some values. I computed them myself from the equation to have a feeling. [MDF uncertainty]: [0 0,2]; [0,1 0,14]; [0,2 0,15]; [0,3 0,18]; [0,4 0,22]; [0,5 0,25]; [0,6 0,26]; [0,7 0,24]; [0,8 0,18]; [0,9 0,1]; [1 -0,01]; Those uncertainties are not negligible (especially at low MDF), however they are not discussed at all.**

The requested plot along with the relevant text (copied from the submitted manuscript) are provided below.

"The CALIOP DOD-to-AOD ratio is our reference for estimating the uncertainty limits of the MERRA-2 dust fraction (MDF). The analysis is performed at 1° x 1° spatial resolution considering only grid cells in which both MERRA-2 and CALIOP DODs are higher to or equal than 0.02. According to this criterion, more than 450000 CALIOP-MERRA2 collocated pairs have been found which are sorted (ascending order) based on MERRA-2 MDF (ranging from 0 to 1) and then are grouped in equal size bins containing 20000 data each sub-sample. For every group, we computed the median MDF (x axis) as well as the 68th percentile of the absolute MERRA-2 – CALIOP dust fraction (y axis) and then we found the best polynomial fit (Eq. 8)."

The geographical distributions illustrated in Figs. 1 and 2, along with the plot given here, indicate that the MDF uncertainties are not negligible and at the same time reveal that they are higher over areas where the dust loads do not dominate in the total burden. Based on the uncertainty analysis, it is apparent that the relative error gradually decreases from ~50% to ~10%, for increasing dust fraction, when MDF values higher than 0.5 (or 50%) are considered. For lower MDF levels (<0.5), the performance

of MERRA-2 in reproducing dust fraction downgrades which makes sense since the dust "signal" is weak over far distant areas or regions that do not affected by dust transport. The relevant discussion about the performance of MDF is thoroughly discussed in Section 4.2 of the submitted text.



8. **Section 4.1 (and also in the conclusion): I do not at all understand this section. Why should the MODIS and MERRA-2 AODs be different? That would underline problems in one of the data sets (at least). And why do they need to be different for this analysis to work? I would say the opposite, that MODIS and MERRA-2 AODs should be similar enough to allow for this work to be relevant. Overall, I find this section 4.1 quite confusing, I am unsure what the authors are trying to show and how it fits in the rest of the paper. I would better see here a short summary of the MERRA-2 AOD validation (with references). And then a short discussion how this will impact the MIDAS data set. Also, this section contains discussions linked to the MDF (dust emission in GOCART for example), which should be moved to the next section.**

As suggested by the Reviewer, we have totally removed Section 4.1 from the revised manuscript and we have kept only parts which fit to the revised text.

9. **Lines 455 to 457: Is the MIDAS DOD expected to be overestimated because the GOCART model overestimates dust emissions?**

In theory yes but that is why we decided to use also LIVAS. In addition, it is not easy to give a sufficient answer because the number of AERONET stations in dust emission areas are limited. For example, in N. Africa, we found slight positive bias [0.0, 0.02] in the Tamanrasset site. Regarding dust transport, there are several factors (e.g., size distribution, winds, identification and activation of the sources, etc.) that can affect the representation of dust burdens by MERRA-2. According to the global DOD maps (Figs. 5 and S6 in the revised manuscript), the agreement of MERRA-2 with respect to LIVAS and MIDAS is good both on annual and seasonal scales. We would like to point out again that from MERRA-2 we are using the dust fraction which is determined by the ability of the model to reproduce accurately both dust and non-dust aerosol species.

10. **Figure 3: High resolution figures are needed. Here if I zoom in (to see details discussed) it becomes blurry**

The quality of the produced png files might have been affected by the conversion of the word file to pdf. However, in order to improve the illustration, we have increased the dpi from 300 to 600.

11. **Lines 485 to 490: I don't understand. If there is a bias of about 10% but other metrics show the algorithm performs well, then why is there a bias? This should be explained also in the manuscript.**

By default, mean bias is affected by outliers in contrast to FB and FGE which are more "smoothed". Some short clarifications along with a reference of the evaluation metrics have been added in the revised manuscript.

12. **Lines 493-494: the correlation between MERRA-2 and CALIOP (LIVAS??) is less good over dust source regions due to the high variability. This is linked by the authors to a poor behaviour of the model in these cases. Can't we also imagine that CALIOP is not perfect there, as the very thin ground coverage makes it miss many events? This is discussed a bit further (lines 521 onwards), but I think it would be good to also mention around lines 493-494 that CALIOP (LIVAS?) is also not perfect.**

We don't think that the narrow footprint of CALIOP at the ground plays such an important role since dust plumes are in general spatially wide (i.e., covering a whole 1-degree grid cell). Definitely CALIOP does not provide the "perfect retrievals" and the main reasons causing a departure from the "ideal scenario" are mentioned mainly in Section 4.2 as well as in relevant parts of the text.

13. **Lines 533-534: the underestimation of CALIOP with respect to AERONET, is it the official product or LIVAS? Here, in this section, it is very confusing. I think most of the section refers to LIVAS but this specific sentence to the official product. If this is indeed the case, then I do not see how this information (and the discussion following) is useful here in the paper, where LIVAS and MERRA-2 are compared. That discussion is already in section 2.3 in a different formulation.**

Between lines 533 and 542 are mentioned two important factors which can hamper CALIOP's performance in reproducing columnar DOD. The first one is related to the dust lidar ratio (LR), which has to be used for the derivation of the extinction profiles (resulted from the multiplication of backscatter coefficients with lidar ratio) and subsequently are vertically integrated in order to get the columnar DOD. This is what actually has been proposed in Amiridis et al. (2013) while in the global map of Figure S1 are depicted the most "representative" lidar ratios applied in our study. The second factor is related to the total attenuation of the laser beam by mineral particles accumulated at very high concentrations and this means that the columnar DOD will be underestimated under these cases since there are not available retrievals throughout and beneath the very thick dust layer (Konsta et al., 2018). Summarizing, the impact of the first factor has been mitigated in the LIVAS database (i.e., selection of more realistic LR than the universal values used in the raw CALIOP data) while for the second one cannot be done nothing.

14. **Lines 558-560: Does this mean that overall, only 10 to 20 CALIOP measurements per grid cell were averaged along 9 years? If yes, this is very low and I don't think it can be considered representative.**

The number of CALIOP L2 profiles (5km resolution) aggregated for the derivation of each LIVAS 1° x 1° grid cell varies from 1 to 24. Overall, we have ~3.4 million of MERRA2-LIVAS pairs (i.e., total number of collocated 1° x 1° grid cells) and we show how these are distributed among classes defined based on the number of CALIOP L2 profiles falling within the 1° x 1° grid cell. A short clarification has been added in the revised manuscript and it is given below.

"Figure S3-ii displays the long-term averaged geographical distribution of the number of CALIOP L2 profiles (up to 24) aggregated for the derivation of the LIVAS 1°x1° grid-cell."

15. **General on section 4.2: this section is quite long, it contains the description of the differences and some discussion about the origin of those differences, but no discussion on the implications of underlined shortcomings on the MIDAS data set? In particular, the underestimation of MERRA-2 over dust sources should be discussed in terms of "how will it affect the MIDAS DOD".**

As we wrote in our introductory response, we have made an effort to reduce the manuscript, shortening also Section 4.2. Regarding MERRA-2 underestimations over dust sources, we think that our reply in a similar comment mentioned above is valid also here. Focusing only over sources, the underestimation of dust emission by MERRA-2 most probably has minor impact on our results since the amount of mineral particles will be lower but their portion (i.e., MDF) to the total load (in optical terms) will be rather stable and above ~90% considering that the contribution from other aerosol species is very small or even zero. As it concerns dust transport, the situation becomes more complex because an accurate representation of the MDF is determined both from dust and non-dust AODs. Actually, our findings (Section 4.1) can be the starting point of a dedicated assessment analysis in which all the factors that affect MERRA-2 dust fraction will be investigated in-depth. Finally, we would like to express our disagreement with the comment "*…, but no discussion on the implications of underlined shortcomings on the MIDAS data set?*". There are several "links" in Sections 4.2 and 4.3 with the obtained findings from Section 4.1 facilitating the interpretation of the deviations found between MIDAS and AERONET (Section 4.2) as well as the disagreements among MIDAS, LIVAS and MERRA-2 DODs (Section 4.3).

16. **Section 4.3: Why redo a MODIS validation against AERONET (not bringing anything new)? I think there are enough papers on that to just refer to one and remove this part, making the paper a bit shorter and less confusing.**

We have removed the relevant part in the revised manuscript.

17. **Figure 5: please change the colour scale for the correlation coefficient as it is now very difficult to see**

Done.

18. **Section 4.4: I think that somehow this section should show what the new MIDAS product brings. The comparisons are currently done in a way that gives the impression it's just another product but not really improved or different from MERRA-2 or the LIVAS climatology. This is linked to the fact that averages over long periods are analysed, so at the end we are just comparing (validating?) climatologies from different products. As MIDAS is not meant to be a climatology, I would not do this kind of comparisons a big point in the paper, but I would instead emphasize what MIDAS gives that those other products can't give. And validate the product at its resolution - but this is done in the comparison with AERONET.**

The powerful elements of the MIDAS dataset, as already have been mentioned in the submitted text, are the almost global coverage, the long-term availability as well as the fine spatial resolution. These features are demonstrated in Section 4.4 where a short discussion about the annual and seasonal global DOD patterns is given. A more detailed climatological analysis is under preparation whereas other studies, relying on the MIDAS dataset, are ongoing and they have already mentioned in the abstract (Lines 35-37), in the introduction (Lines 129-132) and in the last paragraph of the summary.

In Section 4.3, our intention is to make an intercomparison among MERRA-2, LIVAS and MIDAS DODs aiming at assessing the consistency of our product with respect to those provided by simulations and active remote sensing techniques. To our opinion this a very important aspect in our study taken into account the advantages/disadvantages of each dataset. Based on this analysis, we highlighted differences found at specific regions of the planet while through the intra-annual plots for each subdomain the obtained deviations among the three datasets are illustrated and interpreted. Finally, the length of the section has been reduced after refining the submitted document.

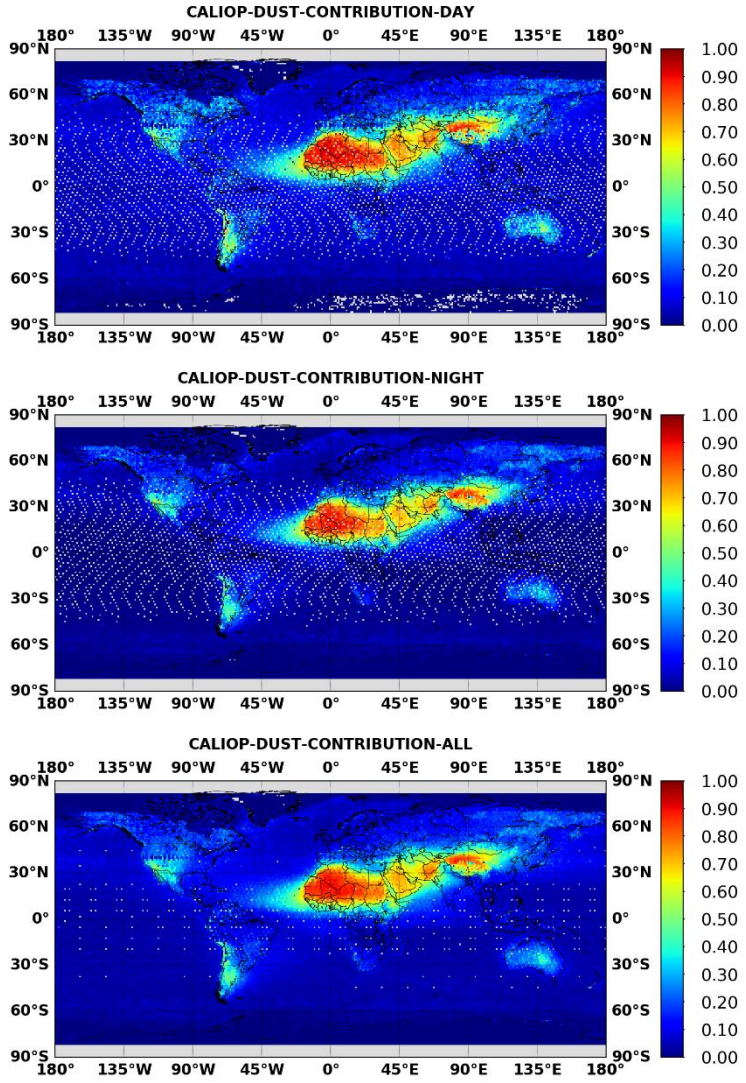
19. **Line 648-649: "the study period extends from 2007 to 2015, driven again from CALIOP's temporal availability" -> this is very confusing. . . CALIOP is still running. . . so the authors probably mean**

LIVAS availability. This is only one of the many examples where it is not clear which data is referred to, leaving the reader in possible misunderstanding.

We agree with the Reviewer and the appropriate corrections have been made.

20. Figure 6: Why do we see orbit-like features on a 9 years average?

Please note that CALIOP revisiting time is 16 days and it has a narrow footprint at the ground. In addition, a series of quality checks is applied on the raw vertical profiles. Therefore, it makes sense that orbit-like features are evident on the long-term averaged global distributions even though these are representative for a 9-year period. In the following figure, the patterns of the dust fraction at daytime (first) and nighttime (second) conditions as well as throughout the day (third), are given. It is clarified that the same CALIOP data have been used for the reproduction of Figure 6 (in the submitted manuscript) but here a different parameter is processed. It is clear that the orbit-like features are evident when only daytime or nighttime patterns are studied in contrast to the "full-day" distribution.



21. Line 673: "CALIOP underestimates AOD over the Sahara" -> again, I think this is the official CALIOP product, right? So how is it relevant here where comparing LIVAS? Same comment/question further, line 677: how does the CALIOP misclassification of clouds impact the LIVAS product?

Overall in this section I have a feeling that there is discussion of both CALIOP and LIVAS but I can't see which is which and I am very confused as to what is really important for the work presented here.

We agree with this comment and we have clarified all the misleading statements in the text. Most of the cases in which very intense dust layers are misclassified as clouds are encountered over/nearby the sources. Under these conditions, the CALIOP profiles are discarded since they are eliminated by the quality assurance filters. This can be one of the reasons where LIVAS DODs are substantially lower than those of MIDAS and MERRA-2 over Bodele, as already discussed in the document, which consists the most active/intense dust source of the planet. Moreover, the underestimations over the major dust sources as observed in the LIVAS database are directly related to the official CALIPSO product and the undetected dust layers therein, as presented and extensively discussed by Winker et al. (2013). These two artifacts are expected to cause an underestimation of the climatological values (see for example the BOD annual values provided in Table 1) keeping always in mind that the appropriate selection of dust lidar ratios (which is highly variable!) plays a very important (or even dominant) role in potential LIVAS DODs deviations from other relevant products.

22. Lines 686-688: I don't see the point of this sentence

A better clarification is given in the revised manuscript.

"Wei et al. (2019b) showed that MODIS underestimates AOD with respect to AERONET while the maximum MIDAS-AERONET negative DOD differences are found at Ilorin and Djougou sites (Figure 4-iv)."

23. Line 732: any explanation for the local minimum of MIDAS in May? This is very surprising.

Based on the updated analysis (i.e., consideration of MODIS-Aqua C061 data) the local minimum has disappeared.

24. Figure 8: Why is the uncertainty higher off the west coast of N Africa than inland?

The uncertainty of the MIDAS DOD results from the combination of the respective uncertainties of MODIS AOD and MDF. Moving westwards from the Sahara, the MDF uncertainty increases due to the reduction of dust contribution while the oceanic AODs are very reliable (i.e., aerosols are suspended over low-albedo surface). Along the coasts of the Gulf of Guinea, MODIS AODs are very high (probably very overestimated) while the performance of MERRA-2 downgrades. This means that both factors converge towards maximizing the uncertainty of the derived DOD over the area. However, we would like to clarify that in the revised manuscript instead of presenting the uncertainties of the average we are providing the average of the uncertainties and their geographical patterns resemble those of DODs.

Minor comments / suggestions:

1. **Line 55: "Gobbi" -> Gobi**

Corrected.

2. **Reference list lines 87-88 -> This list is clearly not aiming at being exhaustive (which is understandable) but here about half the references (and the newest) are work from the (co-) authors of this paper, while overall I don't think they really do represent half the work on dust aerosols from space, and certainly not recently. Maybe it would be best to cite a review paper?**

We prefer to keep the existing references.

3. **Line 100 correct reference is Di Tomaso. . . She is one of the co-authors. . .**

Thanks for correction!

4. **Line 133: "Finally, the main findings are summarized and are drawn" -> I think this needs rephrasing**

We have changed the sentence.

5. **Line 142: "MODIS is mounted on the NASA's twin polar satellites Terra and Aqua acquiring high-quality aerosol data since 2000 and 2002, respectively, while thanks to its wide swath (∼2330 km) provides near-global observations, almost on a daily basis" -> I think there's something wrong in the tenses**

We have modified slightly the sentence.

6. **Line 207: "Over oceans, are also used AVHRR radiances" -> This reads weird, I suggest avoiding the passive formulation**

It is true that the sentence was not written appropriately. We have improved it in the revised manuscript.

7. **Line 289: "requires the of SSA" -> I think a word is missing**

The missing word has been added!

8. **Line 347: "in which ∼68% of the MODIS-AERONET AOD differences fall within" -> I think it needs rephrasing**

We think that it is correct.

9. **Line 375: "higher or equal than" -> higher to or equal than?**

Done.

10. **Line 391: "These two uncertainty quantities" -> values?**

We are referring to the measurement and standard errors.

11. **Line 398: "On the following sections," -> In?**

We think that both are correct.

12. **Line 405: "since a climatological study it is the scientific topic of the companion paper" -> reads weird. Rephrase? Or at least remove "it".**

We have removed "*it*".

13. **Lines 460: by "enormous number of pairs" do you mean that the histogram contains all the single comparisons and not just the time average comparison? This is unclear in the text.**

The histogram contains all the MODIS-MERRA2 pairs found over the period 2007-2016. This part of the text has been removed.

14. **Lines 471-472: “showing the ability of MERRA-2 in reproducing the integrated aerosol fields.” -> This belongs to Section 4.1 on AOD, not 4.2 on dust fraction**

We think that it is stated very clear the separation between existing evaluation studies of MERRA-2 AOD and our assessment analysis of the MDF.

15. **Line 480: the terms fractional bias (FB) and fractional gross error (FGE) should be a bit explained, those are not so standard statistics I think**

The FB and FGE evaluation metrics have been used in many model assessment studies (i.e., Binietoglou et al. 2015) while they are implemented in the forecast evaluation of SDS-WAS. However, we are providing a relevant paper (Yu et al., 2006) providing the formulas of the aforementioned metrics.

16. **Line 580: “discussed” -> described?**

Done.

17. **Line 582: “while for the derived DOD to check the validity of our approach” -> needs rephrasing**

Done.

18. **Line 583-584: “At first, a short discussion is made on the” -> I think “made” can’t be used in that sense**

We think that it can be used in that sense.

19. **Lines 592-593 “and the consideration of AERONET data.” -> please rephrase**

Done.

20. **Line 600: “slight” -> slightly?**

Done.

21. **Line 600 (end) to 604: I think these sentences belong more to the methods section**

We think that it is well placed there because we are recalling to the reader which is the applied Angstrom threshold and then we are mentioning the applied sensitivity test.

22. **Line 624: “fine DOD on AERONET” -> in?**

Done.

23. **Line 625-626:” but its contribution to the total dust AOD it is difficult and probably impossible to be quantified” -> remove “it”?**

Done.

24. **Lines 662-663: “it is apparent a very good agreement” -> A very good agreement is observed?**

We think that it is better to keep the sentence as is.

25. **Line 671: “relied” -> relying?**

Done.

26. **Line 675: “works” -> work?**

The correct is “works” because we are referring to the studies mentioned in the previous sentence.

27. **Line 679: “All these aspects, most likely met over dust sources” -> Please rephrase**

We think that we don’t have to rephrase this part.

28. **Line 682-683: “Across the Sahel, CALIOP provides higher DODs (mainly up to 0.2) both against simulated and satellite products” -> confusing, CALIOP is a satellite, so maybe use here MODIS or MIDAS?**

We have replaced CALIOP with LIVAS.

29. **Line 792: “since it is not expected the accumulation of dust” -> since the accumulation of dust is not expected**

Thanks for the correction!

30. **Line 909: “AEROENT” (typo)**

Thanks!

31. **Line 910: “assuming that dust loads are mainly consist of” -> please rephrase**

Done.

32. **Line 912: remove “resides”?**

Done.