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Atmospheric Measurement Techniques Discussions

Interactive comment on "ModIs Dust AeroSol (MIDAS): A global fine resolution dust optical depth dataset" *by* Antonis Gkikas et al.

Anonymous Referee #2

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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.

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 sceptical 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.

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.

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.

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

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

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.

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

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.

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

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.

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.

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 $\pm$(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.

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.

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.

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

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

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.

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.

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.

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.

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".

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.

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

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 MERRIS-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.

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.

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

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.

22) Lines 686-688: I don’t see the point of this sentence

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

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

Minor comments / suggestions:

1) Line 55: “Gobbi” -> Gobi

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?

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

4) Line 133: “Finally, the main findings are summarized and are drawn” -> I think this needs rephrasing

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 ($\sim$2330 km) provides near-global observations, almost on a daily basis” -> I think there’s something wrong in the tenses

6) Line 207: “Over oceans, are also used AVHRR radiances” -> This reads weird, I suggest avoiding the passive formulation

7) Line 289: “requires the of SSA” -> I think a word is missing

8) Line 347: “in which $\sim$68% of the MODIS-AERONET AOD differences fall within” -> I think it needs rephrasing

9) Line 375: “higher or equal than” -> higher to or equal than?

10) Line 391: “These two uncertainty quantities” -> values?

11) Line 398: “On the following sections,” -> In?

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

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

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

16) Line 580: “discussed” -> described?

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

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

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

20) Line 600: “slight” -> slightly?

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

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

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

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

25) Line 671: “relied” -> relying?

26) Line 675: “works” -> work?

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

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?

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

30) Line 909: “AEROENT” (typo)

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

32) Line 912: remove “resides”?

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