

Interactive comment on “The Mineral Aerosol Profiling from Infrared Radiances (MAPIR) algorithm: version 4.1 description and validation” by Sieglinde Callewaert et al.

Anonymous Referee #1

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Review of the manuscript entitled “ The Mineral Aerosol Profiling from Infrared Radiances (MAPIR) algorithm: version 4.1 description and validation” by Sieglinde Callewaert et al.

This manuscript describes a new method applied to an algorithm that retrieves mineral aerosol profiles from infrared radiances. The method can retrieve the 3D structure of the dust fields which permits to access both aerosol optical depth (AOD) and the altitude at which the dust layer is located. The manuscript first describes the algorithm and then shows examples of extended retrievals for scenes in June 2018. A detailed comparison is then made with AOD 4 AERONET stations during a multi-year period.

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The comparison proceeds with an estimation of the height of the transported dust aerosol layer and a comparison with LIDAR profiles over M'Bour, Senegal and Al Dhaid In the United Arab Emirates. The presentation of the paper needs to be improved and I have several comments that need to be addressed before this paper can be accepted for publication to AMT. Please note that I do not have the background to judge the relevance of the choices made in the algorithm, hence I will focus my comments in relation to the dust cycle.

Main comments:

The authors indicate the existence of parallel efforts such as the ones described in Clarisse et al. 2019 and by Cuesta et al., 2015 to make mineral aerosol retrieval. Although these efforts are mentioned in the introduction there is no mention to the reader of how the presented work here differ by its methods upon these two other studies. I am not asking for quantitative statements but rather that the readers be made aware of the limitations of each of the studies.

A lot of effort is devoted to improvements in the algorithm so that the screening of the scene is improved and the detectability of dust over desert areas with low emissivity becomes feasible. I was surprised that the dust properties chosen in the retrieval were not looked at critically. With regards to either the particle size distribution, and the refractive indices in the shortwave taken from Volz (1972, 1973) and Shettle and Fenn (1979) there are many studies showing large deficiencies in these description of dust properties. A recent study from Ryder et al. (2018) where the full size distribution of dust is measured clearly indicates that a single mode with an effective radius of 2 μ m and a width of 2.0 accounts only for a small part of this size distribution. Have the authors tried to estimate the influence this could have on their retrievals? The following references Dubovik et al. (2002); Sinyuk et al. (2003); Colarco et al., (2003); Balkanski et al. (2007) and Di Biagio et al. (2019) all show that the refractive indices used here are much too absorbing compared to any dust sample examined over the last 10 years. The same question than above should be addressed by the authors: how does

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a correction in the refractive index use would influence their results and the description of the 3D dust distributions that they provide.

When examining the AERONET coarse mode AOD, I did not see any discussion about the possible influence of seasalt on these AOD. This would be particularly sensitive for marine or coastal sites near sea-level and could explain a good part of the discrepancies at these sites. Please indicate it, or try to estimate how much the total coarse mode AOD could deviate from the dust coarse mode AOD at these sites.

I propose a change in the structure of the text of paragraph 5.3. The description of the Lidar characteristics for the 3 lidars at M'Bour, at Al Dhaid and on-board the space station should have been given on an earlier part of the paper so that the authors focus only on the comparison which is the title of paragraph 5.3. This whole paragraph needs to be better organized and better written if you want to keep the attention of the reader. This paper is relatively long so this part has to be well written.

And last but not least of the major comments, that Data availability statement at the bottom of page 28 is very fuzzy as it is. Since this project is financed by Copernicus, the data availability is mandatory and cannot be delayed in time. How can someone interested in studying this dataset access it?

Minor Comments:

In the introduction, the authors should mention that 3D fields of dust based upon observations are described in Ridley et al. (2016).

Page 6, line 16: please delete the sentence: " Above 7 km there is rarely found mineral dust particles, as is shown by a CALIOP based 3 -D climatology described in Winker et al. (2013)." This statement is inaccurate, many lidar profiles above the Mediterranean Sea show dust plumes above 7 and even 10 km. Hence, saying that dust is rarely seen above 7 km can mislead readers.

Page 16, line 3: you mention that you chose 4 surface stations to conduct a more

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thorough comparison with AERONET, please indicate how these stations were chosen.

Page 16, line 18, please change: Indeed, for the a priori, a monthly climatology over 8 years is used (. . .) To A monthly climatology over 8 years is used for the a priori (. . .)

Page 16, line 31-32: “ The first period is identical to the same time and region used by Kylling et al. (2018): 18..”, please indicate the lat, lon of the region that you mention here.

Page 16; in paragraph 5.2 you should indicate that if the dust layer is situated above 7km, it will be missed by your algorithm.

Page 21 lines 19-20: stating that “ This qualitative analysis of aerosol profiles at M’Bour supports our confidence in the value of the new MAPIR algorithm.” Is not justified since we do not have, as of today, a golden standard for dust profiles to judge when we can be confident in a dust retrieving algorithm. Please delete this sentence.

Page 25 line 14, the work you do here is more an evaluation than a validation since you would need very well defined uncertainties on the dust quantities that are measured to make that validation. I propose that you change the term ‘validation’ to ‘evaluation’ in the title of this manuscript and change the text ‘provide validation’ to ‘evaluate’ in this sentence.

Page 27, line 12, there is a typo that your co-authors should have picked up: the units of extinction should be km⁻¹ and not km.

Page 28, line 8, there is one comma too many, please delete the comma and change the text from: “ In Al Dhaid, United Arab Emirates, almost all dust events that were detected by the lidar during the two-month comparison period, were also seen in the MAPIR data.” To “ In Al Dhaid, United Arab Emirates, almost all dust events that were detected by the lidar during the two-month comparison period were also seen in the MAPIR data.”

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