

Anonymous Referee #2

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1.) The manuscript should be thoroughly checked by someone fully proficient in English before publication.

While I have pointed out some errors on the first few pages, numerous mistakes are found throughout the manuscript.

English for the final version of the manuscript will be checked still by a native speaker. The remarks and requested corrections have been made for a revised version.

2.) The AERONET-based validation study presented at the end of the manuscript is not fully comprehensible to the reader. Fundamentals such as data and station selection criteria, period length and number of scenes used are left unexplained. From the information available in the manuscript it seems as if the study covers only a small data set, so that results cannot be generalized. All of these concerns should be addressed in a revision

Validations using AERONET data have been made with selected cloud free MERIS scenes between 2003 and 2006, which have been used for different projects and campaigns. Most AERONET sites used were located in Europe and North America. Following coordinate list is copied from the retrieval program, giving the AERONET sites, which retrieved values of BAER are given for a 5x5 pixel average around these coordinates of the ground based sites. If a scene was used, containing these coordinates a validation could be made.

LATITUDE	LONGITUDE	NAME OF PLACE
58.580	16.150	Stockholm
54.178	7.887	Helgoland
53.586	9.973	Hamburg
53.05	8.78	Bremen (our Cimel)
51.971	4.927	Cabauw
52.110	4.327	DenHaag
51.225	2.925	Oostende
51.035	2.368	Dunkerque
51.354	12.435	Leipzig
49.999	8.300	Mainz
49.093	8.428	Karlsruhe
47.480	8.351	Laegeren
45.803	8.627	Ispra
45.437	12.332	ISGDM_CNR
45.314	12.508	Venise
44.632	10.945	Modena
28.220	-177.170	Midway
28.660	-16.330	SantaCruz
34.656	22.129	FtCrete
40.335	18.111	Lecce
40.307	7.906	Oristano
-7.883	-14.417	Ascension

36.550	126.267	Anmyon
35.517	12.632	Lampedusa
38.197	15.567	Messina
16.733	-22.935	Cabo_Verde
14.974	-23.484	Praja
8.320	4.340	Ilorin
41.840	12.647	Rome
12.200	-1.400	Ouagadougou
13.541	2.665	Bamizoubou
50.612	3.142	Lille
48.700	2.208	Palaiseau
48.867	2.333	Paris
43.136	6.009	Toulon
43.933	4.878	Avignon
35.550	8.683	Thala
38.530	-28.630	Azores
48.407	2.680	Fontainebleau
37.105	-6.733	El_Arenosillo
38.568	-7.912	Evora
57.917	18.950	Gotland
39.030	-76.880	GSFC
39.283	-76.618	MD Science Center
58.255	26.468	Toravere
35.958	-84.284	Walker Branch
37.942	-75.475	Wallops Island
44.226	-79.750	Egbert
36.900	-75.710	Cove
40.053	-88.372	Bondville
34.023	-81.036	Columbia SC
45.379	-71.931	Cartel X

Action: We prepared a new table 5 with the site names of the AERONET instruments, used for validation purpose and included it to the revised version of the manuscript. The old table 5 is now table 6. The references have been changed. Further we inserted a sentence which time and MERIS data are used.

3. In various parts of the manuscript, the algorithm's focus on the VIS and blue regions of the spectrum is stressed. I would like to point out that the blue is part of the VIS. The sections concerned should be clarified accordingly.

VIS - blue

I agree, that the blue is a part of the visible range. However, the blue range of the visible is the important part of the spectrum for the aerosol retrieval over land. Therefore C. Hsu of NASA call her approach "deep blue". Therefore we'd like to mention the blue part explicitly in the manuscript.

4. The current structure of the manuscript makes it hard for a reader to fully understand the algorithm presented. I suggest presenting the general outline of the technique early on. This might be accomplished by moving section 3.5 forward.

Action: As suggested we moved section 3.5 to begin of section 3 and modified it as an outline of the algorithm. The section and figure references have been adapted in the whole text of the manuscript.

5. Near the beginning of the manuscript, changes in the algorithm over previous versions are mentioned as the main motivation for this publication. However, I could not find any explanations of these differences. What has changed over previous versions? Also, it would be interesting to know if these changes improved performance. A validation study therefore should ideally also include a comparison with the old version (or at least cite results of a similar study performed with an older version).

The main improvement compared with older versions was the use of non-Lambertian surface conditions.

Action: We added a sentence in the introduction mentioning the improvements and a reference for this.

6. All acronyms need to be explained on first use in the manuscript.

Action: The acronyms have been checked for existing explanations and missing explanations have been added to the text

2 MINOR DETAILS

1. Abstract: A sentence on the results of the trend analysis would be useful.

Action: Sentence on trends included in abstract

2. Section 2: The role of surface properties and the need to know them is explained several times. There is scope for shortening the manuscript here.

However, I like to express and discuss the differences, because they are important to understand the different retrieval approaches for land and ocean. The mentioned 'doubling' is used for different aspects. Therefore I decided not to shorten this part.

3. 2110-15 (page 2110, line 15): It remains unclear where the information on air pressure comes from (data source).

Sea surface pressure p_0 is either used from ECMWF data or, if not available, assumed as standard pressure. The pressure change with the height is obtained by the barometric height equation. A sentence is included explaining this.

4. 2110-23: Here you assume a very ambitious degree of accuracy (AOT +/- 0.01, reflectance +/- 0.001). This does not seem realistic to me.

This is not the accuracy of the retrieval. However, if GCOS requirements should be reached by the retrieval such accuracies are required.

Action: I made a reference to GCOS and a comment on real accuracy.
GCOS – Systematic observation requirements for satellite-based products for climate. GCOS - 107, Sept. 2006, WMD/TD No.1338. <http://www.wmo.int/pages/prog/gcos/Publications/gcos-107.pdf>

5. 2111-4: "There is no problem..." – What do you mean?

There is no saturation problem with MERIS and SeaWiFS.

Action: This now mentioned in the text.

6. 2111-8ff: Why do you use the data at these resolutions? (Availability, comparability etc.)

These data with these resolutions are available by ESA or NASA.

7. 2113-7: What are the data sources for temperature and pressure information?

As mentioned before sea surface pressure comes from ECMWF data or is standard pressure, if no ECMWF data are available. The temperature comes of the MPI climatology. For elevated surface the pressure change is considered by barometric height equation and the temperature change is considered by the dry adiabatic lapse rate.

Action: This is explained now in the revised text.

8. 2114-4: "The BRDF parameters to be used are still under investigation." – Can you please provide more detail?

BRDF depends on surface type. Therefore a regional data base or relationships with surface characteristics, like NDVI, should give the variability of the BRDF parameters. This is still not considered and the algorithm runs with a static set of data.

Action: Explanation is added to the text.

9. 2114-14: Here you mention Germany explicitly. Is the algorithm designed/tested for use in Germany only? If so, please mention this prominently. If not, why do you mention Germany here?

For Germany and the surrounding AERONET sites we made a study to find the BRDF parameters by minimizing AOT deviations between BAER retrieval and AERONET data. Therefore the values of the BRDF are taken from this study.

10. 2115-3: NDVI is not a "vegetation fraction".

I agree with you. However it is a parameter, which can be detected from satellite, giving a proxy for the vegetation fraction. Therefore we use the NDVI as tuning parameter of the mixing model.

Action: Mentioned NDVI as proxy for the vegetation fraction in the text.

11. 2116-8: LUT is mentioned here for the first time. So far, the reader does not know of the existence and/or intended use of a look-up table (nor does he/she know the meaning of the acronym). Please restructure the chapter to help the reader better understand the algorithm.

Action: Acronyms are explained now.

12. 2117-18: Why polynomial fit? Why second degree?

The polynomial fit of the radiative transfer calculation enables a fast interpolation for values not covered by the radiative transfer modelling. The polynomial fit of second degree fits well with the results of radiative transfer modelling of aerosol reflectance and given AOT.

Action: A sentence is added explaining, that the polynomials are used for interpolation purpose.

13. 2118-9: What are your conclusions from the data shown in figures 7 and 8?

The main uncertainty of an aerosol retrieval comes from the selection of an inappropriate phase function, because there is a wide range in the lateral and back scattering, responsible for the derived AOT.

14. 2118-16ff: I do not understand this paragraph. Please try re-wording.

Action: Re-worded paragraph:

The application of the algorithm with several test scenes, using different LUT and the validation lead to the conclusion, that LUT No.~6, LACE-98, non-absorbing aerosol worked as an all round LUT, giving close results with AERONET data for the most conditions.

15. 2121-1: There is a sign missing in the equation.

Action: missing \leq is inserted now

16. 2121-8f: "Since channel..." – I do not understand this sentence. Please try rewording.

Action: Text changed into:

Since the MERIS channels 1 to 4 (0.412 - 0.510 μm) with low surface reflectance enable to recognize the spectral slope, these channels are most important.

17. 2122-2: What happens to $\tau_{\text{TOA}} = 0.1$?

Action: $<$ is substituted by \leq

18. 2123: I am not quite sure I follow your cloud identification scheme. The following aspects require clarification:

• **In what respect do you consider clouds as inhomogeneous? While cumulus**

clouds may certainly qualify as heterogeneous in terms of surface reflectance and temperature, I would consider low stratus fields as extremely homogeneous.

For us only reflectance is considered not the thermodynamic status of clouds. Our aim is the exclusion of clouds from cloud free scenes. Therefore only thin cloud or cloud fractions of smaller than 5 % within a scene need to be found and excluded from cloud free pixels, used for aerosol retrieval to avoid a cloud influence in aerosol results.

Thick clouds can be recognized well by the reflectance boarder.

It is to mention, that no one criterion is recognizing all types of clouds. A homogeneous stratus is recognized , if it has a reflectance above the reflectance boarder. For very thin Cs the discrimination gets difficult and then it depend on the colour index only.

• line 10: how do you increase $\rho_{CL_{Min}}$? How do you know this situation is encountered?

For the standard approach we work with $\rho_{CL_{Min}}$ given by A.A. Kokhanovsky. Only in specific cases, if we look for such events like the volcanic ash cloud or the fire plumes of Greek forest fires, then manually this criterion has to be changed.

• line 11: do you look at all individual RGB plots? This would make operational application of the technique extremely difficult.

We calculate from RGB data a colour index, which is used for the cloud identification. Histograms of the colour index have been studied to find out colour index ranges for different targets.

• line 14: why do you expect 'a decreased spectral slope'?

We have an decreased spectral slope, because clouds have a spectral slope of 0. Therefore each even thin cloud reduces the spectral slope of the clear sky atmosphere.

• line 16: here you say 'scenes', whereas figure 10, to which you refer, mentions one scene only. Which is correct?

• If you used only one scene, how do you account for effects due to changing solar geometry (time of day)?

• end of block 2: add "for clear conditions."

Fig. 10 gives an example for such histograms analysing satellite scenes.

Action: We changed figure caption of Fig. 10 and mentioned the case of Fig. 10 als an example.

The last sentence of 2. is modified as: Therefore partly cloudy scenes will be removed from cloud free ones by using the criterion $R > 1.15$.

Action: Instead of including all to the single questions I made a reference to the following paper, which used and tested the cloud screening criteria.

Schlundt, C., Kokhanovsky, A.A., von Hoyningen-Huene, W., Dinter, D., Istomina, L., Burrows, J.P.: Synergetic cloud fraction determination for SCIAMACHY using MERIS. AMTD, 3, 3601--3642,

2010. doi:10.5194/amtd-3-3601-2010. URL = {http://www.atmos-meas-tech-discuss.net/3/3601/2010/},

19. 2124-12ff: You say there is no cloud detection technique meeting your criteria. What about the cloud screening technique used in MODIS aerosol retrieval? Wouldn't it be convenient to use that?

We would like to do it, however MERIS and SeaWiFS do not provide thermal IR channels like MODIS, which can be used for the cloud detection. Therefore we have to use VIS and NIR channels only. However this is mentioned in the text.

20. 2126-1ff: I do not understand the first paragraph. Please try re-wording.

Action: This paragraph is reformulated also on request of reviewer #1.

21. 2126-5: Please explain what is seen in the figures and how it shows the algorithms skill.

Action: Explanation of the figures are added.

22. 2127: In the validation study, please mention the following:

- **What scene/s did you use for the study?**
- **By what criteria did you select them?**
- **By what criteria did you select the AERONET stations? Why didn't you use all stations available?**
- **What AERONET level did you use?**
- **Is the relationship between AERONET and your algorithm statistically significant?**

I hope, all this is now included in the revised manuscript.

23. 2128-16: Here you mention that one particular LUT yielded the best results in the validation study. However, in the validation study there is no reference to different LUTs. Please make your argumentation consistent.

The reworded paragraph 14. 2118-16ff mentioned how the 'best' LUT has been found. With AERONET data the LACE-98 experimental phase function gave best agreeing results, however, not with the modelled test data of A. Kokhanovsky.

24. 2129-7ff: I do not understand how degradation was removed. Please clarify.

For the use of the counts of the SeaWiFS observations the calibration function of the SeaWiFS team in the SeaWiFS calibration file has to be applied. This given calibration function considers the channel degradation as it is shown in Fig. 20. There are different calibrations functions of the different SeaWiFS data reprocessings considering different aging behaviour.

Action: We mention this now in the text.

25. 2129-14: Why do you mention the CITYZEN project here? Is there a specific relationship between your algorithm and this project?

One intention of CITYZEN is to investigate trends of climate and pollution parameters. There the algorithm was used for the trend determination in AOT from satellites.

26. 2129-16: What does the suffix 'big' stand for?

By the modelling groups of the CITYZEN project different regions of interest have been defined. There was one with the BeNeLux countries only (BeNeLux_small) and one including the Rhein-Ruhr-region of Germany (BeNeLux_big). The same was also for the Po-valley region in northern Italy. In this paper we only reported results for the larger regions, because the smaller regions gave no significant different results.

27. 2129: Are the trends presented here statistically significant?

The statistical significance has been investigated with the test of Weatherhead et al.. The results of the significance tests are given in the submitted paper of Yoon et al., 2010.

Yoon, J. M., von Hoyningen-Huene, W., Vountas, M., and Burrows, J. P.: Analysis of long-term trend of aerosol optical thickness using Bremen AEROSOL Retrieval algorithm with SeaWiFS data over regions of interest, ACP, submitted, 2010.

28. 2130-24: Here you mention differences between land and ocean algorithm performance. I did not read about this earlier in the manuscript. Since this is the summary and conclusions section, I do not think you should introduce new points.

Action: I made a small paragraph in the section 3.1. Main steps of BAER:

For ocean targets the an ocean surface is used instead of the land surface and the ocean algorithm is therefore much more simple than the land part. It can benefit of the low water surface reflectance in the NIR channels of the instruments and use therefore all MERIS or SeaWiFS channels. The principle of the ocean algorithm is the same as for the land algorithm with different surface properties. Thus we do not describe the ocean algorithm separately.

3 TECHNICAL REMARKS

- | | |
|---|---|
| 1. 2108-8 (page 2107, line 8): Wide Field sensor... | X |
| 2. 2109-10: need -> needs | X |
| 3. 2108-24: of the AEROSOL source regions are ON land, retrieval... | X |
| 4. 2109-1: ... assumed as "black"; its properties need to be ... | X |
| 5. 2109-16: Since its first publication () several improvements have been made, which will be presented here. | X |
| 6. 2109-21: Since the surface contribution cannot be neglected in an aerosol retrieval over land, it needs to be... | X |
| 7. 2110-10: Generally, land surface reflectance... | X |
| 8. 2110-13: Although Rayleigh path... | X |
| 9. 2110-24: If all other influences WERE known... | X |
| 10. 2111-1: considers that disturbing effects like land... | X |
| 11. 2111-3: importance that the instrument DOES NOT REACH saturation... | X |
| 12. 2111-5: same wavelengths AS SeaWIFS SATURATE UNDER some... | X |
| 13. 2112-14: agreement with reality if ocean conditions in the red and NIR are known. | X |
| 14. 2113-15: "Lambertian" should be capitalized throughout the manuscript. | X |
| 15. 2115-16: sensitivity TO aerosol | X |

16. 2116-19: AOT determination		x
17. 2117: Figure 6 seems to appear before figure 5. Please re-arrange.	Will be done for the final manuscript.	
18. 2118-24: this -> these		could not find it
19. 2119-1: CONSTRAINTS		x
20. 2119-13: RMSD not explained		x
21. 2120-3: last square -> LEAST SQUARES		x
22. 2121-13: sufficiently		x
23. 2129-13: Not a complete sentence		x
24. 2130-2: increased -> improved		x
25. Table 1: Possibly use nm?		no
26. Table 3: ALBEDO		x
27. Table 4: BAER, not BEAR		x
28. Figure 10: boarder?	Substituted by ratio value	x
29. Figure 15: surrounding AREAS.		x

Actions: technical remarks indicated with an x are changed within the revised manuscript.

The modified Text (without figures) of a revised versions is submitted to the editorial office and hopefully will be attached.