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Atmospheric Measurement Techniques Discussions

Interactive comment on "Validation of a modified AVHRR aerosol optical depth retrieval algorithm over Central Europe" *by* M. Riffler et al.

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We would like to thank all reviewers of the manuscript for their helpful comments and suggestions. Before answering the questions raised by the reviewers we would like to add some general comments on changes being introduced with the revised manuscript.

In the discussion paper all AVHRR results were based on a continental aerosol model and we discussed possible improvements by deriving new aerosol properties (depending on seasonal changes and based on AERONET inversion products). In the revised version of the manuscript all results are already based on the new aerosol model and differences to the the old (continental) model are mentioned in the discussion of the results. Therefore, all figures/tables containing AVHRR data have been changed accordingly. In addition, in the mean time we have updated our AEROENT data base

which now results in a higher amount of coincident measurements between the satellites and AERONET. Hence, the statistical results have changed slightly compared to the discussion paper, but the main statements remain the same. We have adjusted the figures and tables accordingly.

The other changes will be discussed below by answering the open questions.

Anonymous Referee 1

Specific comments:

3) Conclusions: As stated above I am not fully convinced of the independence from AERONET as inversion products are used to better characterize ω_0 in the retrieval - this should be clearly stated in the discussion section

ANSWER: See Short Comment SC C94 from "Michael Riffler", 30 March 2010. We will also provide a short comment to clarify this in the introduction and conclusions of the revised paper.

4) Methods and assumptions: the methodology used is explained - in some cases a critical statement on the impact or validity of assumptions made would be helpful

ANSWER: This will be provided in the revised manuscript.

6) Traceability of results: The retrieval methodology is described with enough detail and also the validation undertaken is clearly summarized. I recommend that all elements of the methodology description which are currently distributed over two sections (3, 5.1) should be provided in only one integrated methodology section for easier reading.

ANSWER: All important information treating the retrieval methodology will now be provided in the completely revised section 3 and the validation methodology from section 4 will now be found at the beginning of the new section 4 (results and discussion).

9) Abstract: The abstract provides a good overview of the paper. However, the restriction of the entire study to Europe must be mentioned (not only for one part of the

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validation).

ANSWER: We do not believe that the algorithm is limited to Europe. Herein, we are limited to Europe, since we receive and process our own data. With regard to the time series analysis we already extended the investigated area from Central Europe to the area between 35° N–55° N and 5° W–25° E. The resulting AOD shows the same statistics as presented herein. So, we believe that the algorithm is applicable to all regions with similar surface characteristics, of course, the aerosol properties need to be adjusted to meet the conditions in the new area. To clarify this we will add a sentence in the abstract stating that the algorithm might be applied also in other parts of the world with similar surface characteristics like Central Europe.

10) Structure: As mentioned above I find it confusing that several times a statement like "this will be discussed later in the paper" is made. To ease reading this should be avoided and all relevant information provided in the first place it is important. Also some sections are very short (2.1–2.4, 4 - only about 10 lines) so that they should be better combined with other parts of the paper.

ANSWER: We tried to avoid this kind of expression in the revised manuscript as much as possible. We would like to keep the structure of sections 2.1–2.4, since this gives a quick overview on the data used for the study. Hence, the reader does not need to go through the whole section in order to get an idea about the data included. Section 4 has been integrated in the results and discussion section (new section 4).

Answers to the technical corrections:

p 786/l 6: mention limitation "for Europe"

ANSWER: See answer above for Abstract.

p 786/l10: "thresholds" needs explanation here

ANSWER: In the abstract of the revised manuscript we changed the expression into "other processing steps". More details are provided within the main text and we believe

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that at this point of the paper a more detailed description of these thresholds is not useful.

p 787/l 7: also Kokhanovsky et al., 2009 (Springer) could be added with a more actual overview

ANSWER: Thank you for the hint, we have added this excellent citation (Kokhanovsky and De Leeuw, 2009).

p 787/l 17: "spectral properties" of what?

ANSWER: We changed the whole sentence into "Most studies of aerosol properties from AVHRR focused on areas over the oceans [...], where the spectral properties of the underlying surface are generally well-known and the surface reflectance is low."

p 788/l 8; "assess" - and develop, isn't it

ANSWER: Yes, we have changed the expression.

p 788/l 10: applicability to pre-AERONET era - this needs thorough analysis and discussion of calibration stability - a statement should be added

ANSWER: We are currently investigating this topic and will publish the results in a future paper. A statement about calibration will be added at this place.

p 789/l 9: micro-physical properties are not derived, but used

ANSWER: Corrected.

p 789/l 11: "EXT" - needs explanation

ANSWER: The phrase has been changed in "[...] (morning orbit with equator crossing time EXT$\sim$10:00 a.m., which is the local solar time when the satellite crosses the equator)."

p 789/sect. 2.2: MERIS AOD is not (yet) well accepted in the community and (as also your results) show not a good choice as reference product; also missing thermal IR

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bands and consecutively weak cloud flagging is an issue that needs to be mentioned

ANSWER: MERIS is a modern sensor and the product used herein is the ESA operational standard product. Therefore, we think that it is feasible to compare our aerosol product with the MERIS one, beside the reference product of MODIS. Nonetheless, a comment about the weakness of the MERIS AOD will be added.

p 789/l 18/9: what is the impact on the accuracy of this extended product?

ANSWER: The information found about this in literature is ambiguous. While Santer et al. (2007) report a reduced RMSE in the blue with a slightly higher RMSE in the near-infrared, Vidot et al. (2008) mention that "[...] MERIS processing offers a much better spatial coverage but also introduces more errors in the τ_a and α retrievals." We have added this information in the revised manuscript.

p 788-790: for all products known accuracies should be quoted

ANSWER: This will be provided in the revised manuscript.

p 790/l 2: VISvs2.12 needs explanation for non AOD retrieval experts

ANSWER: This part has been changed into: "For the initial MODIS aerosol retrieval Kaufman et al. (1997b) presented a method to identify DDV pixels by making use of the correlation between ρ_{SFC} at 2.1 µm (corrected for atmospheric effects) and visible wavelengths (0.47 and 0.66 µm). In Remer et al. (2005) the product was extended to land surfaces with ρ_{SFC} (0.66 µm) $\leq$ 0.125. Compared to these prior MODIS products, the current surface reflectance estimation is based on an improved parametrization (ratios between visible and 2.12 µm channels, VISvs2.12), now considering variations by surface type (based on the short-wave infrared normalised difference vegetation index) and angular variability in the VISvs2.12 reflectance relationship."

p 791/l 9: "with a pre-defined aerosol model" - this is one place where explanation is only provided later in the paper which makes it hard to understand here

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ANSWER: We revised section 3.1. completely as stated above. Thus, we are convinced that this should be clearly understood now.

p 792/l 17: fig. 1 has almost all values of ρ_{SFC} higher than 0.07 - so it will be difficult to meet this condition - please explain?

ANSWER: For the revised manuscript we provide a more suitable example from another pixel location which meets the criterion.

p 792/l 25: how many AERONET sites were used?

ANSWER: Nine sites were used. We have added this information in the text and Table 2.

p 794/paragraphs 1 and 2: GlobCover only provides land cover, not aerosol model

ANSWER: We have changed the misleading phrase into "They display scatter-density plots and the linear regression equation for the summary of all AERONET sites listed in Table 3 and five additional locations as examples for different land cover types (GlobCover) and aerosol conditions (AERONET)."

p 794/ first paragraph: an aerosol model must also include size / Angstrom coefficient, not only absorption

ANSWER: We added the information about the size dependence of the particles. In addition, the average and standard deviation of the Angstrom coefficient has been included in Table 1 for every AERONET site.

p 794/l 26: "negative variations" - better: "under estimations" or "negative errors"

ANSWER: This information is not included anymore (see following answer).

p 794/l 27 "boxcar average": what is the impact of this step? ("slightly" quantified!)

ANSWER: Initially, the methodology to derive ρ_{SFC} was tested on a limited data set before it was applied to the entire data. After reading your comment we reran the

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analysis with and without the boxcar filtering using the entire data set. It turned out that the RMSE without the boxcar filter is reduced by 0.01, the other statistical parameters remained similar. Thus, we cancelled this processing step and adapted the results in the manuscript accordingly.

p 795/l 6 and l11/12: other potential reasons should be discussed: time of the day, BRDF, solar elevation, water sediments, ...

ANSWER: The time-of-day should not largely affect the retrieval, at least for satellites crossing the equator at similar sun elevations (despite morning or afternoon overpass), which is also demonstrated by the similar performance of both MODIS spacecraft, despite their different equator crossing times. Small differences may be introduced by evolving cloudiness during the day, especially in mountainous areas. BRDF effects may slightly influence the results, but Fig. 1 in the paper shows that the estimation of ρ_{SFC} accounts for variations in the viewing geometry. Some uncertainty will remain due to changes in sun elevation during the 45-day period used for the retrieval of ρ_{SFC} and due to intra-daily variations caused by varying overflight times. Thus, we also tried to pre-correct the data for BRDF effects by using two different BRDF models (Rahman et al., 1993; Wu et al., 1995), but the resulting ρ_{SFC} turned out to be less accurate (similar or higher RMSE, reduced correlation) than without the correction. In addition, we also tried to shorten the 45-day period for the estimation of ρ_{SFC}, which showed some improvements on locations with low cloud coverage. On the other hand, in regions or during periods with high cloud coverage a shorter time period reduces the chance to get cloud-free observations with low aerosol concentrations. We added this discussion in the revised manuscript.

p 795/l 26: “absorbent” ? wrong word!

ANSWER: Corrected.

p 796/l 8: “derived” - from what?

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ANSWER: We have added the information, that AERONET data were used to get the new aerosol properties.

p 795/last paragraph: only here you explain the aerosol model used - this is too late! Also it remains unclear if is this one fixed assumption with adapted ω_0 per site?

ANSWER: We have added the information about the aerosol model in Section 3.1 (general principle). The aerosol model depends on the season and for each season it is fixed for the entire area. We hope that this information is now clearly described in Sect. 3.1.

p 796/l 11: "Owing to quality reasons" what do you mean by this? Explain!

ANSWER: In the revised manuscript we have changed this expression. Now, we explain the accuracies/errors of the aerosol properties from the AERONET inversion products, which depend on the measured aerosol optical depth.

p 796: so the AERONET data are needed to determine the aerosol absorption - not fully independent as claimed

ANSWER: See Short Comment SC C94 from "Michael Riffler", 30 March 2010.

p 797/l 16: ALPHA is not explained

ANSWER: This is the Angstrom exponent and is mentioned several times before in the text.

p 798/l 9: "insufficient" better: "under-estimated"

ANSWER: Corrected.

p 799/l17: the reason is not the repeat cycle of the ENVISAT satellite (similar to TERRA), but the narrower swath of MERIS instrument

ANSWER: We have changed the text accordingly.

p 801/ section 5.3.3: discussion of different time of the day is needed

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ANSWER: We will provide a short discussion in the revised manuscript at this point, similar to the answer of "p 795/l 6 and l11/12".

p 801/l 22: "What is more" - weak expression p 801/l 23: "absorbent" again

ANSWER: Corrected.

p 802/first paragraph: good N17/N18 agreement despite different time of the day - add a statement

ANSWER: This will be provided at this point in the revised manuscript (see also similar questions above).

p 802/l 8/9: "Although, ..." - this sentence is not complete

ANSWER: Corrected.

p 802: a discussion of different overpass times, calibration and impact of AERONET-determined ω should be added

ANSWER: This will be provided in the revised manuscript.

table 1: you could add columns with mean Angstrom coefficient and land cover per station

ANSWER: We have added the Angstrom coefficient. For the land cover we decided not to include this information, since the land cover product contains several categories, which would result in a table on its own.

table 2 caption: instead "proposed method" better "new method" or "adapted method"

ANSWER: Corrected.

table 3 caption: these are not comparisons with various satellites, but with AVHRR AOD retrievals from them; last sentence better: "Abbreviations for the AERONET sites are explained in table 1."

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ANSWER: Corrected.

table 3 and figure 1: ρ_{SFC} ? add wavelenght/band

ANSWER: Corrected.

language and spelling errors: p 788/l 3: towards p 788/l 9: on AERONET data for correction p 788/l 12: assumptions on p 788/l 21: data validation method p 789/l 5: sites from AERONET p 795/l 8: At the same time p 795/l 12: comparison of monthly p 802/l 21: differences between

ANSWER: Corrected.

Anonymous Referee 2

Assuming the MODIS AOT's are the reference, results (Fig. 5) over the Po river valley are somehow disappointing and not very appealing for the next processing of the AVHRR archive. The authors point out the results of the regression analysis (table 4) for explaining the observed underestimates (by almost a factor 2). In figs. 2 and 3, the comparisons with the ISPRA AERONET site (that is close to the Po River Valley) don?t display such tendencies. A deeper analysis is required: impacts of cloud screening, sampling biases (temporal and spatial) have to be discussed. The reader needs to be convinced that the present method will provide reliable and usable aerosol climatology over the last 20 Years.

ANSWER: Considering the monthly averages, the statistics are also reduced for the AERONET site ISPRA, with a correlation R of 0.71 (0.68) for NOAA-17 (NOAA-18) and a linear regression equation of $y{=}0.08{+}0.71x$ *(*$y{=}0.09{+}0.60x$*). For this particular month, the average* τ_a *derived from the AERONET data at ISPRA equals 0.44. Using the MODIS data results in* $\tau_a{=}0.34$ *for Terra and* $\tau_a{=}0.37$ *for Aqua. Considering the AVHRRs, we get* $\tau_a{=}0.33$ *for both, NOAA-17 and NOAA-18 and, therefore, an underestimation of 33%. Even both MODIS sensors underestimate* $\tau_{AERONET}$ *by 19 and 29%. We assume that this may be caused by sampling issues. Considering the central part*

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of the Po Valley, the large underestimation of τ_a should also be a matter of sampling for the following reason. This region is dominated by bright surface types exceeding the ρ_{SFC} limit of 0.07 more often. The same holds true for the Rhone River delta. Hence, only few observations were available to calculate the monthly average τ_a in these regions. Considering the entire area processed for this study, approximately 11% of the pixels show $\rho_{SFC} > 0.07$, depending on the season. This means that the approach is appropriate for almost 90% of the area. Hence, we believe that despite the above mentioned limitations the proposed approach is useful. The information stated herein and some additional information found by Levy et al. (2009) for spatial and temporal sampling have also been added to the discussion of section 4.3. in the revised manuscript. Considering cloud screening, we made the experience that in general cloud screening misses cloudy pixels and that we are not able to identify subpixel clouds. Both effects will lead to an overestimation of τ_a and, therefore, may not explain the underestimation of the monthly average τ_a.

Alexander Trishchenko

[...] The weakness of Riffler et al retrieval scheme that can be traced to original paper of Hauser et al (2005) has several components:

1) 45 day period for assessing of background surface reflectance is too long.

ANSWER: As stated in the answers to Referee 1, we tried to shorten the 45-day period for the estimation of ρ_{SFC}. This resulted in some improvement on locations with low cloud coverage. In regions or during periods with high cloud coverage a shorter time period reduces the chance to get cloud-free observations with low aerosol concentrations and, therefore, reduces the quality of the resulting product. Hence, the 45-day period is currently the best solution in our opinion.

2) SMAC radiative transfer scheme mentioned in the manuscript is not a radiative transfer scheme per se, it is very crude parameterization of 5S (6S) output.

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ANSWER: In fact, SMAC is a semi-empirical method for the atmospheric correction based on 6S with the main advantage that it is very fast (approx. 3600 times faster than 6S). This is an important factor for the processing of large data amounts (data archive). Therefore, we evaluated the accuracy of SMAC compared to 6S, which is shown in Figure **??** *(herein). As can be seen from the scatter plot and the statistics, the overall agreement is very well, except for a few outliers.*

3) The work would benefit if more discussion was included about properties of aerosol phase function. Authors discussed in details single scattering albedo, which is good. However, the main effect of aerosol (in a single scattering approximation) is defined by a product of AOT x SSA x PHASE_FUNCTION.

ANSWER: We have added this in the revised manuscript. Therefore, we changed the structure of section 3.1 and discuss both, SSA and scattering phase function.

Consistency of AVHRR calibration in solar bands is still a problem, which may certainly introduce biases in historical retrievals, especially without tuning to AERONET. This, however, is a responsibility of authors, but should be taken into account in historical data analysis.

ANSWER: We currently investigate the effect of calibration on the archive data. The results will be published in a future paper, but we added some comments on this topic in the conclusions.

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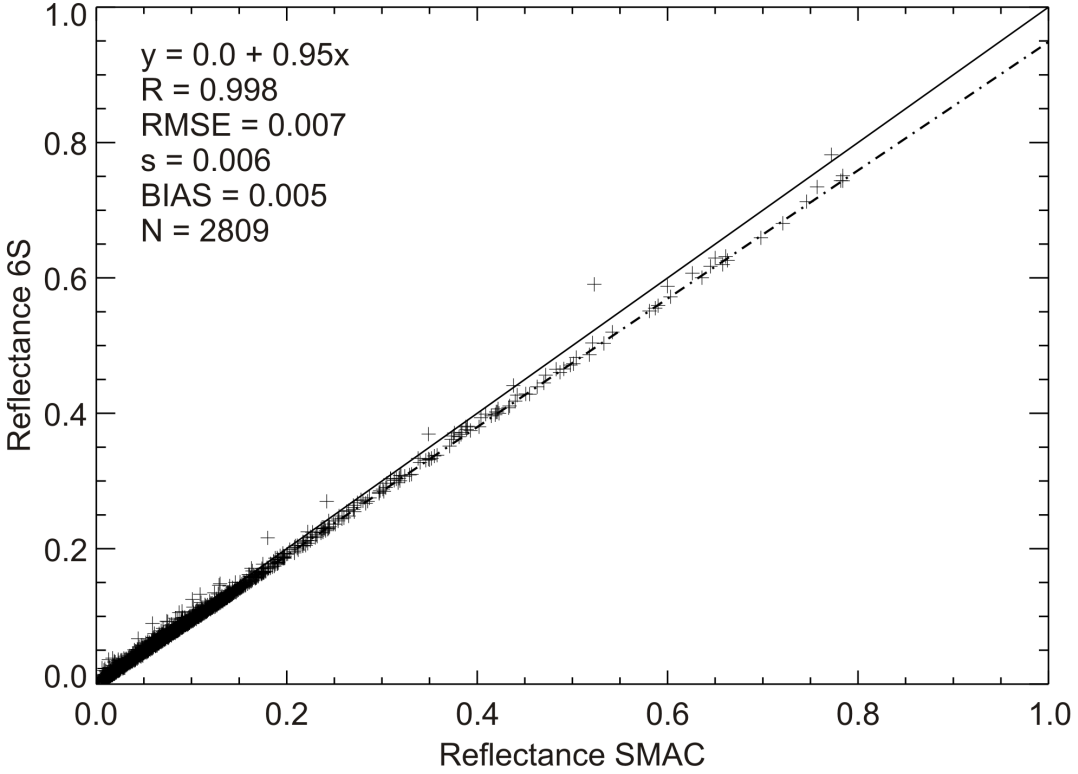

Fig. 1. Scatter plot for the comparison between the semi-empirical radiative scheme SMAC (based on 6S) and 6S.

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