Atmos. Meas. Tech. Discuss., 3, C2735–C2745, 2011
www.atmos-meas-tech-discuss.net/3/C2735/2011/

Atmospheric Measurement Techniques Discussions

Interactive comment on "Retrieval of aerosol mass load (PM_{10}) from MERIS/Envisat top of atmosphere spectral reflectance measurements" *by* G. J. Rohen et al.

G. J. Rohen et al.

guenter.rohen@awi.de

Received and published: 16 February 2011

Reply to comments to Anonymous Referee #1 as Received and published: 21 January 2011

General comments

The subject of satellite-based particulate matter mass load estimation is highly relevant in both scientific and environmental regard. The results presented in the paper are promising. However, the discussion of assumptions, limitations, and potential applications is not appropriate as well as the bibliography (which has no paper from other groups after 2007).

We agree to the comments of the reviewer who both basically had the same critics about 1) missing discussions and the limitations of the retrieval, 2) about the structuring of the paper, and 3) missing references about the actual progress in PM retrievals.

1) We list and discuss the limitations and assumptions and the characteristics of the retrieval. We revised many text passages to this purpose and added a section dedicated to the limitations and characteristics of the retrieval, in particular in the view of other PM retrievals. We also revised our conclusions about potential of the retrieval for air quality surveillances. We listed the main sources of impreciseness of the methodology as well as of the comparison. Those are introduced by the logarithmic relation between Angstroem alpha and the effective radius. Also, this relationship as well as the following derivation of the mass depends on an assumption of a log-normal size distribution function. However, also a critical discussion of the comparisons has been added. Due to the very different measurement devices comparisons itself are error-prone. In view of this discussion, the yielded standard deviation between 25 and 35 percent corresponds to the limitations of this retrieval and is about the state of the art of other PM retrievals (mainly 2.5, PM10 is very difficult to retrieve with the commonly used methodology using a functional relationship between AOD and PM. However, we showed that the retrieval with the corrections works within reasonable errors. In the future, the aerosol types must be classified in more detail in the future research in order to assess the alpha-eff. radius relation and a proper size distribution model. 2) We have made major revision of the paper: we have shortened and revised the abstract, introduction, the section about AOD retrieval and conclusion and introduced a new section about the limitations of the retrieval in detail. We also splitted the section of the PM retrieval into one dedicated for the methodology, as well as several for the corrections of humidity, temperature and the filter discussion. 3) We also added several actual references, in particular which discuss the results as yielded by different retrievals (see details in the response to the specific comments). In particular, we noted

a publication of the methodology in a retrieval from a Minsk institute from our co-author Kokhanovsky which was a big surprise for me. They used the same method, but a different AOD retrieval, but of exactly same day and region we used for our studies. They didn't inplement any corrections of the retrieval and also made the comparisons without a folding function. They claim to publish a new retrieval which is definitely wrong. We showed first results of our retrieval on a Proceeding Conference in 2007 (von Hoyningen-Huene, 2007). However, we think that the yielded results (by the way without any concluding summarizing quantitative measure like correlation or standard deviation) are accidently and that the retrieval approach is unsufficient. They have made no discussions about errors or limitations. 4) We also improved some figures, in particular added standard deviations and biases and revised the captions.

PLEASE NOTE THAT WE HAVE MADE MAJOR REVISONS OF THE PAPER WITH SUBSTANTIAL CHANGES OF THE INTERPRETATIONS AND CONCLUSIONS AS WELL AS FIGURES. A REVISED MANUSCRIPT CAN BE PROVIDED ON REQUEST.

Specific comments

1. Does the paper address relevant scientific questions within the scope of AMT? Yes, it definitely does (see general comments).

2. Does the paper present novel concepts, ideas, tools, or data? The approach presented is new and it is demonstrated and evaluated with a dataset covering Germany.

3. Are substantial conclusions reached? The results shown are promising, but the discussion of the approach and results is not thorough enough – section 4 and 5 discussion / conclusion are clearly too short.

See general comment above. We have made major revisions of all text passages regarding a critical discussion. We revised all test passages in particular the conclusions under these aspects.

4. Are the scientific methods and assumptions valid and clearly outlined? The method-

ology is described and its elements are motivated or supported by underlying physical principles, but the impact of the assumptions made is hardly discussed (only referring to further analysis which needs to be done).

We now introduced critical discussions about the limitations and assumptions as well as potential applications. For this purpose, we also added a dedicated section. In particular, we tried to estimate the main sources of impreciseness, in view of the methodology as well as in view of the comparison with the ground based measurements. This brought new changes of our assessment about the potential of pm retrievals as application for air quality surveillance, in particular for the assessment of the results above Hamburg and for the potential application of the retrieval in general. Therefore we had to revise large parts of the text and conclusion.

5. Are the results sufficient to support the interpretations and conclusions? The results demonstrate the potential of the approach with a correlation coefficient to ground stations of 0.75 over the area of one country (Germany) the result fits well into the current state of the art. In the introduction statements regarding the application perspective are made, which are not supported by the rest of the paper. The statement "or even a replacement of the cost-effective ground . . . measurement . . ." must be deleted. It is far out of reach and even politically debatable.

Agreed, we deleted these sentences, changed our opinion about the potential applications. We made this clear in the text and wrote about limitations and potential applications.

6. Is the description of experiments and calculations sufficiently complete and Precise to allow their reproduction by fellow scientists (traceability of results)? The paper provides the details of the methodology (maybe even in some parts too much detail).

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? There are no references to papers other than the author's group after 2007 - this needs to be updated. For example the review paper by R.

Hoff and S. Sundar "Remote Sensing of Particulate Pollution from Space: Have We Reached the Promised Land?" in the Journal of Air & Waste Manage. Assoc. 59:645–675, DOI:10.3155/1047-3289.59.6.645, 2009 must be quoted - therein also further quotations can be found. Regarding AOD retrieval a more recent overview is provided in A. Kokhanovsky and G. de Leeuw, "Aerosol Remote Sensing over Land" Springer 2009

We updated all references and literature adequately in the entire text. In particular, we added Hoff et al. and several other references for the discussion of the limitations and applications of the retrieval; in particular we added van Donkelaar 2010, Kokhanovsky 2009, Koelemeijer 2006, Glantz 2009, Hoff 2009.

8. Does the title clearly reflect the contents of the paper? Yes, it does.

9. Does the abstract provide a concise and complete summary? Overall, the abstract provides a good summary of the paper; however with its application focused over land, the mentioning of 13 wavelength channels used (only over ocean, where there are only 7 exploited over land) is miss-leading. Also the statement "exclusively based on ..." in C2503 l. 9 is somewhat miss-leading in the light of the later descriptions.

Only seven wavelengths have been used for the retrieval and for the derivation of the Angstroem alpha. AOD from other channels are extrapolated using the Angstroem of these seven short wavelengths (see p.5433 l. 158). This is of course an assumption that can only be made if the size distribution is still valid for longer wavelengths. However, we revised all text phrases in order to make this fact clear. Additionally we deleted

10. Is the overall presentation well-structured and clear? The structure needs improvement, so that the reader does not loose the “red thread”. For example, only one sub chapter 3.1 almost the same length as the section 3. before does not make sense. The overview of the paper structure at the end of the introduction (p. 5433 / l. 18-28) does not match the paper structure and needs to be rewritten according to the paper

sections.

We inserted several sections to make the structure clear and revised the structure of the text in several passages. In particular, we omitted all text passages which are not really relevant for the main conclusion of the presented work.

11. Is the language fluent and precise? Language in general is ok. However, there are several (too many) un-scientific expressions such as “is hoped to be provided” (p.5431, l. 18), “raises the hope” (p. 5431, l. 27), “so called ocean color bands” (p. 5434, l. 19)

We revised all text accordingly.

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes, they are. Some quantities in the equations are not introduced / defined in the text. Please let us know concrete which quantities.

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? I recommend splitting section 3 into the proper PM retrieval and the additional correction part as new section 4.

Done. Additionally, we shortened many paragraphs in order to focus on the main statements. Structure should be much better now. Section 3 is splitted now.

14. Are the number and quality of references appropriate? These are not updated after 2007 (see 7). Regarding European legislation the most recent new directive 2008/50/EC on PM2.5 must be added – the statement on p. 5431 / l. 10f referring to the future for PM2.5 compliance obligations is thus outdated. 15. Is the amount and quality of supplementary material appropriate? The figures shown are appropriate. In figure 5 equation 15 should be plotted.

Directive of 2008 is added as reference and the text has been revised.

Technical corrections

p. 5431 / l. 24: better refer to Kokhanovsky 2009 (Springer)

Added correspondingly.

p. 5432 / l. 10-14 mention two times the importance of the vertical profile/boundary layer height, but make no mention of the aerosol type as critical – this should be added instead of the repetition section 2: For all AOD retrieval topics I suggest to write about "BAER", and use "PMBAER" only in section 3.

We now distinguish between the AOD retrieval by BAER and the pm add-on PMBAER a discussion about the aerosol types and also the distribution function.

p.5434 / l. 11-17: here a single mode size distribution is assumed, which must be mentioned (also at later points referring to the Angstrom coefficient)

We now made it clear that we use this simple assumption and make it clear that this is a potential error source (see discussion section).

p. 5434 / l. 18 and p. 5435 / l. 27: conflicting angles are given

68.5 degree is correct, but we omitted this specification because its not relevant for the purpose of the paper.

p. 5434 / l. 18: a swath width should be given in km, not in angle

1150 km, done. This is relevant for the comparisons with ground devices.

p. 5434 / l. 27: I would not refer to street canyons, as the MERIS RR product with 1km is not suitable for any analysis on that scale

Correct, but FR is able to give a look into streets almost.

p. 5435 / l. 23-27: the use of another dataset stated here contradicts the statement of exclusively relying on satellite+ECMWF+model data made in the abstract – CAMELO database needs to be spelled out and referenced.

We now made the dependence of the retrieval on spectral measurements and models clearer: there have been several measurements and models used for the retrieval (eg

AMTD

3, C2735–C2745, 2011

for the relationship between alpha and effective radius). We have made this clear at all adequate text phrases. CAMELEO is now referenced.

p. 5437 upper part: the BRDF model is suited for Hamburg, maybe parts of Germany but not for global application as stated elsewhere in the paper

This assumption has to be tested for other sites in future studies, but however, I the paper we stated clearly that the BRDF parameter have been found by adjusting top Hamburg AOD and that the retrieval was just shown to work for Germany similar sites.

p. 5437 / l. 14: add numbers for the offset 0.003 to 0.054

Done

p. 5437 / l. 20f: the statement referring to Lentz 2006 is unclear and not helpful

Lentz maybe to detailed as reference, Kokhanovsky fits better now.

p. 5437 / l. 23: which OPAC component is used?

We used the water-soluble component and made this clear in the caption of the figure

p. 5439 / l. 16: where does the number 0.832 come from, how is it justified, motivated?

Kokhanovsky et al. choose the case of coefficient of variance of size distribution divided by average radius = 1. The same assumption was made to find the relation of Angstroem alpha to effective radius by Kokhanovsky et al. 2006 in Fig A5. We made a corresponding remark and give a reference.

p. 5440 / l. 9: the air mass factor depends primarily on geometry and multiple scattering

The air mass factor depends on geometry and scattering. But scattering is depending also on temperature and pressure (see the section about temperature correction). We have rewritten these sentences adequately.

p. 5440 / l. 17: why is Reff set to 1.7 micron?

AMTD

3, C2735–C2745, 2011

Interactive Comment

Basically, there is no information about the radius with a zero or negative Angstroem coefficient. The physical reason for the usage of 1.7 is the fact that we assume a steep extrapolated curve beyond Angstroem coefficient of zero and that only little masses from these particles contribute to the total mass of PM10, and in particular at the here used data over Germany, where we found average radii of about 0.2 to 0.3 mum. This is of course a large restriction for the retrieval methodology in general. But however, we made this clear in the text and showed that this restriction has no value for the data used in the presented study for Germany.

p. 5441: the explanation of the impact of different cut-off radii is confusing and seems to spoil the transferability of the method at large – this should be shortened and written more clearly

The differences due to different definitions of the physical mass particulate matter are an important aspect if one wants to compare different devices and in particular larger particles. As seen in Fig A7, for instances there are differences of about 15% at an effective (!, not even maximal) particle radius of 1 micron. Consider the standard deviation of about 30%. An adjustment of the axes is recommendable.

p. 5442 / l. 9: add UTC times for the change of BL height We added UTC time to the plot of BLH.

p. 5444: it seems to me that the Rayleigh correction has little or no effect on the results as compared to some of the assumptions made – this should be discussed, or even the text on the Rayleigh correction could be deleted

Rayleigh correction has small effect on the result, in particular for short wavelength. It is depending on the surface elevation, too. However, the effect is small but not negligible. We made a remark on this.

p. 5445 / l. 16ff: add also RMSE and bias values to the correlation Done

Sections 4 and 5 must list and discuss all assumptions and their probable impact on

AMTD
3, C2735–C2745, 2011

the results as well as the application potential of the method (other countries, . . .) including a qualitative ordering of the assumptions in terms of impact on resulting uncertainties; issues such as limited coverage (under clouds, especially in winter) need to be discussed.

A dedicated section "critical discussion" has been added which response to all the above questions. We revised the entire text and in particular abstract and conclusions in order to make the limitations of the retrieval methodology clear. We also give a list of largest sources of impreciseness. In spite of the limitations, results stay promising, at least for the presented data over Germany.

p. 5446 / l. 25 "based exclusively on satellite data" is in contradiction with the abstract (relying on satellite+model+meteorological data) and with p. 5447 / l. 5-8.

We revised the passages adequately now.

Fig. 2: add viewing angle to x-axis; explain "typical BRDF effect"

The viewing angle is adequate to the pixel of MERIS spectrometer. The distinguishing is not important for the qualitative and even quantitative classification. The typical BRDF effect is that only under a certain angle enhanced reflection directly into the spectrometer falsifies the true signal to a very large extent. We erased this statement completely.

Fig. 3 suggest that phase functions vary quite a lot – the impact is not discussed; is the one from LACE-98 also for 870 nm? No, 870 is that from AERONET, but for coarse mode, maybe not that clear in the plot. The figure shows how large differences of the phase functions can be, in particular functions for coarse mode particles deviate much from others. In our case we just found fine particle modes and this is the reason that the results fit quite well. Another reason is the different resolutions of the compared measurements. These different phase functions of course contribute to the total error budget, but AOD product has already extensively been discussed in cited papers adequately. We made a remark to this in the discussion section (impreciseness due to AOD product).

Caption to fig. 7: the surface integral is equivalent to the extinction – add this explanation We have added this to the caption of fig. A7.

Fig. 12: ocean (no retrieval done) should be black or white in all images – otherwise it seems to contain results as well

Results have been also plotted over ocean.

Interactive comment on Atmos. Meas. Tech. Discuss., 3, 5429, 2010.