**Atmospheric
Measurement
Techniques
Discussions**

Interactive
Comment

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.

Anonymous Referee #1

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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).

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. 4. Are the scientific methods and assumptions valid and clearly outlined? The methodology 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). 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. 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 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

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l. 9 is somewhat miss-leading in the light of the later descriptions. 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. 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 colour bands" (p. 5434, l. 19) 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. 13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? I recommend to split section 3 into the proper PM retrieval and the additional correction part as new section 4. 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.

Technical corrections p. 5431 / l. 24: better refer to Kokhanovsky 2009 (Springer) p. 5432 / l. 10-14 mention two times the importance of the verticalprofile/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. 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) p. 5434 / l. 18 and p. 5435 / l. 27: conflicting angles are given p. 5434 / l. 18: a swath width should be given in km, not in angle 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 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. 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 p. 5437 / l. 14: add numbers for the offset 0.003 to 0.054 p. 5437 / l. 20f: the statement referring to Lentz 2006 is unclear and not helpful p. 5437 / l. 23: which OPAC component is used? p. 5439 / l. 16: where does the number 0.832 come from, how is it justified, motivated? p. 5440 / l. 9: the air mass factor depends primarily on geometry and multiple scattering p. 5440 / l. 17: why is Reff set to 1.7 micron? 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 p. 5442 / l. 9: add UTC times for the change of BL height 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 p. 5445 / l. 16ff: add also RMSE and bias values to the correlation Sections 4 and 5 must list and discuss all assumptions and their probable impact on 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. 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. Fig. 2: add viewing angle to x-axis; explain "typical BRDF effect" 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? Caption to fig. 7: the surface integral is equivalent to the extinction – add this explanation Fig. 12: ocean (no retrieval done) should be black or white in all images – otherwise it seems to contain results as well

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

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Discussion Paper