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Atmospheric Measurement Techniques Discussions
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Interactive comment on "Aerosol type retrieval and uncertainty quantification from OMI data" *by* Anu Kauppi et al.

Anonymous Referee #1

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The paper addresses a very important topic in aerosol retrievals from satellite: the uncertainty of the selected aerosol model and the impact on the AOD. Indeed, this is one of the largest uncertainties for aerosol AOD and the proposed method is very appropriate. It builds on current methods and elegantly generalises the techniques using a sound approach. The paper is well written and well structured and I feel this paper could give an important contribution to a more accurate retrieval of AOD from space based instruments.

There are a few improvement that I deem necessary for this paper to be acceptable. In its present form it lacks a clear definite conclusion and recommendation. A very decent physical and mathematical framework is presented, however at the end the reader is left with a somewhat unsatisfactory feeling, not knowing whether the whole exercise was successful or not. For me the questions that are addressed here are: 1) does the

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AOD retrieval improve when a combination of aerosol models is allowed and combined using the Bayesian model evidence? 2) Does the model selection uncertainty give a better estimate of the AOD uncertainty than the current one?

The authors pose the questions and address them, but I see no clear answer for these questions. It's left hanging in the conclusion section. It says 'the posterior probability distribution can characterise the uncertainty more extensively than commonly given standard deviation'. Fair enough, but what does this mean? Is it better? Should we generally apply this method? Also from the provided sensitivity studies it is just not clear whether things work as expected (probably leading to the general inconclusive conclusion section).

What I lack is an answer to these questions (supported by evidence): Does the average AOD perform better than the standard one, when compared to AERONET? If not, is this reflected in a larger uncertainty? If yes, are the AERONET and OMI AOD retrievals consistent within this new the uncertainty?

If this could be adequately answered, i recommend this paper for publication.

Minor comments:

p1l24 (and a few more): data is -> data are

p3l25: referred -> referred to

p3l28: a cloudy ground pixel sounds strange. I would say a cloudy atmosphere pixel. Or just a cloudy pixel.

p3l31: What is a wise quality?

p4l4: Before the start of the new sentence, add 'For surface reflectivity,' (we used.. etc)

p4l11-13: you say: the band at 477 nm adds important info, yet you exclude it specifically. Why?

p5l3: Equation (1) is not just a 'formula'. Start this discussion with a physical description like: Assuming a Lambertian surface the contribution of the radiation at the TOA can be separated from that of the atmosphere (e,g. Chandrasekhar, 1960), viz. etc.

p5l10: of the real -> of the aerosols in the real

p5l10-11: This forward model app error,.. Which one? You haven't described an error yet. Do you mean the difference between real and approx. reflectances? Then describe that.

p5l13: This is strange: I would expect that a total (megs) error would be forward model error, noise (and perhaps more). Noise surely doesn't include forward model error? What is epsilon_obs? Noise or total? Rephrase l11.

p6. Increase the size of eq. 4 and 5, like eq. 1. They are the basis of the paper.

p7l19: cover -> covers

p7l20: cover -> covers a

p7l22-25: Move this to section 2. And add a description of MODIS, which is introduced in the next paragraph.

p8l1 & Figure 1. The OMI pixels -> The OMI pixels that were analysed The location of the OMI pixels within the MODIS swath are not clear. In Figure 1 add the contours of the OMI pixels that are used in Fig 2-6.

p8l2: The pixel has no data if -> No data are reported if the pixel is

p8l18: pixel wise -> pixel-wise

p823: in the latter day case -> On the 27th,

p9l8-10: Figure 8 is superfluous. Remove it and on describe the results from it in the text. It will reduce the number of figures, which is needed anyway.

p9l11-13;: Elaborate on this result. It is as important as the 16th.

p9l14-22: Here's the first missing conclusion. So you compared the Angstrom exponents. Whats the conclusion from all this? Does it improve as expected or not. Describe this, instead of just showing numbers in a table. The table is just there to backup the story.

p9l24 & Figure 9: This figure is inadequate. Again the location of the OMI pixels is not clear. Merge MODIS quicklookd into one RGB image and overlay the OMI pixel contours.

p10l3: the selection of the volcanic type is most probably.. : Most probably? Who is going to give a conclusive answer to that if not the authors themselves? First, indicate where the OMI pixels are in the MODIS RGB image as suggested above. Then, conclude whether or not this is due to the 'white area'. . . Do you mean cloud?

p10l10: perhaps indicating..: Again, why perhaps? Tell the reader whether there was dust or not. If not, why select this day? Surely a dust event can be easily found using OMI UVAI on a clear day. Indeed, 26 March 2008 shows low UVAI over the northern Sahara, so change this day and choose a day where you know what's going on and what aerosol model you should expect.

p10l24-26. So what's the conclusion here? Is the posterior uncertainty better or the same in the case of one chosen model? Does the (new) high uncertainty include the difference between the two measurements, or is it too small?

The conclusion section should be extended with a clear recommendation.

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