

Interactive comment on “Evaluation of spatio-temporal variability of Hamburg Aerosol Climatology against aerosol datasets from MODIS and CALIOP” by V. Pappas et al.

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We would like to thank the Reviewer for the useful comments that helped us to improve our manuscript. Below are given point by point answers to the comments (also provided in Italics).

General comments:

1) English should be checked by a native speaker;

The English has been checked and improved by a native speaker.

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2) More information on HAC are needed in the Paragraph 2.1;

More information has been provided in Section 2.1 (page 4, lines 9-25) on HAC. This included information on the basic process of HAC creation, as well as on the way the spectral resolution and the vertical distribution have been developed.

3) Please, provide more discussion throughout the paper on the reasons for the observed differences between HAC and MODIS/CALIOP/AERONET. Sometimes reasons are not given. See some examples below;

The relevant parts of the manuscript have been revised and likely reasons have been provided for the differences between HAC and MODIS/CALIOP/AERONET.

4) It would be very nice to compare also the temporal series of the AOD data from HAC and MODIS at selected locations (for example in the Mediterranean region, or in one or more of the 5 selected sites used in Fig. 8); Is any trend observed? Is any difference between HAC and MODIS at specific times observed? Is there any event which is worth to consider over the years? The analysis of the temporal series may add important/interesting information;

This kind of additional information would indeed be interesting, but HAC does not provide a temporal series. HAC serves as a climatology, based on measurements of several years and on modelling output. Therefore, the only temporal comparison between HAC and MODIS (or CALIOP and so on) can be in terms of seasons or months, but only as an average. The latter is provided here.

5) This is just a suggestion: Why do not dedicate a paragraph to the Mediterranean Basin, given its peculiar characteristics in terms of AOD and composition? The authors take as an example the Mediterranean when comparing HAC with CALIOP, but it would be also interesting to see how AOD behaves in the Mediterranean with respect to MODIS (AOD spatial distribution) and AERONET (spectral dependence);

This is a very useful suggestion. In fact, a paper of ours that is to be submitted soon

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for review assesses the effect of the different AOD and composition suggested by HAC and MODIS on the direct radiative effect of aerosols. Since the main advantage of HAC is its availability for the whole globe, it was thought that it would be better to perform the assessment at a global scale. However, a next study looking at certain regions, such as the Mediterranean or South America would definitely be something we would like to do. In those studies, a complete assessment of the performance of HAC against MODIS and AERONET looking at finer geographical and temporal scales would be possible, in contrast to the present study in which such a comparison between HAC and MODIS Mediterranean AOD values is already included but only on annual mean basis and as part of the global analysis.

6) Spectral variability: To my opinion the authors should describe the differences directly in terms of Angstrom coefficient rather than presenting the wavelength dependence of AOD values. Text (and Figures also) should refer to the Angstrom coefficient calculated from AOD data;

In our manuscript we preferred to perform the evaluation of spectral variability of HAC AOD values at full spectral resolution rather than on the basis of parameters like the Angström coefficient which is just a measure of spectral variability relying on some pairs of wavelengths. We acknowledge that this is a useful parameter used in many analyses, but radiative transfer or climate models, of which HAC AOD values are potential users, require multi-wavelength spectral AOD values. Moreover, evaluation of spectral variability using the Angström coefficient would require the computation of such values at multiple pairs of wavelengths in order to ensure complete spectral coverage, which is not so practical. For these reasons, we believe that in our study it is more appropriate to work with complete spectral HAC AOD values than a derived Angström coefficient.

Specific comments

- Fig. 1: Enhance the size of the figures; also it would be interesting to add three more

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figures with different scales (and not from 1 to 1) to appreciate AOD distribution: for example for the anthropogenic map the scale does not allow appreciating differences.

Such figures are now added in the Supplement (Figure S4a,b,c).

- Fig. 2: Write in the caption the period considered. This has been added (page 26, line 1).

- Pag. 5128, Line 12: The polluted area is big and includes Shanghai as well not only the area "around Beijing".

The relevant text has been corrected (page 4, lines 30-31).

- Pag. 5129, Lines 8-9: " ... and clear inter-hemispherical asymmetry of AOD with larger values in NH than in SH". It is better to comment this when describing Figure 1.

The relevant text has been corrected (page 4, lines 26-27).

- Pag. 5129, lines 9-11: Why is the spectral dependence for natural aerosols lower than for anthropogenic aerosols? There are no comments on this.

This following text has been added in page 5, lines 18-21: "...This is due to the size of the particles, with anthropogenic (fine) being weak absorbers of near-infrared radiation, as opposed to natural (coarse) whose size is closer to infrared radiation wavelengths and therefore are more efficient in absorption throughout the solar spectrum."

- Pag. 5130, lines 11-14: move this comment to the paragraph 3.2 and give more details about the reasons for the presence of missing grid cells.

As indicated, the specific comment has been moved to Section 3.2 (page 10, lines 20-24), along with explanation of the reason for missing cells, based also on Figures 2 and S3 (Supplement).

- Pag. 5132, line 25: How much is the value provided by Levy et al. (2010)?.

The relevant value (0.203) has been added in section 3.1, page 8, line 7).

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- Pag. 5132, lines 16-22: I would say that the biases in NH (-16.2%) and in SH (-17.1%) are similar within the error. So it seems that the biases are independent on aerosol load. Elucidate this point.

We agree that the biases are not necessarily dependent on aerosol load, rather than on aerosol type. This is now indicated in the text, section 3.1, page 8, lines 1-4.

- Pag. 5133, line 6-10: If land dominates the NH and oceans dominate the SH and if MODIS biases high low AOD values and biases low high AOD values, why are the differences for annual NH (-16.2%) and SH (-17.1) between HAC and MODIS similar?

The sign of the relative difference of HAC-MODIS AOD over land is both positive and negative, depending on the region with available values, with the largest part being positive. On the other hand, the relative difference over ocean is mostly negative, except for positive values in higher latitudes of both hemispheres. However, due to the fact that most land in NH is in the higher latitudes, applying latitudinal weights, the effect of oceans (negative sign) dominates the overall NH difference. This, together with the predominant fractional coverage by oceans in both hemispheres and the extended land areas having missing information (white shaded areas) in NH, finally result in about equal overall percent AOD differences in NH and SH.

- Figure 2: Add a map for the annual mean in the Figure.

We believe that an annual map does not really provide any extra information, additional to the seasonal one. However, following the Reviewer's comment, we have now added in the supplement a map for the annual mean (Fig. S5).

- Pag. 5134, end: It seems from Table 2 that the differences as a function of the seasons are for global and not for NH as stated in the text. Please, clarify this point.

The 'NH' in the text referred to 'boreal'. This has now been changed (page 9, line 27) to be clearer.

- Pag. 5135, lines 1-12: Why the authors describe the seasonal differences for the
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Amazon Basin only? Are there other regions where seasonal cycle was observed? If yes, please comment.

The case of biomass burning is chosen to show how the MODIS erroneous retrievals of biomass burning smoke can affect the comparison values. The relevant text has now been edited (page 10, lines 12-18), to make this clearer.

- Pag. 5135, line 1: Why during JJA the HAC-MODIS difference drops to -13.8%?

This is mostly due to the larger HAC AOD values over the Southern Ocean (yellowish and reddish colours, Fig. 2c). This is now explained in page 9, line 25-29.

- Pag. 5135, lines 16-17: The difference between HAC NH in spring (0.137) and HAC NH in summer (0.144) is only 5%, so it seems like there is a similar bias between HAC and MODIS during these seasons. It seems more interesting to comment why the GLOBE-HAC has a relative minimum in May. Do the authors have any explanation for this low value in May?

We agree with the first part of this Reviewer's comment. Nevertheless, even though spring and summer HAC AOD values differ by 5% this is enough to produce a slightly different annual cycle, as shown in Figure 3a. This has been indicated in the text, page 9, lines 29-32. As for the second part of the comment (May minimum), from the monthly HAC plots (not shown here), it appears that during May: a) the desert dust AOD over Saudi Arabia is reduced, relative to April and June plots, b) the dust transport from West Africa over the Atlantic Ocean is also reduced and c) there is significantly reduced AOD in the far north-west corner of Pacific Ocean, again due to reduced transport of pollutants from East Asia. This has now been added in the text, page 9, line 32 through to page 10, line 3.

- Fig. 3: Why is the SH-DIFF positive in summer? Is biomass responsible for this positive bias?

SH difference is positive in summer due to biomass burning and Southern Ocean larger

HAC values. This has now been added in the text, page 10, lines 5-6.

- Pag. 5135, line 20. Please clarify if it is "summer (JJA)" or "winter (DJF)". It cannot be "summer (DJF)".

It referred to the austral summer, which is DJF. This has now been corrected, page 10, line 5.

- Pag. 5138, line 9: Replace "Figure 8c", with "Figure 6c". This has now been corrected, in page 12, line 18.

- Figure 7 c-ii and c-iii: What's the reason for the different behaviour of natural and anthropogenic aerosols in the free troposphere above 4 km? Fine aerosols cumulative fractions in SH is lower than in NH, whereas coarse aerosols cumulative fraction in SH is higher than in NH.

We would like to note that the plots are for fine (pre-industrial fine and anthropogenic) and coarse aerosols, not for anthropogenic and natural. For the SH, there is little fine-mode AOD above 4 km. That's why cumulative fractions for fine aerosols are larger for SH than for NH above 4 km. For coarse aerosols, there is additional AOD in the S. Hemisphere above 10 km in the zones 50°S-70°S resulting in larger percentage of columnar coarse AOD above 4 km in these latitudes, while on a hemispherical mean basis there is larger percentage of columnar coarse AOD above 4km in the NH and SH. The different behaviour of fine and coarse aerosols in the free troposphere above 4 km has to do with the location of their main source areas over the globe (Figs. S4b,c in Supplement) in relation with the prevailing low or high pressure systems over or around them. This has been reported in the text, page 14, lines 16-22.

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