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Comment

## ***Interactive comment on “Analysis of linear long-term trend of aerosol optical thickness derived from SeaWiFS using BAER over Europe and South China” by J. Yoon et al.***

### **Anonymous Referee #2**

Received and published: 16 September 2011

Review of paper: acp-2011-504 “Analysis of linear long-term trend of Aerosol Optical Thickness derived from SeaWiFS using BAER over Europe and South China” by J. Yoon, W. von Hoyningen-Huene, M. Vountas, and J. P. Burrows

### General comments

In the present paper authors deal with trends of aerosol optical depth over Europe and South China as well as with aerosol climatological patterns over the same regions. Given the role of aerosols for the Earth’s climate and the discussion of current climate changes, it is obvious that the paper’s objective is challenging and highly interesting. A number of studies have appeared in the last decade dealing with inter-annual changes

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of aerosol properties, and the present study adds on them. Its novelty is the use of the specific data, Bremen Aerosol Retrieval (BAER) and SeaWiFS aerosol optical thickness (AOT), for the specific two world regions.

The paper is well organized and written, in general. The applied methodology is innovative, in matter of the nature of data used, and the utilized tools, though simple, like for example the performed linear regression fits, are adequate. The obtained results are nicely and clearly presented, and the text is concise. The conclusions drawn are in line with other published results. However, the discussion of results is relatively short, for example section 4 including the main findings of the study is only 2 pages long. It can/should become further elaborated and more extensive. Moreover, the analysis should be improved and extended in order to make the results of the paper more robust. The paper is very close to meet the standards of ACP and can be published after the authors address the following issues.

### Major Comments

1. The validation of satellite data (BAER SeaWiFS AOT) is made through comparison against surface measurements from AERONET data. Indeed, this is the appropriate way to do it. However, the number of selected AERONET stations is only 4 and becomes 3 finally after applied filter availability criteria to data. Moreover, all of 3-4 stations are located in Europe, the one of the two studied regions, none in Asia (Pearl River Delta). This is a limited number of stations, and more of them are necessary for deriving reliable conclusions on satellite data quality. Especially, when dealing with trends, the increased number of stations becomes even more necessary. If the former case, i.e. overall comparison between BAER and AERONET, a larger number of AERONET stations should be easily found. In the latter case, i.e. comparison of AOT trends, if the number is limited, the temporal interval could be decreased. Even for less than 11 years, successful comparisons between AOT trends from the two datasets will strengthen the validity of the BAER based results and conclusions.

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2. According to the results of section 3, BAER AOT changes are altered by about 1-3% when applying the inter-correction method (between BAER and AERONET). Essentially, this is a first-order estimate of the uncertainty of BAER AOT trends. In section 4 the BAER AOT trends over European and Pearl River regions are examined, solely based on BAER. Given the findings of section 3, how can the results of section 4 be affected by the specific BAER AOT uncertainty? Is this uncertainty critical for some of them? I believe that this has to be assessed and will be important for strengthening the validity of the derived conclusions on AOT trends over the studied regions.

3. The discussion of AOT trends and their attribution to specific causes/factors is not enough complete. When attempting such an attribution, more thorough analyses need to be undertaken before to draw conclusions. In order to be able to safely state what exactly is the responsible process/parameter for an identified trend, other possible contributing factors should be excluded after analyses either made by the authors or at least based on existing literature. It is well known that aerosol loads are determined by both emission and removal (wet and dry) processes. Therefore, stating that, for example, an increasing/decreasing AOT trend is due to decreasing emissions, the role of removal mechanisms like precipitation has to be proved that is of minimum importance. This has to be done in the present study as well.

4. Although the selection of the two studied regions and sub-regions is explained, it has to be further discussed and explained. Why have the authors selected those and not other regions?

5. As explained above, there exist a number of other studies dealing with trends of AOT over Europe and Asia (China). Even if the study period of these studies does not completely overlap with that of present paper in all cases, it can be relatively close to that. Therefore, it would be useful to compare the findings of this paper with those of others that exist in literature. This will maximize the value of the paper's conclusions.

Specific Comments

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1. Introduction: page 2, line 24 through page 3, line 28: apart from referring to various papers that have dealt with AOT trends, it would be useful to briefly report their conclusions regarding the trends (see also major comment 5). This has also to be done in section 3 and mainly in 4.
2. Introduction, page 3, line 2: which products do "... level 2 and level 3 ..." refer to? MODIS, MISR? Please, specify.
3. Introduction, page 3, lines 4-6, "They also ... (GMI) model simulations": rephrase better.
4. Introduction, page 3, line 8, "... anthropogenic aerosols": why only anthropogenic? Natural aerosols are also emitted over land.
5. Introduction, page 3, line 28: the authors have missed some papers dealing with AOT trends in the Mediterranean, e.g. Papadimas e al., 2008; Kischa et al., 2008; Koukouli et al. 2009; or in Asia-China, e.g. Xie and Xia, 2008; de Meij et al., 2010; Lei et al., 2010.
6. Introduction, page 3, line 30, "... the improvement of ...": it has to be specified how this improvement will be achieved.
7. Introduction, page 4, line 12: it is not enough to refer to CITYZEN with regards to the importance of the selected specific world regions for study; it has to be clearly documented here as well.
8. Introduction, page 4, lines 16-18: I assume that authors pretend that the quality of space-based retrievals is not completely adequate, so they should state this more clearly.
9. Introduction, page 4, lines 19-22, "This network ... (SSA), and more.": this part can move to next section (2).
10. Introduction, page 4, line 24: what the word "individual" refer to? Please clarify.

11. Section 2, page 5, Change title to: “Bremen Aerosol Retrieval (BAER) and AERONET Data Sets”.
12. Section 2, page 5, line 27: the results of Figure 2 are not discussed. I suggest omitting this figure.
13. Section 2, page 6, line 13: regarding AERONET, no explanation is provided of the applied criterion for the selection of AERONET stations, whose location falls within the studied regions. It has to be given here. Why only 3 stations and no more have been selected?
14. Section 3, page 6, lines 21-23, “... selected stations ... history were used for validation”: not enough. Please explain further the selection procedure of stations.
15. Section 3.1, page 7, line 13, “... aerosol properties ...”: which properties? They should be defined.
16. Section 3.1, page 7, lines 12-16, “In this study, ... research period”: this sentence is irrelevant to the content of present paragraph; what do they authors try to say?
17. Section 3.1, page 7, line 16: A bit further discussion should be provided: what about the error bars on the points or the distribution of matched data pairs with respect to the guide dot lines? Also, what are the biases? What is the overall uncertainty of BAER AOT with respect to AERONET AOT?
18. Section 3.2, page 8, line 18: replace “... 1.58/1.72 at 443/555 nm ...” by “1.58 and 1.72 at 443 and 555 nm.
19. Section 3.2, page 8, lines 18-20, “One of the most ... in the statistics”: this conclusive sentence is somewhat arbitrary; it has to be supported whereas the authors have to argue on that. Is the statement made based on the literature or have the authors performed sensitivity tests to derive it?
20. Section 3.2, page 8, line 23, “misclassification of strong aerosol loading as clouds,”:

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cannot be the opposite as well?

21. Section 3.2, page 9, second paragraph: how exactly was the inter-correction made? Was it applied to both BAER and AERONET AOTs, as it appears from both BAER and AERONET AOTs being changed, and in what way? Also, the authors should perform a test to check whether or not and in what way do the statistics of linear regression fits change if the non representative monthly AOT values are excluded from the time-series of both BAER and AERONET. Probably, they should try to apply a common threshold, for example 5 days, for the necessary availability of daily AOT values per month. Finally, while the inter-correction is applied to winter AOTs, this is been made based on linear correlation equations (Fig. 3) that have been derived from year-long data. Probably, even better results can be obtained if similar equations are derived from winter AOT data only.

22. Section 4, page 10, first paragraph: dust transport has also to be mentioned, at least for Mediterranean (e.g. Hatzianastassiou et al. 2009).

23. Section 4, page 10, lines 13-16: further discussion has to be made on the determination of seasonal cycle of AOT in Pearl River. Emissions and removal mechanisms should be reported for the region in more detail.

24. Section 4, page 10, lines 19-20: in two previous figures (6 and 7) it is shown that a significant decrease of AOT (- 6-7%) exists at FORTH-Crete, which is inside the eastern Mediterranean. Can the authors comment on this? Also, what about results from other datasets like MODIS? These have to be commented at least based on existing bibliography (e.g. papadimas et al., 2008; Hatzianastassiou et al., 2009 for Mediterranean).

25. Section 4, page 10, lines 23-24, "... are attributed to ... improving air quality": it is dangerous to attribute so simply the discovered AOT trends to causes. Thorough analysis is required first in order to assess the role of other determinant factors as well, like wet removal.

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26. Section 4, page 10, lines 28-29, “On the other hand . . . increasing trend”: similar to previous comment.

27. Section 4, page 11, lines 11-12, “On the other hand . . . aerosol and clouds”: what is the meaning of this sentence? Please clarify. Also, how stagnant meteorological conditions and frequent rain are observed together in summer?

28. Section 4, page 11, last paragraph: further discussion is needed on the investigated two properties from AERONET. For example, what are their uncertainties? Also, what is the covered period by the data used? How have the plotted data been computed?

29. Section 4, page 11, line 25: replace “. . . oceanic aerosol . . .” by “. . . maritime aerosol . . .”.

30. Section 4, page 11, line 26, “The aerosol properties over Crete . . . predominant”: a brief discussion should be added on aerosols over Crete, based on existing literature (e.g. Fotiadi et al., 2005; Kalivitis et al., 2007).

31. Section 4, page 11, lines 27-28, “Especially interesting is . . . fine mode in summer”: it is useful to provide a Table with computed fine and coarse mode averaged effective radius of aerosols for each station and each season.

32. Section 4, page 11, line 31 through page 12, line 2, “Therefore, the aerosol optical characteristics . . . over the pearl River Delta”: this conclusive sentence is not necessarily based on the previous statements. Increased anthropogenic emissions have to be documented in support of it.

Figure 5 caption: better say: As in Figure 4, but for 555 nm.

Figure 9: to make it more clear, mention in the caption that the bottom figures for each station correspond to 555 nm whereas the top figures to 443 nm.

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Interactive comment on Atmos. Chem. Phys. Discuss., 11, 20757, 2011.

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