**Atmospheric
Chemistry
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Discussions**

Interactive
Comment

Interactive comment on "Study on long-term aerosol distribution over the land of East China using MODIS data" *by* Q. He et al.

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Our manuscript acpd-11-10485-2011 ("Study on Long-term Aerosol Distribution over the Land of East China Using MODIS Data") has been revised according to the Anonymous Referee's comments. The English composition of this manuscript has been improved. The spelling has been checked throughout the manuscript. All the figures were plotted carefully as reviewer suggested. Almost all reviewers' suggestions have been incorporated into the revised paper. In the following, we will give an item by item response to the reviewers' comments.

Best wishes. Qianshan He, Chengcai Li

Anonymous Referee #1 Received and published: 17 May 2011 This paper analyzes

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Interactive Discussion

Discussion Paper

aerosol distributions in East China using MODIS level 2 aerosol products for years between 2000 and 2007. They classify aerosols into six different types using AOD and FMF values. They analyze three regions within East China and provide their analyses of seasonal and inter-annual variabilities. The idea of trying to understand the sources of aerosols in China is important and interesting, but there are significant problems in their data analysis and some of the logics are simply difficult to follow. In addition, a lack of clarity as well as presentation and English errors call for a significant improvement for this to be published in ACP. I provide some major issues I see in this paper and then comment on some minor details. First, their explanation of methodology is not very clear, and it is difficult to understand what has already been done and what is new in their paper. I believe more explanation is essential in section 2. For example, on p. 10494, they explain their methodology as following previous studies, but I believe this should be explained in the methodology section, rather than inserting it in results. R: Thanks for the reviewer's contribution to this paper. His concern will help us to greatly improve the manuscript. We understand the main concern is about the usage of our quantitative application of the aerosol type classification with AOD and FMF. This classification results are not very necessary to the long-term aerosol optical properties distribution study, so these parts are removed from the manuscripts. However, we give a detail citation on what have been done in previous studies in the section 2 to support our analysis. The English and grammar of the manuscript will be revised in the revised manuscript.

The lack of reasoning also makes it difficult for readers to follow their logic. For example, they state on p. 10490 l. 19-21: "Though MODIS derived FMFs have not been validated over large area on land, they provide a good indication of the type of aerosol (fine or coarse) that dominates the size distribution." I believe that differentiating fine/coarse aerosols versus the source types as they do in the paper is different, and more reasoning is needed why their methodology can be utilized. The assumption that authors make that the aerosol properties in China being similar to those over North India also needs further support. Also, if this is indeed the case, which of the three

Chinese regions is similar to North India? If their findings are that these regions show different source types, does their assumption hold? Later in the paper, they also mention that "[t]he industry is flourishing in Brazil, similar to East China (p. 10494, l. 11)" and they categorize biomass burning type and urban/industrial based on Brazil. However, is it valid to assume that there is more industry in Brazil than North America as they mention in the paper? I do not follow why Brazil is more similar to East China, and I think it is too rough to state that Brazil and North India have similar aerosol properties as East China, when the authors themselves argue that there is a variation among the three regions within East China. I thus believe their categorization needs reconsideration. R: Thanks for the reviewer's comments. We added more statements to support the validation of FMF in the corresponding paragraph, such as Kleidman et al., 2005; Ramachandran, 2007, and quoted the research results by Xia et al., (2006) to explain the applicability of the threshold values for biomass burning type and urban/industrial type in East China.

Second, although they mention the problem of bias using the MODIS data, they do not analyze the uncertainties or biases in their methodology, which leaves readers into questioning the validity of their analysis. For example, on p. 10493 l. 24-26 they write: "However, caution must be note (this needs to be change to "noted") here that the alpha is even more sensitive to the assumptions on the spectral dependence of the land surface than the AOD, and may be biased for specific surface type or season", but there is no further analysis regarding how this might affect their analysis. R: Thanks for the reviewer's comments. We added a paragraph to analyze the uncertainties or biases of Angstrom exponent.

Third, their data analysis needs improvement. On p. 10498, l. 11-12, they argue that the "results clearly indicate that AOD is considerably higher during spring and summer than in winter". However, when I look at the values including the standard bias, I do not find a significantly different values between the two, as in region I, spring AOD value appears to be 1.01+-0.61 whereas for winter, it is 0.49+-0.31. I would appreciate

more explanation on how we should understand this standard bias. R: Thanks for the reviewer's comments. The sentence "results clearly indicate that AOD is considerably higher during spring and summer than in winter" is changed to "results clearly indicate that seasonal mean AOD is considerably higher during spring and summer than in winter", and a sentence "More detailed explanation about the variation of aerosol loading in different season can be found in section 3.4." is added in this paragraph.

The correlation does not mean causation. The authors state that "strong southeasterly winds result in heavy aerosol loading (p. 10502, l. 7-8)", but to me the Fig. 8 only shows correlation. Also, is Fig. 8 the mean of all months and years for each zone? How are we to understand the seasonal variations? R: Thanks for the reviewer's comments. The sentence "strong southeasterly winds result in heavy aerosol loading" is changed to "Strong southeasterly winds(âĹij6ms−1) related to heavy aerosol loading". In Figure 8, we tend to analyze the dependence of all samples of wind direction and speed on AOD, the seasonal variations of the wind-AOD dependence might be repetitive to the previous section 3.4.

Figures need some improvement. The curves and lines in Fig. 9 do not appear to explain the variations much. What are the R^2? For Fig. 10, please explain what these dots are. (Why are there 12 dots for spring and 14 for summer? Are they some kind of mean values? If so, please mention how they were calculated.) Also, how were the thresholds chosen for wind speed and direction for Fig. 10? Was it based on Fig. 8? Why was it not good enough to analyze the values as they were per season? The threshold values appear very ad-hoc, so please provide reasoning behind them. Also, I would like to see the R^2 values here too. R: Thanks for the reviewer's suggestion. This paragraph has been rewritten according to the reviewer's comment.

The authors mention that Liu et al. (2003) found that the AOD maximum appearing in spring using the derived MODIS AOD between 2001 and 2002. They speculate that this might be because the Asian dust is weakening, but I believe it is possible to test this using their data. Could they show the change from 2000 to 2007 in their spring AOD

values? Do they find spring maximum in 2001 and 2002 that show a decrease in later years? I believe such an analysis is necessary for them to state the weakening of Asian dust. R: Thanks for the reviewer's comments. A figure of a time series of seasonally mean AOD over YRD from 2000 to 2007 with corresponding analysis is added in this paragraph to support the conclusion about the weakening of Asian dust.

Minor comments: 1) Overall, please explain the abbreviations when they are first introduced in the paper R: All the abbreviations are explained in the place of first introduction.

2) P. 10487 l. 7 – does this mean human health or human welfare? R: This sentence has been rewritten as "...human health...".

3) P. 10487 l. 7 – "affect the visibility" – take out "the" R: This sentence has been rewritten according to the reviewer's comment.

4) P. 10487 l. 10 – aerosols temporal –> aerosols' temporal R: This sentence has been rewritten according to the reviewer's comment.

5) P. 10488 l. 10 – "were suggested to relate to" -> were suggested to be related to R: This sentence has been rewritten according to the reviewer's comment.

6) P. 10488 l. 23-25 – Please make a citation to support this statement. R: A citation (Lee et al., 2006) is added to the statement.

7) P. 10488 l. 27 "using the observation data"-> take out "the" R: This sentence has been rewritten according to the reviewer's comment.

8) P. 10489 l. 1-6 This part is really difficult to understand, and it appears tautological to me. Also, it should be "the mean value" rather than "meaning value" R: This sentence has been rewritten according to the reviewer's comment.

9) P. 10490 l. 1 put (e.g., vegetation and soil moisture) right after "surface type" rather than where it is right now. R: This paragraph is shortened because the detailed pre-

sentation for the algorithm is not necessary for this paper.

10) Please support the sentence of the source and the types of aerosols on p. 10490 lines 16-19 by citing appropriate paper(s). R: A citation (Ramachandran, 2007) is added to the statement.

11) P. 10493 l. 14-15: Please show how anti-correlated the angstrom exponent is with AOD by indicating the r value. R: Anti-correlated coefficient of the angstrom exponent with AOD has been calculated and indicated in this statement.

12) P. 10501 l. 2-5: It seems like there are contradicting arguments. On l. 2-3, they mention that the "contribution from urban/industrial aerosols increased from June to December", but then on l. 5, they write "However, high AOD values were measured over eastern China in summer". Please clarify. R: This paragraph has been rewritten to avoid the contradicting arguments.

13) P. 10503 l. 17 Plenty of urban/industrial aerosols ARE suspended over zones I and II. R: This sentence has been rewritten according to the reviewer's comment.

14) Figure 1 and 6 might be able to be combined, and if cordilleras appear as they do in the paper, they might be shown on the figure too. R: The figure has been re-plotted and a more detailed figure with cordilleras and rivers is shown in this paper.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 10485, 2011.