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Atmospheric Chemistry and Physics Discussions

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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Dear Reviewer,

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 reviewer's suggestions have been incorporated into the revised paper. In the following, we will give an item by item response to the reviewer's comments.

Best wishes. Qianshan He, Chengcai Li

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Interactive Discussion

Discussion Paper

Anonymous Referee #2 Received and published: 23 May 2011 This paper analyzes the aerosol seasonal and interannual variability, distributions, and types in East China from 2000 to 2007 using the MODIS data of AOD, Angstrom exponents, and fine mode fraction. The topic is interesting, but unfortunately the analysis is very problematic. I will not recommend publication on ACP in the current form. Major issues: 1. MODIS FMF: The authors recognized that the MODIS FMF products have not been validated over large areas over land although they do provide good indicator of domination of fine/coarse mode aerosols. However, good indicator does not mean that you can use it quantitatively, especially when FMF is between 0.3 and 0.7. Therefore, it is a serious problem to use FMF as a quantitative measure of determining aerosol types. Using Angstrom exponent for particle size would be more reliable, but it again needs to be validated with AERONET data. R: Thanks for the reviewer's comments. In the revised manuscript 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. The research result about validation of Angstrom exponent has also been introduced in the paper. On the other hand, quantitative application of the classification is removed in the new manuscript to shorten the paper.

2. Aerosol type identification: The identification of 6 aerosol types is very problematic, even if the FMF were perfect. For example, the criteria for dust (AOD>0.3, FMF<0.7) and urban/industrial (AOD>0.3, FMF=0.35-0.7) totally overlap with each other and there is no way to separate them using the criteria. For dust and sea salt, the upper limit of FMF (0.8 and 0.7) is much too high, because these two types are dominated by coarse mode particles. The bottom line is that this method cannot distinguish aerosol types, not only because the problem with quantitative values of FMF but also the criteria used to identify the aerosol types. R: According to the reviewer's comment, we have moved the quantitative classification parts. Our analysis on the aerosol optical properties are all based on the primary remote sensing products instead of a further

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quantitative classification. The classification parts should be carefully studied in an absolute manuscript to make this paper focusing on the long-term change of the aerosol optical properties from the remote sensing views over this region.

3. Inconsistencies: There are several obvious inconsistencies in the analysis. Some examples: (1) Aerosol size and type: Figure 2 shows that over Hebei province (about 37N, 113-114E) the small particle dominates with Angstrom exponent well above 1.2, but Figure 3 shows that the same area is dominated by marine aerosol, which should be mostly in the coarse mode. How can that be? Besides, it does not make much sense that marine aerosol could be the major aerosol type over Hebei that is far away from the ocean. R: Thanks for the reviewer's comments.The figure and text about aerosol type identification have been re-plotted and rewritten according to the reviewer's comment.

(2) Aerosol loading and weather: on one hand the author indicated that the summer months are characterized by large amount of precipitation, which should contribute to efficient aerosol removal; on the other hand the seasonal AOD clearly shows the maximum AOD in the summer. Why? R: Thanks for the reviewer's comments. As presented in section 3.4 and 3.5.1, although precipitation reduces aerosol loading, increased photo chemical reaction rate combined with anthropogenic emissions are also important source terms in summer. Also, hygroscopic growth of fine hydrophilic aerosols due to the enhanced relative humidity in the lower troposphere and biomass burning may contribute to the maximum AOD in summer.

(3) The changes of AOD over Hubei (about 30N, 113-114E) and northern part of the domain (Shandong, Hebei) are at the same or larger magnitude as those over YRD, but the authors did not explain anything about those changes. R: Thanks for the reviewer's comments. The figure and text about aerosol type identification have been re-plotted and rewritten according to the reviewer's comment.

(4) Aerosol sources: The authors conclude that the aerosols "in the north can be attributed definitively to anthropogenic emissions and in the south to natural emissions"

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(page 10496). But this conclusion is inconsistent with the aerosol types shown in Figure 3 – in the north part most area is covered with "dust" aerosols (Figure 3), not anthropogenic, and in the south "continental" and "mixed", not "natural". It is puzzling why "more cordilleras, more extensive vegetation cover and less human activities" in the south would result in "steady aerosol loading increase" in the south (page 10496)? R: Thanks for the reviewer's comments. The figure and text about aerosol type identification have been re-plotted and rewritten according to the reviewer's comment.

(5) Aerosol types in zone I, II, and III: The text describes that "zone I is more frequently influenced by Asia dust", but Figure 7 shows a less frequent dust presence than in zone II; the text says that "Zone II is the typical urban/industrial region", but Figure 7 reveals a typical dust region; the text says that "Zone III represents the natural background with dominant continental and marine aerosols", but Figure 7 shows the dominance of "mixed" aerosol! How can you reconcile these contradictions? R: Thanks for the reviewer's comments. This paragraph has been removed.

More examples of inconsistency are given below in "other comments" part. 4. AOD maximum shift from spring to summer: The authors speculate the reason of the differences between Liu et al. 2003 and this work regarding the season of maximum AOD is due to the dust intensity decrease from 2004 to 2007. I am surprised that the authors did not even do some straightforward analysis of the data to support this conclusion. For example, they could have looked the same period (2000 to 2002) as Liu et al did their analysis to see if indeed the AOD was max in the spring during that period. They could also have looked the Angstrom exponent change from 2000 to 2007 to see if dust contribution has decreased from 2004 to 2007. But unfortunately they did not do any of those. R: Thanks for the reviewer's comments. More analysis about AOD maximum shift from spring to summer and a figure of a time series of seasonally mean AOD over YRD from 2000 to 2007 have been added in this paragraph to support the conclusion about the weakening of Asian dust.

5. Location of the area/region/cities should be better marked in the figure. It would

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be very difficult for non-Chinese, or even for some Chinese readers to know all the locations mentioned in the analysis, such as Poyang Lake Plain, Shanghai-Nanjing Railway, Huang-Huai region, etc, even for some well-known names, such as Yangtze River Delta. If it is difficult to put the locations on the map, then an approximate latitude longitude range should be given in the text. R: The figure of location and terrain of East China has been re-plotted and a more detailed figure with cordilleras and rivers is shown in this paper.

Other comments: p. 10487, line 7: add “health” after “human”. R: This sentence has been rewritten according to the reviewer’s comment.

p. 10487, line 12: “over a long time period” – for climate change study, 8-year data record (2000-2007) is very short. R: This sentence has been rewritten according to the reviewer’s comment.

p. 10488, line 5 and 12: spell out “YRD” and “GMS” since this is the first time they appear in the main body of text. R: This sentence has been rewritten according to the reviewer’s comment.

p. 10489, line 2 and 3 - change “climatologic meaning” to “climatologic mean”; line 6 - change “prerequisite” to “necessary”; line 7 - change “satellite based sensor” to “satellite sensor; line 10-11 – change “anticyclone is a dominant system” to “dominated by anticyclone”; line 12 – spell out FMF; line 14 – change “to that of fine- and coarse mode aerosol” to “to total aerosol”. R: This sentence has been rewritten according to the reviewer’s comment.

p. 10489-10490, first paragraph of section 2: It is unnecessary to describe the MODIS retrieval because it has been presented by the MODIS retrieval team many times in the past and you are not doing anything new by yourself. You need to just have a brief description of the MODIS products that you are using in this study. R: This paragraph is rewritten according to the reviewer’s comment.

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p. 10490, MODIS FMF: As I mentioned earlier, the MODIS FMF over land can only be used qualitatively as an indicator, not quantitatively. The large uncertainties in quantitative values are not even acknowledged here! R: 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. Further more, we removed the quantitatively usage of FMF to make the manuscript focusing on the longterm analysis of aerosol optical properties based on the direct remote sensing.

p. 10490, last paragraph, which continues on p. 10491: Do you use the old product based on the constant spectral ratios or the product from the "new algorithm" described in Levy et al. 2007? Which MODIS data Collection do you use? R: We used the products (C005) from the new algorithm described in Levy et al. 2007 and the corresponding introduction has been added in this paper.

p. 10491, line 8: "diurnal" value from MODIS? Where is this diurnal measurement from? Definitely not MODIS! R: This sentence has been rewritten according to the reviewer's comment.

p. 10491, line 14-15: change to "has the largest coal storage and steel industry in China". R: This sentence has been rewritten according to the reviewer's comment.

p. 10491, line 15: The Yangtze River is invisible on all the maps. Because of it is the divider of northern and southern part of East China, it should be marked on at least one of the maps, such as figure 1. R: The figure of location and terrain of East China has been re-plotted and a more detailed figure with cordilleras and rivers is shown in this paper.

p. 10491, line 19-20: What do you mean by "industrial outputs"? GDP? R: The words "industrial outputs" has been changed to "gross industrial output value ".

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p. 10492, line 16: delete “derived”. R: This sentence has been rewritten according to the reviewer’s comment.

p. 10492-10493, first paragraph in section 3.1: All the locations should either be marked on the maps or given latitude/longitude. R: The figure of location and terrain of East China has been re-plotted and a more detailed figure with cordilleras and rivers is shown in this paper.

p. 10493, line 16-17: change “constructive” to “construction”. Why is construction only in northern China (where Angstrom exponents are smaller) but not in southern China? Does not make sense. How does “poor vegetation cover” lead to small Angstrom exponent? R: This paragraph is rewritten according to the reviewer’s comment.

p. 10493, line 24: change “note” to “taken”. R: This sentence has been rewritten according to the reviewer’s comment.

p. 10494, first paragraph: I have already indicated the problems of using FMF for aerosol types. R: 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.

p. 10494, line 17-19: This is ironic - marine aerosols over the mountains, but dust and urban aerosols along the coastal lines! This tells clearly the problem with the type classification. R: The figure and text about aerosol type identification have been re-plotted and rewritten according to the reviewer’s comment.

p. 10494, line 23-24: You should look seasons other than spring - if you still see dust dominates you know your classification is problematic! R: This paragraph has been rewritten according to the reviewer’s comment.

p. 10494, line 25-26: Figure 3 does not show any transport - what is the evidence of transport? R: This paragraph has been rewritten according to the reviewer’s comment.

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p. 10495, line 5: Change “Annual” to “Interannual”. R: This sentence has been rewritten according to the reviewer’s comment.

p. 10495, line 9-13: Awkward sentence. Rephrase. R: This sentence has been rewritten according to the reviewer’s comment.

p. 10495, line 21: Visually I don’t see 2001 being the cleanest. How do you determine the “cleanest”? Do you have the domain average AOD for each year? R: This paragraph has been rewritten according to the reviewer’s comment.

p. 10495-10496, Section 3.3.1: There is certainly large variations from year to year but the change is not just a simple “increasing trend” - why 2004 is lower than 2003? What happened in 2006 with very high AOD in the north? R: More explanation has been added in Section 3.3.

p. 10496, line 7: What do you mean by “unbalanced variation”? This is a strange phrase. R: This sentence has been rewritten according to the reviewer’s comment.

p. 10496, line 12: what is the measure of "air quality" here? This phrase is used too casually. R: This sentence has been rewritten according to the reviewer’s comment.

p. 10496, line 15-16: you are saying that the aerosol in the north can be attributed definitively to anthropogenic emissions and in the south to natural emissions, but on the other hand Figure 3 shows a lot of dust in the north! You should make your story straight and consistent. R: More explanation about dust in the north has been added in the paper.

p. 10496, line 24-25: Figure 5 does not show seasonal change of particle size. What evidence is this sentence based on? Wet removal should sufficiently remove both large and small particles, not just large particles. If wet scavenging is frequent in July and August, then why AOD is maximum in the summer? R: A series of figures about the seasonal change of particle size have been added in the paper. Also, as presented in section 3.4 and 3.5.1, although precipitation reduces aerosol loading,

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increased photo chemical reaction rate combined with anthropogenic emissions are also important source terms in summer. Also, hygroscopic growth of fine hydrophilic aerosols due to the enhanced relative humidity in the lower troposphere and biomass burning may contribute to the maximum AOD in summer.

p. 10497, line 4-5: How can you tell that there is a large fraction of transport in spring but small in summer? You analysis does not show that at all, not anything you can tell from what you have shown. R: This sentence has been rewritten according to the reviewer's comment.

p. 10497, line 15: How much is "huge amount"? R: This sentence has been rewritten according to the reviewer's comment.

p. 10497, line 21: How do you explain the max AOD in the northern part of East China where the largest seasonal contrast is, much larger than YRD? R: A series of figures about the seasonal change of particle size have been added in the paper. Also, as presented in section 3.4 and 3.5.1, although precipitation reduces aerosol loading, increased photo chemical reaction rate combined with anthropogenic emissions are also important source terms in summer. Also, hygroscopic growth of fine hydrophilic aerosols due to the enhanced relative humidity in the lower troposphere and biomass burning may contribute to the maximum AOD in summer.

p. 10497, last line: Can you tell from the change of Angstrom exponents (and FMF) to see if this is indeed shifted from 2000 to 2007 (or to 2004)? R: More analysis about AOD maximum shift from spring to summer and a figure of a time series of seasonally mean AOD over YRD from 2000 to 2007 have been added in this paragraph to support the conclusion about the weakening of Asian dust.

p. 10498, line 1: change "typical" to "selected". R: This sentence has been rewritten according to the reviewer's comment.

p. 10501, line 18: "aerosol has a strong regional imbalance" – what does that mean?

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R: This sentence has been rewritten according to the reviewer's comment.

p. 10502, line 20: What does each data point represent? Is it regional average, seasonal average, or annual average? Is the RH column average or at a particular altitude (e.g. surface)? Where is the RH value from? R: More explanation about data obtained method has been added in Section 3.5.

p. 10503, line 11-13: Again this is inconsistent with the conclusions you had before that dust dominates zone I, which should be the least sensitive to RH! R: The text about aerosol type identification has been rewritten according to the reviewer's comment.

p. 10503, line 14-15: How is the growth saturated in zone III? Why it is not saturated in zone I? What are the evidences? This is too much hand waving. R: This sentence has been rewritten according to the reviewer's comment.

p. 10503, line 22-23: What is the aerosol particle concentration relevant here? Certainly this is not a one-size-fits-all concentration! R: This sentence has been rewritten according to the reviewer's comment.

p. 10505, line 3: "Four high AOD values" – should be "high AOD values are found in four regions". What are the AOD values? R: This sentence has been rewritten according to the reviewer's comment.

p.10505, line 11-12: The dominant aerosol type in coastal area is clearly dust over north of 30N, according to Figure 3! R: This paragraph has been rewritten according to the reviewer's comment.

p. 10505, line 16-17: "expanding year by year" – but AOD in 2007 is much lower than 2006, and 2004 lower than 2003! This is not the pattern of expanding year by year. R: This sentence has been rewritten according to the reviewer's comment.

p. 10505, line 19-20: "The peak AOD: : :": This is an awkward sentence. Which one, Shanghai or whole YRD? Shanghai is not equal to whole YRD. R: This sentence has been rewritten according to the reviewer's comment.

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p. 10514, Figure 1: The location labeled “Fujian” is actually “Jiangxi”. R: The figure of location and terrain of East China has been re-plotted and a more detailed figure with cordilleras and rivers is shown in this paper.

p. 10520, Figure 7: The color is somehow different from Figure 3. Even if they were the same, it would helpful to show the color code here again. R: The figure has been removed.

Figure 4, 5, and 8: Labels are too small to read. R: The figures have been re-plotted.

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