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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aerosol physical and chemical properties retrieved from ground-based remote sensing measurements during heavy haze days in Beijing winter" *by* Z. Q. Li et al.

Anonymous Referee #3

Received and published: 17 April 2013

Summary and Recommendation: This paper reports the aerosol microphysical properties derived by a surface remote sensing radiometer (Aeronet) . It focuses on characterizing two different heavy haze events in Beijing, China during the winters of 2011 and 2012 as well as the reporting of some the same observations averaged through the same year. The data reported are the standard aerosol properties available from Aeronet group. In addition, the aerosol types (black carbon, dust,etc) present in the atmospheric column are derived by a new aerosol inversion algorithm that ingests the Aeronet derived size distribution and other parameters. The focus of the paper is in the characterization of the haze events. A number of interpretations are drawn regarding the aerosol events studied with many of these conclusions based on the results

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Interactive Discussion
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of the new algorithm. I find the subject and topic very compelling particularly for the researchers in the satellite remote sensing community. Generally speaking satellite algorithms (like MODIS and OMI) have difficulty in retrieving aerosol properties in this region and the reporting of surface remote sensing are very much welcomed information useful for the proper interpretation of the satellite products. With this regard, I view this work as a useful contribution.

However, I find this work still incomplete in many ways. The most outstanding issue is the use of the outputs of an algorithm whose performance is not known to the scientific community. The description provided in p5098 is too brief and not clear and the reference provided (Wang et al, 2013) still does not address the performance and sensitivity to errors in the retrieval. This reviewer is left with no way to evaluate if the statements regarding the aerosol properties found are really geophysical features or algorithm artifacts.

As a result I recommend this paper as accepted with major revisions or reject until the concerns raised in this review are addressed.

Wang, L., Li, Z. Q., Tian, Q. J., Ma, Y., Zhang, F. X., Zhang, Y., Li, D. H., Li, K. T., and Li, L.: Retrieval of absorbing black carbon, brown carbon and dust aerosol components from ground-based remote sensing measurements, J. Geophys. Res,accepted, 2013.

Detailed Comments:

First of all, I would like to acknowledge the Editor for facilitating a copy of the Wang et al (2013) to this reviewer.

Major General observation 1: My major concern is the use of an algorithm that has not been properly tested as it is apparent by the information provided in this paper and in Wang et ., (2013). I am concerned that many of the statements made in the Result sections are justified or explained in terms of geophysical reasons with no consideration of the intrinsic uncertainty introduced by the new algorithm proposed. Specifically,

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what is the sensitivity of the proposed code to errors in the input size distribution and indexes of refraction from Aeronet inversions? For example, in a given day, Aeronet can provide 2-3 aerosol inversions in the morning and another 2-3 in the afternoon. These distributions provide a range of daily variability which could be an artifact introduced by the Aeronet inversion or an actual variability of the size distribution during day. When you consider this variability as input in the proposed algorithm, what is the range of values obtained in the different "composition" groups? This variability defines the space of error of the new algorithm. Once these ranges are defined, the results shown in this paper can be interpreted. This is a basic analysis that should have been reported in the Wang et al., (2013) work but it wasn't there. The Wang et al., 2013 work cites the uncertainties in the Aeronet inversions (which are the original uncertainties reported in the Dubovik et al., 2000 work) but does not discuss the impacts in the proposed new algorithm. As a result, this reviewer is left with no reason to critically assess statements such as the discussion of Aerosol chemical fraction in Section 3.2. This is the main reason why I think major elements of this paper need to be reassessed. I think, for example, the authors should consider add a section to this paper addressing the uncertainty added by this new code to the uncertainty already contained in the Aeronet standard inversions. Then, after making such assessment, the authors can reinterpret the results reported in this paper.

Major General observation 2: This second comment concerns the labeling of the aerosol groups used by the authors. Specifically, it refers to the fact that all retrievals reported in this paper are labeled with terms more commonly associated with aerosol chemical terms. For example, labels such as sulfate aerosol or water content aerosols give the impression to the reader that the algorithm is actually retrieving or measuring these parameters. In fact the algorithm is just finding an aerosol model with a real and imaginary index of refraction generally associated with these group of aerosols and assess the contribution of each group. Only actual chemical measurements and comparisons with the retrievals can show that this is the case, that is more comparisons like the one reported in Figure 11. While this terminology can be understood within the

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remote sensing community, this is very confusing to non-remote sensing researchers like atmospheric chemists who might read this work. I think this can be easily corrected in the paper by making very clear that these are retrievals and while they are labeled with easy-to-remember names , it does not necessarily mean that retrievals are actual composition measurements. Another way of avoid confusion could be to label the retrieved aerosol groups just by generic labels such as Aerosol Group 1,2 ,3, etc, and put in table the correspondence of each Aerosol Group with the specific indexes of refraction and size distribution associated.

General formatting and minor comments.

Abstract :

Line 1: replace “With the development of economy” with “With the increased economical development”

Line 10-11 : improper use of composition terms . This paper is about remote sensing and should not use these labels as they were actual composition measurements. A more proper labeling would be something like “aerosol fractions identified as BC, BrC. . .. “ and so on.

Line 14: what do you mean by “stable”? not clear if mean change in time.

Line 15: not clear the meaning of “Therefore, a parameterized heavy haze characterization was drawn to present a research” . Is the paper reporting parameterization? Is the paper parameterizing something? I do not think this is a proper use of the term.

Line 16-18: Please be more quantitative with respect to the size distribution. For example, you may want to add information about the fine mode fraction.

Line 21 : remove “obviously” , it makes the sentence confusing.

Line 23 : no proper use of the word “occupied”

Introduction Page 5093, Line 1 4: vapor is invisible and it only contributes to visibility

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Interactive Comment

in the condensation phase. Please correct. Rewrite the whole sentence, the use of which is not recommended here. Page 5093, Line 15: type “blown” Page 5093, Line 24: Clarify what a “new polarized sun-sky radiometer” is. Are the new Cimel detectors measuring polarization? Is this polarization used in the retrievals? Page 5094, Line 13-14: replace “in a triplet way within about 30 s which can be used to detect clouds” with “three times within 30 s and its variability in the period is used”

Page 5094, Line 24-25: More information needs to be provided about the inherent uncertainty of the standard Aeronet retrievals. Relies on the quality of the retrievals to make inferences, the uncertainties need to be stated and not referred to a publication.

Page 5095, Line 15-19: I think the author should expand on this. OMI specially and probably MODIS have difficulty sensing aerosols at heavy loading conditions like these. One of the reasons are no sensitivity to the lower levels of the aerosol and it is assumed that a measurements like Aeronet from the ground would not have such problem. But if you think that Aeronet is having problems too like not been able to sense the whole column of the aerosol, please state it.

Page 5096, Line 21: what do you mean with “which agrees well with the calibration accuracy of the polarization measurements” ? is Aeronet measuring polarization and used in the retrieval or in the calibration only? I think you need to be more clear about this.

P5097-5098: description of new algorithm. This is confusing and too brief. While it is appropriate to reference the original paper description of the algorithm, this algorithm is very new and the brief description provided is inadequate for the vast majority of readers who are not familiar with it and not necessarily will go to the original paper to learn about it. I suggest expanding this description to the point of dedicating a whole section to it (or an appendix). P 5099 , 17: “the absolute value” of what? Please clarify

P 5100, 1-5 : Can you speculate/suggest why the differences observed?

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P5100: overall this page has too many grammar errors and poor choice of words. Here are some corrections, but by no means theyare not all. Please have the text read by a native or very knowledgeable person in English. Line 1 : correct with “exponent IN 2012 than IN 2011 in Fig. 1” Line 8 and 12 , replace “reflects” with “modulates” Line 13: replace “property” with “coefficient” Line 14: replace “reveal” with “indicate”

P5100, line 22-23: Why do you state that water is responsible for the lower refractive index? Do you have any proof? Otherwise, it is just one of the possibilities.

P5101, line 17: Any growth factor large 1, indicates that a particle will grow in the presence of water. So this sentence is not clear because it reads like if only particles with GF>1.1 will grow, which is not the case. Please clarify/modify the sentence.

P5102,20 to p5103,6: I do not see much value in comparing with the models used by 6S. The 6S aerosol models are old (1990’s) and do not reflect much of the knowledge gained on aerosol optical properties since then. So, I suggest to remove these references and/or offer an alternative comparison with other algorithms/models.

P5104, line 5: replace with “2) there is a good”

P5104,7-10: this explanation is not clear. Are you referring to variability of aerosol in the atmospheric column? Is this the only explanation?

P5104, 10: I am not fan of these equations. They are notably imprecise and difficult to generalize. Although I do not advocate for the removal, the authors need to be very clear under what conditions this equation is valid and can be applied. Otherwise it has little value and it could be used in the wrong set of conditions.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 5091, 2013.

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