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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Merging aerosol optical depth data from multiple satellite missions to view agricultural biomass burning in Central and East China" *by* Y. Xue et al.

Anonymous Referee #1

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General comments

This paper addresses biomass burning from agricultural fires in Central and East China, by using a combination of products derived from remote sensing instruments in space. This idea is not new, but the authors also combine a novel algorithm developed by them, currently under discussion in AMTD (Xue et al., 2011). This paper lacks critical analyses and key in-depth discussions as described below, making the manuscript sketchy and unacceptable for ACP. Inasmuch as this paper mainly focuses on describing advantages of a new tool, it is not within the specific scope of ACP. I recommend it to be rejected and to be resubmitted to AMTD after thoroughly addressing

the issues discussed below.

Specific comments

1) Proposing a new integrated aerosol product must start with a discussion about the physical basis that define the background and the rationale for the new product. In the case of merging products, how to address the different assumptions and constraints considered in their development? What key factors determine the accuracy of the retrievals in the merged product? What about differences in the original algorithms about the necessary signal strength for deriving aerosol products? All these key issues need to be addressed in a consistent way.

2) MODIS and MISR have many advantages in what they can provide products to analyze aerosol spectral dependence, or Angstrom exponent, derive particle size, phase function and other optical properties. What can we learn from the new product proposed in the manuscript? It only includes AOD from MODIS and MISR, why not combining more information, that's already available, and thus gain a true synergistic approach?

3) The main source of information in the new product comes from the SRAP-MODIS algorithm described by the same authors in a paper currently under review (Xue et al., 2011). This can be inferred from the comparison of Figures 2d and 2e, where maybe $\sim$90% of the information in the final product comes from the SRAP-MODIS input. However, the paper by Xue et al. (2011) describing SRAP-MODIS has gotten rejection recommendations from both reviewers that have analyzed it. Thus the core around which the new product in this manuscript is built still lacks the ballast of acceptance by the scientific community.

4) The authors never really address in the manuscript the issue on how to compare AOD derived from their product, which represents a time snapshot of the atmosphere with large spatial variability, and the AOD from AERONET or CARSNET, which correspond to point measurements with time variability. On page 10470, lines 9-10, the

authors simply mention “The time interval between an AERONET measurement and a satellite passing overhead is less than 30 min. ” Then, on page 10472 lines 23-25 the authors say “In the comparison with AERONET AOD data (. . .) we required that the time difference between satellite overpass and the AERONET AOD measurement within half an hour. ” The authors must follow a formal procedure to convert spatial and temporal scales, like for instance the one discussed by Ichoku et al. (2002), who describe how to convert spatial averages of 50 x 50 km into 1 hour of AERONET data.

Ichoku, C., D. A. Chu, S. Mattoo, Y. J. Kaufman, L. A. Remer, D. Tanré, I. Slutsker, and B. N. Holben (2002), A spatio-temporal approach for global validation and analysis of MODIS aerosol products, Geophys. Res. Lett., 29(12), 8006, doi:10.1029/2001GL013206.

5) Validation issues. The authors correctly point out that validation efforts should be considered as a continuous process, but still they should have produced more results: there are several years available with plenty of MODIS and MISR data, but the authors show only a few days worth of data, which is unacceptably scarce. If the focus is only on biomass burning season, still there are several years available to analyze that season. Another issue is the lack of validation against CARSNET observations. The authors indicate they have used this network, but Figure 3 shows comparisons only with AERONET measurements. When discussing data available for the sake of validation, the authors fail on citing the seminal paper by Holben et al. (1998), not properly crediting that landmark work for AERONET measurements. On page 10470, lines 7-9 the authors report using AERONET level 1.5 data, when usually it is recommended to use level 2.0 calibrated data for validation efforts. Level 2.0 data usually takes longer to be available for analyses, but since the studied period was from 2007, most likely this level 2.0 data could be used in the manuscript, otherwise the authors should make the case for their use of level 1.5 data.

Holben, B. N., Eck, T. F., Slutsker, I., Tanré, D., Buis, J. P., Setzer, A., Vermote, E., et al. (1998). AERONETâĂŤA Federated Instrument Network and Data Archive for Aerosol

Characterization. Remote Sensing of Environment, 66(1), 1-16. doi:10.1016/S0034-4257(98)00031-5

6) Statistical issues. Throughout the manuscript there’s a lack of care about the proper statistical treatment for the data. All results are shown with 4 significant figures, even for cases where the number of data points is as scarce as 6 (Figure 3a). The largest dataset for validation has 72 data points (Figure 3d), so the statistical analyses also cannot be presented with 4 significant figures. The authors do not mention any regard to uncertainty in their estimates for the AOD retrievals using their proposed product. From a purely statistical point of view one needs to address the uncertainty in an estimated quantity in order to attribute any meaning to the results.

7) Methodological issues. Considering the merging of data products derived under very different conditions and assumptions, the authors mention on page 10472: “The sensors have varying designs and characteristics, and there are differences “associated with radiometric calibration, assumption of aerosol properties, cloud contamination, and correction of the surface effect” (Li et al., 2009). ”. There’s no further discussion on these complex issues, or how they should be tackled when merging the data. The manuscript mentions the merging of data from SRAP-MODIS, Deep Blue MODIS, Dark Target MODIS, and MISR data. However, the method section starting on page 10471 describes only SRAP-MODIS and Dark Target MODIS, nothing else. The model description on page 10471 is confusing and lacks a better structured discussion.

8) Conceptual issues. Section 4.2 on pages 10473-10474 seeks to compare satellite retrievals with ground-based observations. There is no discussion about whether in-situ measurements on the ground can reflect or how they can be related to the satellite retrievals. The authors simply put these results together without a word of caution or any mitigating circumstances about when a comparison is properly in order. The use of the cloud mask product MOD/MYD35 is not enough accurate for aerosol retrievals, that’s why the MODIS team has developed their own cloud mask which is embedded in the MOD/MYD04 aerosol product. Seeking to infer cloud information only from

MOD/MYD35 datasets as discussed by the authors will likely lead to misidentification and to completely missing out small clouds under the 1-km resolution for that product, and again no discussion on these flaws is offered by the authors. Figures 5 and 6 show time series of AOD and Angstrom exponent derived from AERONET and CARSNET. The results in Figure 5 and 6 are interpreted without linking them to the retrieved values from the product discussed in the manuscript. Also, the authors attribute AOD spikes seen in Figure 5 to "thin cirrus clouds" (page 10473 lines 26-28, page 10474 line 1), but the data should had been cloud-screened, so the AOD series should only contain optical depth information attributed to aerosols, not clouds. This is also another reason why level 2.0 AERONET data should be used (cf. issue 4 above). Moreover the data is inconsistent: if there are cirrus clouds why the Angstrom exponent remains unchanged before, during, and after the spikes in the AOD series in Figure 5?

9) Minor issues. Tables 1 and 2 can easily be combined in just one table. Table 3 shows a lot of information about the spectral channels in MODIS and MISR that are just not used on the manuscript. Figure 1: why is it necessary to have 2 maps shown? The map on the left shows the names of provinces in China that are just referred to once in the paper. The colors chosen to represent the maps are confusing: mainland China is represented in white, and so is the ocean. The 2 maps in Figure 1 are also confusing because the names of AERONET and CARSNET sites cannot be easily read from the Figure. To improve the readability the authors should change the names to letters or numbers, and assign them in the Figure caption. This should also be made to Figure 4. Figures 2 and 3 legends show items a,b,c, etc., but these identification letters are not shown in the graphs.

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