Comments on “Ground-based remote sensing of aerosol optical properties and their radiative impacts in the PRD region of China” by B. Mai et al.

General comments:

This manuscript presents a study on aerosol optical properties and their impacts on radiation forcing in the PRD region, China. In this study, the authors derived typical values for several key aerosol optical property parameters such as AOD, SSA, ω, and AE from 7 years of ground-based sunphotometer measurements in the PRD region of China during dry season. They found that the PRD region is dominated by strongly absorbing fine particulates, and provided an estimate of aerosol radiation forcing effects by utilizing a radiation transfer model, SBDART model. The topic represents a great practice interest and the study provides useful information for better understanding of aerosol radiative effects in the region where people are interested in but the knowledge is limited. However, an in-depth analysis is lacking and English writing needs large improvements. The manuscript is recommended for publication by JGR with major revision.

Specific comments

1. What are the specific scientific questions in this study? It seems that the study is limited to observational data analysis.
2. The abstract needs to be rewritten in a more precise way.
3. The study is focused on the dry season in the PRD region, China. A more appropriate title of manuscript is suggested.
4. An explanation is needed on why a negative shortwave radiation forcing can be found at the top of atmosphere since we assume there are no aerosols (or very small amount?) above the top of atmosphere. In fact, the manuscript did mention that instantaneous aerosol direct forcing at the top of atmosphere in close to zero in other study (Xia et al., 2007a; Liu et al., 2012) (see Lines 81-82). Why does this study find a negative annual mean shortwave direct radiation forcing at the top of atmosphere (-6.22+2.22 W·m-2)?
5. Is there any inconsistency for the statements between Lines 37-38 and Lines 41-42? The former says that “the annual mean short wave direct radiation forcing at surface is -33.51 ± 8.41 W·m-2” and the latter says “the annual mean reduction in surface direct radiation due to aerosol is -81.89 ± 18.42 W·m-2”. In addition, two decimal digits are not necessary here. Please check the similar issue in other places (e.g., 87.82% in Line 30, 0.535 in Line 214).
6. On Page 11, the third paragraph, is there any evidence supporting that the high AOD and AE in February is related to high water content? “May be” is not a good way for scientific explanation.
7. “ω=0.07” and “AE=0.75” are the threshold numbers that people found from US and other places. Can these criteria be applied to the cases in the PRD region of China without adjustment given varying physical, chemical and optical properties in different regions?
8. It will be helpful to provide a brief description of the SBDART model since it is utilized to calculate the aerosol radiation forcing.
9. Line 72, what are the “above” parameters? SSA and ASY?
10. What are the full names of CIMEL (Line 113) and SKYRAD (Line 123)?
11. P9/L179, please double check Equation (3). Is this “definite integral” or “indefinite integral”? What is “t”?
12. P11/L222, “prime” should be “primary”? Please make corrections for any spelling and grammar errors in other places of the manuscript.
13. P10/L200, “from 50-60%” 🡪 “from 50% to 60%”? Please make similar correction in other places.
14. P11/L229, where are “Xianghe” and “Xinglong”? More specific location information is needed.
15. The sub-title of section 3.3 is not matched with the contents.
16. Adding one more section on how aerosols affect energy partitioning will be helpful since the authors did some discussion as showed in Figure 7.
17. In the conclusion part, paragraphs 2 and 3 should be integrated together (P16, Lines 322-330).
18. As pointed out in the General Comments above, the English writing needs a significant improvement. For example, the sentence on Lines 180-182” needs to be rewritten.