**Atmospheric
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Comment

Interactive comment on "Source attribution of light-absorbing impurities in seasonal snow across northern China" *by* R. Zhang et al.

Anonymous Referee #1

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The paper, "Source attribution of light-absorbing impurities in seasonal snow across northern China" by Zhang et al. tried to discuss the absorbing impurities in 7 regions in China in terms of source contributions. Their method on the combination of the Positive Matrix Factorization (PMF) with the trajectory analysis should be useful to capture their purpose. However, the current discussions and analyses are not enough for the paper acceptance. The current form in the paper still includes much ambiguities and insufficiency of the analyses. Before the publication of this paper, the authors further need to do or consider the followings.

(Major points) 1. As the authors mentioned, this study is a compliment of Wang et al. (2013). Also probably the snow samples were taken from Huang et al. (2011). However, the descriptions at some parts in this paper are very confusing which points

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are the new outcomes from this study and were done in Huang et al. (2011), Wang et al. (2013), or others in the previous studies. The authors need to clearly clarify the new outcomes obtained from the current study and fully acknowledge (cite) the previous studies even if some of the authors worked in the previous studies. Please properly clarify the contribution from this study and mention the contribution from Wang et al. (2013) and others though out the text.

2. The authors mentioned that they used the estimated BC concentration in snow by the parameter, C (BC, max) from Wang et al. (2013) used in Doherty et al. (2010). Although the authors mentioned that this parameter included the fewest assumption, the better numbers of BC concentrations should be C (BC, est) than C (BC, max). I could not understand the reason to use this parameter as follows. At some locations in region 1 and in region 7 in this study (region 1 and some location in region 2 in Wang et al. (2013)), only C (BC, equiv) data were provided in Wang et al. (2013). However, the author mentioned they used C (BC, max). I could not understand which data the authors used in this study. Also in this study, the authors tried to separate the factors of source contributions into Biomass burning & Biofuel, Industrial/urban aerosol, and Soil dust. However, they used C (BC, max) considering that all particulate light absorption (650-700 nm) is due to BC, mentioned in Wang et al. (2013) and Doherty et al. (2010). This seems to generate inconsistency.

3. Wang et al. (2013) reported relative contributions to the total absorption optical depth by BC, OC, and Fe for surface stirred snow samples in their Fig. 11. In their study, except for their region 1, mostly BC and OC were dominant rather than Fe indicating dust. However, in this study, the authors reported that soil dust contributions were the overall main source of the LAI. The authors did not discuss in details why this opposite outcome was obtained and it generates ambiguities of this paper losing its importance. They need to fully discuss this point and what could be the true things in terms of the source contributions in the LAI data. But again note that before discussing they need to make the question #2 clear.

4. For the back ward trajectory analysis, the authors did not mention but my concern is how many trajectories for each case they calculated. If the authors only calculate one trajectory for each case, then the robustness of the analysis decreases. They started trajectory at 500 m a.g.l. (i.e., at ending points). In general, this kind of altitude closer to the surface always affected by the surface friction and using just one trajectory includes much uncertainty. Hence, if they did with one trajectory, I strongly suggest the authors to calculate multi-trajectories for each case, which is also available for the HYSPLIT model. Then, the PMF analysis should be done. It can reduce uncertainties on the source attribution analysis and it can increase the robustness of their analysis. Also note that trajectory analysis provides us the information on just paths of air mass. If the aerosols deposit before their ending points, then sometimes it loses the transport information of the focused aerosols. The authors should mention this point too.

5. On the part of the trajectory analysis, the authors used the observed precipitation data in China to judge the snowfall timings. But they did not use any temperature data. Are there any possibilities of rainfall rather than snowfall? They should use air temperature data too to figure out this point if it is available. In addition, if the Chinese observations (i.e., the China Meteorological Service) are available on some websites, they should mention the URL for readers.

6. If the author would like to discuss the local dust contributions, why they did not analyze the particle size distributions using such as particle counter or simply using the filter with larger pore size. If the snow samples included higher concentration of larger particles, those can be attributed to dominant local dust contributions. It is straightforward and clearer than the trajectory analysis. Please explain this point.

(Minor points) 1. P2156L12-13: The increase of the contributions from Industrial/urban aerosol and Biomass burning & Biofuels source fraction in Northeastern China should also be mentioned. It is also an important point in this paper.

2. P2156L17: Warren and Wiscombe (1980) should be the pioneer study rather than

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Warren (1982).

3. P2157L20: "LAI in snow in China" is clearer.

4. P2158L9-10: Please explain more how to plot the emission areas based on Zhang et al. (2009).

5. P2158L28-P2159L1: Why can the filter with its pore size of 0.4 μm separate BC and others? The submicron aerosols include not only BC but also other small insoluble particles. How can they be separated? Please explain this.

6. P2159L20-22: Please explain more in details about the key correction factors.

7. P2161L4: Please put any available sources (e.g., URL) for the US EPA PMF 4.1 model so that any reader can use the same model in future studies.

8. P2163L14: "China" should be "Chinese"?

9. P2166L5-7: ACP can include more figures and I think the authors can include the Supplemental figures into the main text or Appendix which is published though it depends on the authors.

10. For Fig. 1, what kind of data was used to figure out the desert regions?

11. For Fig. 4, the word, "LAI concentration" is strange. The authors should use the clear word following Wang et al. (2011) or Doherty et al. (2010).

12. For Fig. 5, why the fire areas were different in each figure? If the time period in each figure was different, put the time periods here for the cluster trajectories and fire data.

13. Using too many "this" or related words are difficult for reading.

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