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Interactive comment on "Quantifying light absorption and its source attribution of insoluble light-absorbing particles in Tibet an Plateau glaciers from 2013–2015" *by* Xin Wang et al.

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Response to reviewer 2

We are very grateful for the reviewer's critical comments and suggestions, which have helped us improve the paper quality substantially. We have addressed all of the comments carefully as detailed below in our point-by-point responses. Our responses start with "R:".

This manuscript presents interesting and valuable measurements of light-absorbing particles (LAP) from Tibetan plateau glaciers. The LAP concentrations are surprisingly

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high, when compared to other studies from the Tibetan plateau and the Himalaya. A strength of the manuscript is the extent of data presented. The amount of work that goes into collecting samples from such remote and harsh locations as the Tibetan plateau often seems to be neglected in the larger community. Here the authors have performed measurements covering large areas that presumably have different sources of LAP, as well as different meteorology affecting the glaciers. Publishing these measurements would be of benefit to the public, and this should be possible after some structural and interpretational changes are made. Along with the suggested changes, the language needs to be reviewed carefully, in order to make sure that future readers will interpret the claims and results of the paper correctly. See below for both major and specific comments.

R: Thanks for all of the comments. We have carefully responded the following questions and concerns.

Issues:

Comment 1: In the section describing the sampled glaciers (2.1) information should be added. It currently lacks crucial information such as: any estimates on the area or volume of the glaciers? Are they 'typical' valley-type glaciers? or what are the general characteristics of the glaciers? Nearby emission sources? Are the glacier fronts heavily debris covered? (See also additional specific comments given below).

R: We have added some information in this section as follows: The Qiyi glacier (39°14' N, 97°45' E) is located in the eastern part of the TP, with an elevation of 6178 m. It is classified bucket-valley glacier according to its shape, and is classified subcontinental glacier according to the physical characteristics of glacier. The Xiaodongkemadi glacier (33°04' N, 92°04' E) is located in the central Qinghai-Tibetan Plateau. It is 2.8 km in length and the average snowline is 5560 m a.s.l. The annual mean air temperature at the equilibrium line altitude is in the range -5∼-7âĎČ. The surrounding region is mainly tundra. The Yuzhufeng glacier (35°38' N, 94°13' E) is the highest peak across the

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eastern Kunlun Mountains, with an elevation of 6178 m. The Meikuang and Qiumianleiketage glaciers are also located over the Kunlun Mountains, and these glaciers have an average altitude of 5100 m and 5500 m a.s.l, respectively. The Meikuang glacier is located in the eastern Kunlun Mountains, where is characterized by alluvial deposits and sand dunes, the glacier covers an area of 1.1 km2, is 1.8 km in length (Xiao et al., 2002). The Qiumianleiketage glacier is located in the Heyuan District of the Nagora River, the largest river in the Qaidam Basin, which originated in the Kunlun Mountains of the Qinghai-Tibet Plateau. The length of the glacier is 2.6 km, and the area is 1.73 km2. The glaciers of Hariqin and Meikuang have similar altitudes but are from different mountains. The Gurenhekou glacier is located on the south-eastern margin of the Nyenchen Tanglha Mountains, and seated about 90 km northwest of Lhasa, the capital city of Tibet (Liang et al., 1995).

Comment 2: In the same section I would like to see more details on the snow sample collection. For example: what was the elevation of sample locations; which ones were snow? ice? In the supplementary material I see what the cold and warm seasons correspond to, but this information is valuable to have in the manuscript itself also (In table 1 possibly).

R: Parts of the detail information of the snow sample collection were listed in the method section in Page 5, Line 25-Page 6, Line 12 (Also see the reply to comment 1). We have submitted this table as Table 1 in the previous manuscript in this journal, and the former editor suggested that this Table should be moved to the supporting information, because it is too long and not appropriated to show in the main manuscript. However, if all of the reviewers and the editor agreed, we can change the Table S1 to Table 1 in the final manuscript.

Comment 3: Although the measurement method for the filters has been used previously, it would be beneficial to have a few words on the principles of the method (in section 2.2), and how measurements were carried out in practice for this manuscript. After reading Doherty et al. (2010; 2014; 2016) it is evident that the instrument has

gone through modifications with time. Please provide information on how the instrument used in this manuscript is similar/different compared to Doherty et al. and if it contains the latest updates or not. Also, how did the authors take into account filter samples with a high mineral dust load? With higher load a bias can be introduced to the data (described well in Doherty et al. 2016, doi/10.1002/2015JD024375, and references therein). Further, information about the filters used should be provided since it can make a significant difference on the undercatch (look for example in Doherty et al. 2014).

R: Yes, we have added information on optical analysis as follows: An updated integrating sphere/integrating sandwich spectrophotometer (ISSW) was used to measure the mass mixing ratio of BC in snow, which is similar with the instrument developed by Grenfell et al. (2011). Compared with the ISSW spectrophotometer developed by Grenfell et al. (2011), the major difference is that we used two integrating sphere to calculate the relative attenuation instead of the integrating sandwich diffuser to reduce the diffuse radiation during the measuring process. This ISSW spectrophotometer measures the light attenuation spectrum from 400 to 700 nm. The total light attenuation spectrum is extended over the full spectral range by linear extrapolation from 400 to 300 and from 700 to 750 nm (Grenfell et al., 2011). Light attenuation is nominally only sensitive to ILAPs on the filter because of the diffuse radiation field and the sandwich structure of two integrated spheres in the ISSW (Doherty et al., 2014). Briefly, the transmitted light detected by the system for an ice sample, S(λ), are compared with the signal detected for a blank filter, S_0 (λ), and the relative attenuation (Atn) is expressed as: Atn=lnâĄą[S_0 (λ)/S(λ)] The following measured parameters included equivalent BC ("C" _"BC" ˆ"equiv"), maximum BC ("C" _"BC" ˆ"max"), estimated BC ("C" _"BC" ˆ"est"), fraction of light absorption by non-BC ILAPs ("f" _(non-BC)ˆ"est"), the non-BC absorption Ångström exponent (Ånon-BC) and the absorption Ångström exponent of all ILAPs (Åtot), were calculated by using the wavelength dependence of the measured spectral light absorption and by assuming that the MACs of the BC, OC, and Fe are 6.3, 0.3, and 0.9 m2 g-1, respectively, at 550 nm and that the absorption Ångström

exponents (Å or AAE) for BC, OC, and Fe are 1.1, 6, and 3, respectively (Doherty et al., 2010, 2014; Grenfell et al., 2011; Wang et al., 2013). These parameters are defined as follows: 1. "C" _"BC" ^"max" (ng g-1): maximum BC is the maximum possible BC mixing ratio in snow by assuming all light absorption is due to BC at the wavelengths of 650-700 nm. 2. "C" _"BC" ^"est" (ng g-1): estimated BC is the estimated snow BC mixing ratio derived by separating the spectrally resolved total light absorption. 3. "C" _"BC" ^"equiv" (ng g-1): equivalent BC is the amount of BC that would be needed to produce absorption of solar energy by all insoluble particles in snow for the wavelength-integrated from 300-750 nm. 4. Åtot: absorption Ångström exponent is calculated for all insoluble particles deposited on the filter between 450 and 600 nm. 5. Ånon-BC: non-BC absorption Ångström exponent is defined as the light absorption by non-BC components of the insoluble particles in snow between 450-600 nm. 6. "f" _(non-BC)^"est" (%): fraction of light absorption by non-BC light absorbing particles is the integrated absorption due to non-BC light absorbing particles, which is weighted by the down-welling solar flux from snow at the wavelengths of 300-750 nm. It is well known that the aerosol composition and the size distribution are key parameters that affect the absorption Ångström exponent. Doherty et al. (2010) reported that the value of the absorption Ångström exponent of OC was close to 5, which is consistent with previous studies with values ranging from 4-6 (Kirchstetter et al., 2004). Several studies indicated that the absorption Ångström exponent of mineral dust ranged from 2 to 5 (Fialho et al., 2005; Lafon et al., 2006). The variation in the absorption Ångström exponents for urban and industrial fossil fuel emissions is typically in the range of 1.0-1.5 (Millikan, 1961; Bergstrom et al., 2007), which is slightly lower than that of biomass burning, which primarily falls in the range of 1.5-2.5 (Kirchstetter et al., 2004; Bergstrom et al., 2007). Although the source attribution of the insoluble light-absorbing particles in the samples is not a dominant determinant of the value of the absorption Ångström exponent, fossil fuel burning may have a lower absorption Ångström exponent (<2) than 2-5 (Millikan, 1961; Fialho et al., 2005). In this study, we noted that the absorption Ångström exponent (Åtot) is due to a mix state of BC and non-BC impurities on our

filters, and the calculations of Åtot and Ånon-BC could be found in the study of Doherty et al. (2014). The OC mixing ratio was also determined according to Eq. (2) in Wang et al. (2013), and the Fe concentration was determined according to the inductively coupled plasma-mass spectrometry (ICP-MS) measurements.

Comment 4: In the current manuscript text the discussion on the Ångström exponent is not sufficient and I'm not entirely sure that the interpretation is correct. For the Ånon-BC fraction, more information in section 2.2 on how it was determined is needed. For the results, I do not think that the differentiation between fossil fuel and biomass burning aerosol can be determined in the way it is done here (combined with the PMF it is possible nonetheless to distinguish fossil vs. biomass burning). I assume the authors already have studied Doherty et al.'s work, but I would urge the authors review it once more (especially Doherty et al. 2014 regarding Ångström) to guide their interpretations and text (and of course reference to what is done the same way).

R: Previous studies indicated that the absorption Ångström exponent is driven primarily by the composition of the aerosol and secondarily by the aerosol size distribution. Therefore, variations in Åtot can reflect variations in the source of ILAPs in the snow. Sure, Åtot can't distinguish fossil or biomass burning, however, combining with the "C" _"BC" ^"est" and Ånon-BC, we can get the general status of the BC and non-BC contents in the snow or ice. Therefore, we note the parameter of absorption Ångström exponent is still valuable in this study. But we also reconstructed section 3.1 based on the recent study by Doherty et al. (2014). Reference: Doherty, S. J., Dang, C., Hegg, D. A., Zhang, R. D., and Warren, S. G.: Black carbon and other light-absorbing particles in snow of central North America, J. Geophys. Res.-Atmos., 119, 12807-12831, 10.1002/2014JD022350, 2014.

Comment 5: Section 3.1 would benefit from more structure. In its current form, I have a hard time following in a logical order. At the moment it starts with an introduction to the concentrations of LAP in the samples, followed by statements on the Ångström exponent, and then back to more discussion on LAP concentrations. Additionally, it

would be valuable to see the results obtained in this study compared in a larger Tibetan/Himalayan perspective with results from other nearby measurements of LAP in snow.

R: In the section 3.1, we firstly analyzed the general description of the total concentration of ILAPs and the absorption Ångström exponent in seven glaciers firstly. Then, we discussed the distribution of ILAPs concentration in each glacier region. We have also reconstructed this section and added more comparison with the other nearby measurements of ILAPs in snow.

Comment 6: Section 3.2 needs to be majorly changed. In the references provided in this manuscript the OC/BC ratio is not used in such a way that the authors here claim. From the ratio it is not (unfortunately) as straight forward to say that a ratio of XX corresponds to the aerosol particles originating from fossil or biomass burning. As an example, what about secondary formation of organics? This section therefore needs further work, or to possibly taken out from the manuscript.

R: We have taken out this section and reconstructed the result section based on the reviewer's suggestion.

Comment 7: Page 3 Lines 4-6: The second sentence of the introduction almost seems contradictory to the following sentence. I believe this contradiction could be removed with careful language editing.

R: Thanks for the reminder. We have converted the relative sentences as "Ample evidence has indicated that the deposition of insoluble light-absorbing particles (ILAPs) was one of the major factors (up to 30%) to lead the greatest decrease in length and area of negative mass balance in the TP glaciers over the past decade (Xu et al., 2006, 2009a; Yao et al., 2012; Qian et al., 2015; Li et al., 2017)." Reference: Li, S., Yao, T., Yang, W., Yu, W., and Zhu, M.: Glacier Energy and Mass Balance in the Inland Tibetan Plateau: Seasonal and Interannual Variability in Relation to Atmospheric Changes, Journal of Geophysical Research: Atmospheres, 10.1029/2017jd028120, 2018. Qian,

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Comment 8: Page 3 Lines 8-9: This is incorrect use of the Jacobi et al. 2015 reference for that statement.

R: We have deleted the reference.

Comment 9: Page 3 Lines 17-19: Do you mean climate forcing from BC deposition world-wide? Please clarify. Also believe reference should be Bond et al. 2013, not 2014.

R: We have corrected this mistake, and we believed that the climate forcing in here shows the global deposition of black carbon on the snow and sea ice in the industrial era. Therefore, the we have modified the sentence as “Bond et al. (2013) indicated that the best estimate of global climate forcing from BC deposition on snow and sea

ice . . .”

Comment 10: Page 3 Lines 29-2 (page 4): Insoluble organic carbon in previous studies, please provide the references.

R: We have modified the sentence as “Although the mass mixing ratio of insoluble organic carbon in the snow and ice has been widely investigated in previous studies(Xu et al., 2006, 2009; Flanner et al., 2009; Wang et al., 2013), there are still limited studies that measure the mass mixing ratios of both WSOC and ISOC in ice samples, especially across the TP regions (Li et al., 2016; Yan et al., 2016).” Reference: Flanner, M. G., Zender, C. S., Hess, P. G., Mahowald, N. M., Painter, T. H., Ramanathan, V., and Rasch, P. J.: Springtime warming and reduced snow cover from carbonaceous particles, Atmos. Chem. Phys., 9, 2481-2497, 2009. Li, C. L., Chen, P. F., Kang, S. C., Yan, F. P., Li, X. F., Qu, B., and Sillanpaa, M.: Carbonaceous matter deposition in the high glacial regions of the Tibetan Plateau, Atmos. Environ., 141, 203-208, 2016. Wang, X., Doherty, S. J., and Huang, J. P.: Black carbon and other light-absorbing impurities in snow across Northern China, J. Geophys. Res.-Atmos., 118, 1471-1492, 2013. Xu, B. Q., Yao, T. D., Liu, X. Q., and Wang, N. L.: Elemental and organic carbon measurements with a two-step heating-gas chromatography system in snow samples from the Tibetan Plateau, Ann. Glaciol., 43, 257-262, 2006. Xu, B. Q., Cao, J. J., Hansen, J., Yao, T. D., Joswia, D. R., Wang, N. L., Wu, G. J., Wang, M., Zhao, H. B., Yang, W., Liu, X. Q., and He, J. Q.: Black soot and the survival of Tibetan glaciers, Proc. Nat. Acad. Sci., 106, 22114-22118, 2009. Yan, F. P., Kang, S. C., Li, C. L., Zhang, Y. L., Qin, X., Li, Y., Zhang, X. P., Hu, Z. F., Chen, P. F., Li, X. F., Qu, B., and Sillanpaa, M.: Concentration, sources and light absorption characteristics of dissolved organic carbon on a medium-sized valley glacier, northern Tibetan Plateau, The Cryosphere, 10, 2611-2621, 2016.

Comment 11: Page 4 Line 3: The abbreviation ‘ILAPs’ should first be written out.

R: The sentence has been modified as “Due to the importance of the climate effects

by insoluble light-absorbing particles (ILAPs), numerous snow surveys have been conducted to investigate the light absorption of ILAPs and their potential source attribution in snow (Clarke and Noone, 1985; Doherty et al., 2010, 2014; Hegg et al., 2010; Huang et al., 2011)". Reference: Clarke, A. D., and Noone, K. J.: Soot in the Arctic Snowpack - a Cause for Perturbations in Radiative-Transfer, Atmos. Environ., 19, 2045-2053, 1985. Doherty, S. J., Dang, C., Hegg, D. A., Zhang, R. D., and Warren, S. G.: Black carbon and other light-absorbing particles in snow of central North America, J. Geophys. Res.-Atmos., 119, 12807-12831, 2014. Doherty, S. J., Warren, S. G., Grenfell, T. C., Clarke, A. D., and Brandt, R. E.: Light-absorbing impurities in Arctic snow, Atmos. Chem. Phys., 10, 11647-11680, 2010. Hegg, D. A., Warren, S. G., Grenfell, T. C., Doherty, S. J., and Clarke, A. D.: Sources of light-absorbing aerosol in arctic snow and their seasonal variation, Atmos. Chem. Phys., 10, 10923-10938, 2010. Huang, J. P., Fu, Q. A., Zhang, W., Wang, X., Zhang, R. D., Ye, H., and Warren, S. G.: Dust and Black Carbon in Seasonal Snow across Northern China, Bull. Amer. Meteor. Soc., 92, 175-181, 2011.

Comment 12: Page 4 Lines 3-6: Did all of these references actually perform source attribution of the ILAP in the snow? If not, please adjust references referred to. Lines 6-9: This sentence needs to be reworked, confusing at the moment.

R: We have carefully checked the references throughout the manuscript, and rewritten the sentence as "Due to the importance of the climate effects by insoluble light-absorbing particles (ILAPs), numerous snow surveys have been conducted to investigate the light absorption of ILAPs and their potential source attribution in snow (Xu et al., 2009a, b; Doherty et al., 2010, 2014; Hegg et al., 2010; Wang et al., 2015; Wang et al., 2017). For instance, Hegg et al. (2009) found out that the light absorption in Arctic snow by ILAPs is mainly originated from two distinct biomass burning sources, a pollution source, and a marine source based on the EPA PMF receptor model." Reference: Doherty, S. J., Warren, S. G., Grenfell, T. C., Clarke, A. D., and Brandt, R. E.: Light-absorbing impurities in Arctic snow, Atmos. Chem. Phys., 10, 11647-11680,

2010. Doherty, S. J., Dang, C., Hegg, D. A., Zhang, R. D., and Warren, S. G.: Black carbon and other light-absorbing particles in snow of central North America, J. Geophys. Res.-Atmos., 119, 12807-12831, 2014. Hegg, D. A., Warren, S. G., Grenfell, T. C., Doherty, S. J., Larson, T. V., and Clarke, A. D.: Source Attribution of Black Carbon in Arctic Snow, Environ. Sci. Technol., 43, 4016-4021, 2009. Hegg, D. A., Warren, S. G., Grenfell, T. C., Doherty, S. J., and Clarke, A. D.: Sources of light-absorbing aerosol in arctic snow and their seasonal variation, Atmos. Chem. Phys., 10, 10923-10938, 2010. Wang, M., Xu, B., Cao, J., Tie, X., Wang, H., Zhang, R., Qian, Y., Rasch, P. J., Zhao, S., Wu, G., Zhao, H., Joswiak, D. R., Li, J., and Xie, Y.: Carbonaceous aerosols recorded in a southeastern Tibetan glacier: analysis of temporal variations and model estimates of sources and radiative forcing, Atmos. Chem. Phys., 15, 1191-1204, 2015. Wang, X., Pu, W., Ren, Y., Zhang, X., Zhang, X., Shi, J., Jin, H., Dai, M., and Chen, Q.: Observations and model simulations of snow albedo reduction in seasonal snow due to insoluble light-absorbing particles during 2014 Chinese survey, Atmos. Chem. Phys., 17, 2279-2296, 2017. Xu, B. Q., Cao, J. J., Hansen, J., Yao, T. D., Joswia, D. R., Wang, N. L., Wu, G. J., Wang, M., Zhao, H. B., Yang, W., Liu, X. Q., and He, J. Q.: Black soot and the survival of Tibetan glaciers, Proc. Nat. Acad. Sci., 106, 22114-22118, 2009a. Xu, B. Q., Wang, M., Joswiak, D. R., Cao, J. J., Yao, T. D., Wu, G. J., Yang, W., and Zhao, H. B.: Deposition of anthropogenic aerosols in a southeastern Tibetan glacier, J. Geophys. Res.-Atmos., 114, 2009b.

Comment 13: Page 4 Lines 14-16: What did the results of Doherty et al. 2014 show? In the previous sentences you provide some highlight from each study, but not for Doherty et al. Could be useful for readers.

R: Sorry, we have added the results of Doherty et al. (2014), and the sentence has been rewritten as “Recently, vertical profiles of ILAPs in seasonal snow were performed from 67 North American sites, and biomass/biofuel burning, soil and fossil fuel pollution are explored as the major sources of particulate light absorption based on the chemical and optical data, and these were (Doherty et al., 2014)”.

Comment 14: Page 4 Lines 17-18: At this time there has been an increasing number of observations on ILAP in Tibetan snow. The other referee provided a comprehensive list on this.

R: We have corrected this mistake.

Comment 15: Page 4 Lines 29-30: Units for the AOD numbers? And for what time period is this? Also, it would be good to include some information on what an AOD number of XX means (e.g. 0.4 meaning high optical depth, etc.)

R: Aerosol optical depth (AOD) is a description of the attenuation of light by aerosol, and it is a dimensionless quantity. Figure 1 showed the spatial distribution of the averaged AOD retrieved from Aqua-MODIS over Tibetan Plateau from 2013 to 2015. According to the reviewer's suggestion, we have added this information to the caption in Figure 1.

Comment 16: Page 5 Lines 1-2: How is the glacier located above the ELA? Lines 2-3: The glacier must encompass an elevation range, and not only located at 5743 m a.s.l. The average snowline is based on what? Reference on this number ? Lines 4-5: Is the Yuzhufeng glacier part of the highest peak? Please clarify.

R: Page 5 Lines 1-2: The sentence has been rewritten as "The Qiyi glacier (39°14' N, 97°45' E) is located in the eastern part of TP, with an elevation of 4850 m". Lines 2-3: The average snowline is just a general background of Xiaodongkemadi glacier, and doesn't correlate with our study. Therefore, we deleted the sentence. Lines 4-5: The sentence has been rewritten as "The Yuzhufeng glacier (35°38' N, 94°13' E) is the highest peak across the eastern Kunlun Mountains, with an elevation of 6178 m".

Comment 17: Page 5 Lines 8-9: The surrounding areas characteristics, why is this information important? Is such information available for other glaciers? Line 12: What is the point of the Liang et al. 1995 reference?

R: Page 5 Lines 8-9: this information was used to describe the topography of the

Meikuang glacier. The Meikuang glacier is located in the eastern Kunlun Mountains, where is characterized by alluvial deposits and sand dunes (Xiao et al., 2002), meanwhile, ∼47.9% of the sources of ILAPs in the Meikuang glacier was from soil dust in the PMF analysis, so there was a good correspondence. For line 12: It should be a general description to illustrate Yangbajing glacier and we deleted the citation of Liang et al. (1995).

Comment 18: Page 5 Line 15: I don't see in table S1 (or anywhere else) if the sample is ice or snow. Please provide this information. Line 16: What was the volume of the tubes? And what kind of tubes were they?

R: Due to the samples were only collected approximately for each six months in the TP glaciers, we note that most of the collecting samples were ice samples, which were less than 1 m from the glacier surface (Details could be found in Table S1). Line 16: We have modified the sentence as "The collected snow/ice samples were preserved in 0.5-m pure clean plastic bag with a diameter of 20 cm, and kept frozen at the State Key Laboratory of Cryospheric Sciences, Cold and Arid Regions Environmental and Engineering Research Institute in Lanzhou".

Comment 19: Page 5 Lines 18-20: Please provide more information on how the snow/ice samples were cut. After this (and elsewhere in the manuscript) I'm missing some description on the filtering of snow samples. Lines 24-28: This sentence is confusing and needs to be reworked. I do not find the argument made in the references given, but once this sentence is reorganized it may be more clearly.

R: We collected 67 ice samples in seven glaciers on the Tibetan Plateau, and cut each sample from top to bottom at a length of approximately 10 cm. Finally, we obtained a total of 189 small samples. For the filtration process, we have added the description in section 2.2. Lines 24-28: The sentence has been rewritten as "The OC mixing ratio was also determined according to Eq. (2) in Wang et al. (2013), and the Fe concentration was determined according to the inductively coupled plasma-mass spectrometry (ICP-

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MS) measurements".

Comment 20: Page 6 Lines 19-20: What does this opening sentence have to do with the chemical analysis?

R: We have reconstruction the method section, and the sentence in Lines 19-20 has been deleted.

Comment 21: Page 6 Lines 20-23: What does the MACs have to do with the chemical analysis? This information should instead be included in the optical analysis section (2.2.). Line 24: Do you mean 10 mL of water from the filtered meltwater? Please clarify. In addition, a few more words describing the total carbon analyzer would be beneficial. Lines 29-30: Was it the filtered meltwater again that was analyzed for major metallic elements?

R: Page 6 Line 20-23: We have moved this information into section 2.2. Line 24: 10 ml refers to the amount of sample solution after filtration that used to measure the WSOC concentration, and more descriptions of the total carbon analyzer were described by Cong et al. (2015). Lines 29-30: We used the sample solution before filtration when analyzed the major metallic elements.

Comment 22: Page 7 Line 3-4: Did you acidify all samples or not? I find it confusing with the opening of the sentence 'generally speaking'. Line 5: Measurement precision range, do you mean $\pm$?

R: No, for each ice sample, we acidified all samples before filtration and then used ICP-MS to measure the trace element contents. Meanwhile, we deleted "generally speaking". Line 5: The relative deviation between most of the measured values of the elements and the standard reference values is within $\pm$10%.

Comment 23: Page 8 Line 6: What are EFc ? Please write out the abbreviation. Line 25: What does Q values stand for?

R: We have changed "EFc" to "EF", which is consistent with the caption of section 2.4.

EF is the Enrichment factor defined as the concentration ratio of a given metal to that of Al. It is a reliable measure of crustal dust, normalized to the same concentration ratio characteristic of the upper continental crust (Wedepohl, 1995), calculated with the following equation: "EF=" ãĂŰ"(X/Al)" ãĂŮ_"snow" /ãĂŰ"(X/Al)" ãĂŮ_"crust" Line 25: Q is the goodness of fit parameter calculated excluding points not fit by the model, which is defined as samples for which the uncertainty-scaled residual is greater than 4. The best solution in PMF is typically identified by the lowest Q value along with the path and may be imagined as the bottom of a trough in the multidimensional space.

Comment 24: Page 9 Line 1: Should not this section be referred to as Results and Discussion? And not only Results as it is now. Line 4: I'm confused with the number of samples. Here you state over 67 and in section 2.1 ∼67. Please clarify. Line 5: The second sentence of section 3.1 is vague and not necessary for this section. As mentioned previously, please elaborate on this in the methods section.

R: We have changed "Results" to "Results and Discussion". Line 4: The number of samples is 67 and we have modified them. Line 5: We moved the sentence into the method section, and reconstructed the method section.

Comment 25: Page 9 Lines 7-10: Here the authors provide a range for the higher values, what is the range for the lower values? Lines 15-16: This is interesting that there is no difference between seasons since several other authors have observed the contrary. This should be elaborated on in the revised manuscript. Lines 17-29: These sentences should rather be included in the methods section, assisting with data interpretation.

R: According to the Table 1, the range for the lower median values of "C" _"BC" ˆ"est" is 23-53 ng g-1 in the Xiaodongkemadi, Hariqin, and Gurenhekou glaciers. Yes, we found that there is no apparent difference of ILAPs in each glacier between monsoon and non-monsoon seasons in these glacier regions. We noted several reasons could lead this discrepancy. First of all, all of the ice samples were collected in an individual time.

Another major issue is that except the long-range transport of ILAPs, local air pollutants can also affect the ILAPs in the glacier via wet and dry deposition. For instance, Li et al. (2016) indicated that the Fossil fuel contributions of BC is much higher in the Laohugou No. 12 glacier (close to Qiyi glacier) due to human activities. Finally, based the study by Xu et al. (2009), the ILAPs in the TP glacier also can be affected by the European air to reach that location on the eastern plateau, and thus lead the greater proportion of the heavy leading of the BC in the glacier. For instance, the mass concentration of BC in the Muztagh Ata and Xiaodongkemadi glaciers in northern part of Tibetan Plateau is much higher than that in the East Rongbuk, Noijin Kangsang and Zuoqiupo glacier, which are located in the southern part of Tibetan Plateau. Above all, we noted that the reasons could be very complicated, and the possible explanation has been added in the result section in Page 13, Line 12-23. Lines 17-29ïijŽWe have moved the sentences into the method section based on the reviewer's suggestion.

Comment 26: Page 10 Lines 2-5: Low Åtot means that the LAP originated from combustion sources? Lines 16-21: I find it confusing with these couple of sentences here as the previous sentences discusses Ånon-BC, and the sentences before that Åtot. First discussion on Åtot, and then into Ånon-BC. Lines 27-29: I do not understand this reasoning and how the sentences leading up to this reasoning support it. Line 29-30: Section 3.1 started with introducing the LAP concentrations and now they come back continued in the following sentences. The structure of this section would be better by having the concentration discussion intact.

R: Based on previous studies, the variation in the absorption Ångström exponents for urban and industrial fossil fuel emissions is typically in the range of 1.0-1.5 (Millikan, 1961; Bergstrom et al., 2007), which is slightly lower than that of biomass burning ranging from 1.5 to 2.5 (Kirchstetter et al., 2004; Bergstrom et al., 2007). Meanwhile, the lower median values of the absorption Ångström exponents in Xiaodongkemadi glacier is about 2.1, so the results indicated that the emission of the ILAPs in Xiaodongkemadi glacier likely originated from the combustion sources. Lines 27-29: We have deleted

the sentence. Lines 29-30: See our reply to comment 5.

Comment 27: Page 11 Line 2: What do you mean by individual period? Lines 4-5: How is a decreasing trend observed? Please include information in the manuscript that show the trend. Line 7: How is that due to heavy human activities? Lines 5-8: Please check this sentence structure. Line 8-9: In what sense is there good agreement with Ming et al. 2013? Line 16: What does the sample depth range presented here correspond to? Line 20: In table S1 I find ISOC to be 9.16 ppm, not 8600 ng g-1.

R: As shown in Table S1, the sampling time for each sample in seven glaciers was different, so we noted that all of the ice samples were collected in the individual period from 2013 to 2015. Lines 4-5: The sentences have been modified as “Based on our sampling locations shown in Figure 1, the Qiyi, Qiumianleiketage, Meikuang, Yuzhufeng glaciers were located in the northern part of Tibetan Plateau, while Xiaodongkemadi, Hariqin, Gurenhekou glaciers were located in the southern part of Tibetan Plateau. As shown in Figure 4, the median values of the "C" _"BC" ˆ"est" and "C" _"ISOC" obviously showed a significant decreasing trend from the northern TP to the southern TP.” Lines 5-8: The sentence has been rewritten as “Comparing with Hariqin, Xiaodongkemadi and Gurenhekou glaciers, the relative higher values of the "C" _"BC" ˆ"est" and "C" _"ISOC" in Qiyi, Qiumianleiketage, Yuzhufeng, and Meikuang glaciers are mainly influenced by human activities.” Lines 8-9: We have modified the sentence as “The mass concentration of BC in northern TP glaciers was higher than that in southern TP glaciers, which shows a good agreement with Ming et al. (2013).” Line 16: We have rewritten this sentence more clearly as “The depth of these ice samples collected in Yuzhufeng glacier is ranging from 15 to 45 cm as Table S1 shown.” Line 20: We have corrected this mistake. Reference: Ming, J., Xiao, C. D., Du, Z. C., and Yang, X. G.: An overview of black carbon deposition in High Asia glaciers and its impacts on radiation balance, Adv. Water Resour., 55, 80-87, 2013.

Comment 28: Page 12 Lines 5-6: The previous sentences suggest that it is not similar emission sources. Please clarify. Lines 6-8: A similar statement has been made earlier

in the manuscript. Please combine these observations. Line 17-18: Do you mean increased from the bottom to the top? Lines 20-21: How were the samples more complicated? Line 26-28: Would it not lead to lower LAP concentrations at the surface the way this sentence describes it now? with LAP being scavenged with meltwater?

R: We agreed with the reviewer, and deleted the sentences in Page 12 Lines 5-6. Lines 6-8: The similar statement in the earlier manuscript has been deleted. Lines 17-18: Except sites 7, 8, and 9, the mixing ratios of ILAPs in the ice samples increased remarkably from the top to the bottom from the Figure S4. Lines 20-21: Comparing with the other glaciers, there is no significant trend of the vertical profile in the mixing ratios of ILAPs in Xiaodongkemadi glacier (see Figure S5), so we indicated that the vertical profiles of the mass mixing ratios of BC, ISOC, and Fe for the ice samples in Xiaodongkemadi glacier were more complicated than those in the other regions. Lines 26-28: During snow melting process, black carbon (BC) and other insoluble light-absorbing particulate impurities (ILAPs) were retained at the snow surface, because their scavenging efficiency with meltwater was <100%. Therefore, the mass concentrations of ILAPs in surface snow increased with snow melting (Doherty et al., 2013). Reference: Doherty, S. J., Grenfell, T. C., Forsström, S., Hegg, D. L., Brandt, R. E., and Warren, S. G.: Observed vertical redistribution of black carbon and other insoluble lightâĂŘabsorbing particles in melting snow, J. Geophys. Res.-Atmos., 118, 5553-5569, 2013.

Comment 29: Page 13 Line 6-7: This is the wrong Conway et al reference, should be 1996 instead of 2002. Please check this also in the reference list.

R: Sorry, we have modified the reference as follows: “Previous studies have also illustrated that the ILAPs could become trapped and integrated at the surface of the snowpack due to melting and sublimation to enrich the surface concentrations (Conway et al., 1996; Painter et al., 2012; Doherty et al., 2013)”. Reference: Conway, J. H., Hardin, R. H., and Sloane, N. J. A.: Packings in Grassmannian spaces, Experimental mathematics, 5, 139-159, 1996. Doherty, S. J., Grenfell, T. C., Forsström, S.,

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Hegg, D. L., Brandt, R. E., and Warren, S. G.: Observed vertical redistribution of black carbon and other insoluble lightâĂŘabsorbing particles in melting snow, J. Geophys. Res.-Atmos., 118, 5553-5569, 2013. Painter, T. H., Bryant, A. C., and Skiles, S. M.: Radiative forcing by light absorbing impurities in snow from MODIS surface reflectance data, Geophys. Res. Lett., 39, L17502, doi: 10.1029/2012gl052457, 2012.

Comment 30: Page 14 Lines 15-17: Please provide a reference for this. Lines 17-19: I do not find this to be the case in given reference. Lines 23-27: How is that?

R: We have provided a reference in Lines 15-17, and modified the reference in Lines 17-19. Then, the sentences have been rewritten as “There is a very large fraction representing the average WSOC (>80%) to TOC, which can absorb solar light and enhance cloud formation through their direct and indirect climate effects (Ram et al., 2010). These results are highly consistent with previous study showing that fossil fuel combustion plays a key role in leading to the higher fraction of WSOC to total organic carbon (TOC) (Zhang et al., 2012)”. Reference: Ram, K., Sarin, M. M., Strawa, A. W., Kirchstetter, T. W., and Puxbaum, H.: Spatio-temporal variability in atmospheric abundances of ec, oc and wsoc over northern india, J. Aerosol Sci., 41, 88-98, 2010. Zhang, X., Liu, Z., Hecobian, A., Zheng, M., Frank, N. H., and Edgerton, E. S.: Spatial and seasonal variations of fine particle water-soluble organic carbon (wsoc) over the southeastern united states: implications for secondary organic aerosol formation, Atmos. Chem. Phys., 12, 6593-6607, 2012.

Comment 31: Page 15 Lines 1-2: Is this a general statement or results of this paper? My guess is the former, and if it is that, I would place this statement in the introduction of this paper. Line 13-15: That there is a small contribution from dust is, to my knowledge, not consistent with that results presented earlier in the manuscript. Please clarify this.

R: Yes, it’s just a general statement, and we have moved this sentence into the introduction section. In the former section, we analyzed the fraction of BC and non-BC contents in the ice samples. We should note that the non-BC contents are not only

include OC, but also include MD. However, we demonstrated the median fraction of light absorption by ILAPs is only ∼13% due to MD based on the PMF receptor model in section 3.4. As a result, we indicated that there is no conflict between our major results.

Comment 32: Page 16 Lines 11-12: What do you mean 'for the anthropogenic emission source'? I find this sentence confusing.

R: Except the natural dust source, we referred to attribute the major industrial pollution and biomass burning sources to the anthropogenic emission sources, so we noted that the natural dust source and anthropogenic emission source are both non-negligible to the light absorption by ILAPs in the TP glaciers according to the previous analysis.

Comment 33:As a last note, I wanted to comment on the in-text citations, are they done by year or alphabetical order? I did not find an order to this. Please check this throughout your manuscript.

R: We have arranged the in-text citations in chronological order throughout the manuscript.

Please also note the supplement to this comment:
https://www.the-cryosphere-discuss.net/tc-2018-86/tc-2018-86-AC2-supplement.pdf

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2018-86, 2018.

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