Referee comments on Wang et al: Quantifying light absorption and its source attribution of insoluble light-absorbing particles in Tibetan Plateau glaciers from 2013-2015

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

Major comments:

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).

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).

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 have 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).

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 $Å_{non\text{-}BC}$ 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).

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.

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.

After the revisions of the manuscript results have been carried out, the conclusions should obviously be updated as well, so that they are reflecting the findings in this paper.

Specific comments:

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.

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

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.

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

Page 4

Line 3: The abbreviation 'ILAPs' should first be written out.

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.

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.

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.

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.)

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.

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?

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?

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 clear.

Page 6

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

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?

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 ±?

Page 8

Line 6: What are EF_c? Please write out the abbreviation.

Line 25: What does *Q* values stand for?

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.

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.

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.

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}.

Page 12

Lines 5-6: The previous sentences suggests 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?

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.

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?

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.

Page 16

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

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.