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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Single particle characterization of black carbon aerosol in the Northeast Tibetan Plateau, China" *by* Q. Y. Wang et al.

Anonymous Referee #2

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This paper reports black carbon aerosol measurement at a potentially important site, Qinghai Lake in the Tibetan Plateau with a Single Particle Soot Photometer (SP2). While there are really little aerosol data in this special region, the results, as presented in the manuscript, made me a little disappointed. Except providing the basic dataset, neither was the dataset analyzed and discussed enough, nor were the conclusions obtained in a solid way. Despite QHL is a special sampling site, I have not seen much interesting or unique scientific findings. Local sources were discussed mostly in the text, which makes this special dataset less useful to the community. I have many major concerns on the manuscript as below, and suggest that the current manuscript should be rewritten with much better data analysis and resubmitted.

Major concerns: 1. While the authors stated in the abstract that QHL is affected by the East Asian summer monsoon, Indian summer monsoon, winter monsoon, and the westerly jet stream, indicating that the meteorology there is unique, there is really litter discussion about how the meteorology affected the observation results? How is this dataset in a short campaign representative for the general situation at QHL? Why is this dataset important scientifically? This is one of the key issues for the current manuscript. In addition, the meteorology should be well discussed in order to examine the features of the regional or long range transport of aerosol at this special site.

2. Line 21 and the following, page 21950. How was this SP2 configured for the scattering and incandescent detectors? The detectors decide the detection ranges of BC particles and mixing state.

3. Line 7, page 21951. Why was the calibration conducted over a range of 125–400 nm mobility diameter? As it is feasible to select particles with a size less than 100 nm by DMA, the higher minimum size used would affect the measured SP2 detection limits in this campaign. What are the measured detection limits in this campaign? In Figure 5, the BC data below 100 nm were presented. It is a little bit confused and should be clarified.

4. Line 11, page 21951. "The uncertainty in the rBC mass determination is $\sim$25% due to uncertainties in the rBC mass calibration, sample flow measurement, and estimation of rBC mass outside of SP2 detection range." How was 25% calculated? As this uncertainty is campaign-dependent, the process to obtain this specific value should be provided with some details.

5. Line 17 and the following, page 21951. As to the method to identify BC mixing state, although the "time delay" method was previously used by Schwarz et al., 2006, there are new (and possibly better) methods also used by Schwarz et al. (2008). Why was the old method chosen by the authors (including Schwarz)? The threshold of 2 us seemed not to be a clear criterion from Figure 2. Also, to my knowledge, SP2 cannot

identify mixing state for all BC-containing particles. What was the detection range for mixing state in this campaign and how are the current results representative for all the BC particles. As SP2 detects particles one by one, it is possible to see the distribution of BC mixing state versus BC particle size, which will be helpful to address the above issue.

6. Page 21953. It is better for the authors to make a clear table for the comparison of BC concentrations, with necessary sampling information for the other sites.

7. Lines 10-22, page 21954. As to the diurnal variation, I did not see apparent high concentrations around 08:00 LST corresponding to local residential activities as the authors stated, especially when considering the uncertainties of the hourly averages. The authors should also avoid using the wording "heavily influenced by local rBC sources", because they actually did not have enough evidence to say that.

8. Lines 17-19, page 21955. How was the 30-nm layer estimated for this campaign? While the authors citing Schwarz et al. (2008) is fine, it may not be good for this campaign, because of the different instrumental configuration and mixing state identifying methods.

9. Figure 6. I do not think there is useful information as the authors discussed, as the regression is really too weak. The interpretation of this figure in the aspect of sources is too arbitrary and without effective support. The text between lines 9-15, page 21956 is only kind of assumption.

10. Lines 21-29, page 21957. The calculation of MAC in this part is of little significance. Firstly, the authors should very clearly state that the MAC talked about in this paper refers to the MAC of BC on an aethalometer filter, not the true MAC in the real atmosphere. While the authors already concluded that "non-BC aerosol species strongly affect the Aethalometer at QHL" in line 11, they then made a contradictory assumption that "assuming that rBC is the only absorbing component in the ambient aerosol" in line 25. Therefore, the MAC calculated here is useless in terms of reflecting the BC

property in the air. Actually, the MAC here should be only regarded as a correction factor for aethalometer measurement.

11. Lines 7-18, page 21959. Since CO is a long life-time species, the background level of CO would largely determine the BC/CO ratios, which should be talked about. In terms of identifying sources using BC/CO ratios, I believe that BC/delta-CO is more frequently used in the literature as it can exclude the influence of the background CO. The authors should carefully go though the literature and select effective index. Also, the authors should make a table to clearly list the relevant results in the literature for better comparison, with necessary information included, like sampling time, site, method, etc.

Minor concerns: 1. Line 14, page 21949. condensation of emitted gases => condensation of emitted semi-volatile gases.

2. Line 16, page 21951. "entering the instrument to below either 1.0-μm or 2.5-μm diameter depending on setup." When for 1.0 and when for 2.5? Why? How would this strange setup affect the results?

3. Line 7, page 21953. The QHL conc. was compared with an urban atmosphere, Shenzen. How far between the two sites? Any significance for such a comparison? There are also similar problems in the other parts. Any comparison should produce some scientific information, not just list data.

4. Line 10, page 21955. Any more explanation for the small second mode?

5. Lines 26-28, page 21955. "Variability in this metric was larger during the day than at night, suggesting that nighttime aerosol was less influenced by individual sources for short times." This sentence should be rephrased to make the point clear.

6. Lines 15-20, page 21957. Since the BC at QHL has been found significantly influenced by local sources, it is not safe for the authors to state that the GAW site, 130 km away, has a similar situation for aethalometer measurement. This statement should be

removed.

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