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Interactive comment on “Carbonaceous aerosols recorded in a Southeastern Tibetan glacier: variations, sources and radiative forcing” by M. Wang et al.

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The status of the Tibetan glaciers are greatly concerned by the public. Black carbon (BC) has been presumed by some studies to be a significant factor inducing their rapid melting. The depositing history of carbonaceous aerosols (CAs) (including organic carbon (OC) and BC) in the surfaces of the glaciers is very important for understanding long-term variations of CAs' emissions and their potential impacts on these glaciers. Source attribution via modelling is another important contribution of this work to interpreting where these CAs came from. From these view angles, this work has its own value worthy of being published, but necessarily after very serious and intensive mod-

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ifications. The major concerns and minor comments on this work are listed in below.

Major concerns

1. The paper should not properly be entitled as “Carbonaceous aerosols recorded in a Southeastern Tibetan glacier...” for the main dataset used in this work has been pre-published by Xu et al. (2009) in PNAS and the primary aims are not to reintroduce the variations of carbonaceous aerosols recorded in the ice core. It is suggested to be changed to “Modelling of carbonaceous aerosols for their sources and forcing based on an ice core in the Zuoqiubu glacier”.

2. This work introduced a new concept, OC’s forcing, which has not been widely recognized by the societies. The introduction of a new concept must be previously supported by measurement, as we all know. The OC’s forcing is just like an aerolite in the whole paper. The authors should firstly list some literatures that clarified OC has the characteristic of radiation absorption in snow as well as it does in atmosphere claimed by Bond and Bergstrom (2006) and Kirchstetter et al. (2004). The online SNICAR model only simulates the reductions of snow albedo caused by BC and dust, but cannot have the ability to simulate the forcing of OC. The mass absorption cross-section of OC in the atmosphere cannot be directly used for it in snow.

3. Source attribution of CAs using CAM5 model may be an innovative highlight of this work. However, the authors missed introducing many details, which may confuse readers. For example, in the method description, the authors did not introduce the initial weather field that drives the model including the meteorological parameters and their temporal and spatial resolution. The performance of the model in the highly elevated Tibet region is not well known. If my understanding is right, the authors used an inventory of CAs in 2000 to calculate the deposition of BC and OC on a large-scale region including the Zuoqiupu glacier. Does that mean the whole history of CAs depositing in the glacier can be retrieved through only one-year emissions, or just the one-year CAs’ concentrations recorded in the ice core in 2000? However, the comparison between

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the results of measurements and modelling is missing in the paper, which might be extremely concerned by readers.

4. The presentation of this paper is very difficult to understand. Sometimes I have to guess the meaning that the drafting author really meant to explain. I strongly suggest the authors would find some language specialists related to this study to improve the presentation largely.

Minor comments (not including the language errors)

1. Line 2 in Page 19720. How “high” is the temporal resolution. . .? The resolution of the ice core record is not introduced in the paper. The word “high” here is some like a tree without a root.

2. Line 10 in Page 19720. “. . .followed by East Asia (14% and 21%, respectively)”. I don’t understand what “14% and 21%” mean.

3. Line 14 in Page 19720. Should point out that South Asia as a main contributor “in the annual mean”, because in different seasons main contributor changes.

4. Line 17 in Page 19720. Be careful to state the forcing of OC.

5. Line 17 in Page 19720. “. . .and organic carbon (OC), which also absorbs in the near infrared, but more weakly than BC”. Here there should be a reference.

6. Line 2 in Page 19721. “Jacobson, 2001” should be changed to a more representative literature, e.g., the most recently released IPCC report.

7. Paragraph 2 in Page 19721. Ming et al. (2013) in Adv. Water. Resources suggest BC deposited in Himalayan and High Asian glaciers cannot significantly affect their energy balances, which is a very minority but different viewpoint from most literatures listed here, which should not be neglect here.

8. Line 21 in Page 19721. There should be “burning” after “biomass”.

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9. Paragraph 1 in Section 3.1. When heavy pollution occurred in South Asia, the aerosol monitoring in the Tibetan hinterland can also detect the signal of high BC concentration (See Zhao et al., 2013, “Observation of carbonaceous aerosols during 2006–2009 in Nyainqentanglha Mountains and the implications for glaciers” in Environmental Science and Pollution Research).
10. Line 11 in Page 19725. The “sink” should be “deposition”.
11. Paragraph 1 in Page 19725. This paragraph should be moved to Section 2 (method).
12. Line 28 in Page 19725. I don’t understand the relationship between Wang et al. (2014) and this work. Obviously, Wang et al. did an Arctic work.
13. Paragraph 1 in Page 19728. The sentence “More recent . . . South Asia” is confusing.
14. Paragraph 1 in Page 19729. The OC/BC can be used to separate the dominant sources of CAs. It is my understanding that if the ratio is higher than 60, biomass burning should be the primary source of CAs. However, the neglected OC can influence the reality of the ratio and thus miss judging the burning sources, although it cannot alter the trend of OC/BC.
15. Last sentence of paragraph 1 in Page 19729. Improved combustion technology not only reduce the emission of BC, but that of OC, which can result in the unclear varying of OC/BC.
16. Line 7 in Page 19731. There should be “ng g⁻¹” after “4.4”.
17. Line 10 in Page 19731. The forcing of BC increases from 0.75 W m⁻² in 1956–1979 to 1.95 W m⁻² in 2006, which is comparable to the result of East Rongbuk glacier conducted by Ming et al. (2008) and believing to be true.
18. Summary and conclusions. This part should be shortened.

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19. Figure 3. The wind field in the surface and of 500 hPa in the Tibetan Plateau area is very doubtful. The mean elevation of the TP is as high as 4000 m above sea level. In meteorology, wind field in this area is usually blanked in these geopotential heights.

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