We thank Dr. Jing Ming and the anonymous referee for their valuable comments to the manuscript and their constructive suggestions for improving the presentation quality. Below, we explain how the comments and suggestions are addressed (responses in blue color) and make note of the changes we have made to the discussion paper (in italic).

Referee #2 (Dr. Jing Ming)

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".

Response: We agree with the referee that the ice core data set used in the present study has been originally published by Xu et al. (2009) to elucidate the important influences of black carbon on the melting of Tibetan glaciers. Although the long-term variations and seasonal dependence of BC and OC have been shown in the literature, contributions from specific source regions and potential changes in combustion sources have not been analyzed, and the increasing importance of OC-in-snow radiative forcing has not been raised. Undoubtedly these issues are also important to address.

In the present study, we calculated the source contributions to BC deposition in the southeastern Tibetan glacier using the state-of-the-science global aerosol-climate model (CAM5) with an explicit source tagging technique, which suggests a dominant contribution of South Asia emissions to the annual mean deposition of carbonaceous aerosols in the southeastern Tibetan Plateau. Moreover, we did further in-depth analysis to the changes in combustion sources based on the ice-core recorded temporal variations of OC to BC ratio, and revealed the increasing contributions of biomass burning and/or coal combustion versus oil to carbonaceous aerosols. In addition, the influence of carbonaceous particles on radiative fluxes at the glacier surface has been estimated using the SNICAR model.

Considering the referee's concern on the main focus of this study, we have changed the title to "Carbonaceous Aerosols Recorded in a Southeastern Tibetan Glacier: Analysis of Temporal Variations and Model Estimates of Sources and Radiative Forcing".

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.

Response: Following the referee's suggestion, we have added a paragraph to mainly introduce the role of OC acting as a light-absorbing impurity in snow/ice as follows:

BC is often the most important light-absorbing impurity in surface snow because of its strong absorption of solar radiation. Effect of BC in snow on surface albedo reduction and resultant positive radiative forcing have been widely addressed and reported (e.g., Warren and Wiscombe, 1980; Clarke and Noone, 1985; Hansen and Nazarenko, 2004; Hadley and Kirchstetter, 2012; Flanner et al., 2007; 2009; McConnell et al., 2007; Ming et al., 2008; Kaspari et al., 2011; Qian et al., 2011). In contrast, the impact of OC in snow has not been widely assessed because of its relatively weak light-absorption over the entire spectrum compared to BC, and because of large uncertainties associated with OC light-absorbing properties and measurements of OC in snow. However, there have been increasing interests in light-absorbing OC (a.k.a. brown carbon) and its radiative effect in the atmosphere (e.g., Kirchstetter et al., 2004; Andreae and Gelencsér, 2006; Hoffer et al., 2006; Moosmüller et al., 2009; Yang et al., 2009; Lack and Cappa, 2010; Cheng et al., 2011). Hoffer et al. (2006) estimated that humus-like substances as part of OC from biomass burning contribute ~7% to the absorption over the entire spectrum, which is not negligible. Yang et al. (2009) highlighted that as the contribution to absorption from BC decreases towards the ultra violet wavelengths, absorption due to brown carbon and dust becomes more significant, and they reported that at an observation site near Beijing brown carbon contributes over 10% to total absorption at mid-visible wavelengths. Thus the contribution of OC in snow to the surface albedo reduction is likely to be important, which has also been considered in recent climate

modeling studies (see Qian et al., 2014 for a review).

BC's atmospheric mass absorption cross-section (MAC) is generally adopted in radiative forcing estimation at snow surface. In the present study, we use BC and OC's atmospheric MAC with respective value of 7.5 m^2 g^{-1} and 0.06 m^2 g^{-1} at 550 nm to calculate the radiative forcing in snow. Although MAC of OC shows a different spectral dependence from that of BC (Barnard et al., 2008), for simplicity we use the same spectral dependence of MAC for OC in the SNICAR model calculation. Therefore, the absolute value of radiative forcing derived from OC may has larger uncertainties than that of BC, but the increasing trend in the contribution of OC since 1990 is robust. We have made a note of this in the manuscript as follows:

Note that the mass absorption cross-section of OC is highly spectral dependent (Kirchstetter et al., 2004; Hoffer et al., 2005; Barnard et al., 2008; Yang et al., 2009). It increases greatly towards shorter wavelengths. Consequently, the absorption of OC may be largely biased. It is also important to note that we didn't consider variations in chemical compounds of OC, or the changes of OC during sample filtration. Although the estimation of OC radiative forcing herein is rather crude, the increasing trend should be robust.

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

Response: We agree with the referee that source attribution of BC at this Southeastern Tibetan Plateau site using the CAM5 model is novel. Our original plan was to just use

this modeling tool to guide the more in-depth analysis of ice-core measurements for the contrast between monsoon and inter-monsoon means and the long-term trend. The evaluation of the model performance in simulating BC in the entire Tibetan Plateau and some sub-regions is documented in a separate paper. The referee's understanding is correct that BC deposited to a single model grid (about 200km by 200km, in which the Zuoqiupu glacier locates) is assumed to be uniformly distributed and compared to measurements. The referee also raised a good point on the BC emission inventory. The IPCC AR5 year-2000 emissions are supposed to be representative for the present-day global scenario (for one decade). However, uncertainties in emissions are very large, up to a factor of 2-4 for global mean, and could be even larger for some specific regions like South Asia and East Asia. This is part of the reason why we did not design the model experiment to simulate the whole historical record of BC in the ice core, but rather a 10-year period to demonstrate the impact of meteorology (and associated transport and removal) on the seasonal dependence of BC deposition in the target region and the lack of longer-term trend of deposition without considering the temporal variation of emissions. To address the referee's concerns, we have added more details about the method description and model configuration, and made note of the limitations of coarse grid global models in this kind of model-observation comparison.

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.

Response: Thanks for the suggestion. The writing has been improved by native English speakers.

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.

Response: The annual accumulation of snow/ice was around 2 meters at the drill site, and the oxygen isotope samples were cut at 10 cm intervals, resulting in 18 ice samples per year on average. To reduce uncertainties in using the filter-based method, about 9 liquid samples per year were analyzed for carbonaceous aerosols. This is the way that we

defined it a "high" temporal resolution record. We have now made it clear in the manuscript as follows:

The annual accumulation of snow/ice at the drill site was around 2 meters on average. The oxygen isotope ($\delta^{18}O$) samples were cut at 10 cm internals, and BC and OC samples at 10-25 cm, resulting in 18 and 9 samples per year on average, respectively. Thus this ice core provided a high temporal-resolution of $\delta^{18}O$, and BC and OC concentrations.

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

Response: 14% and 21% are the contribution of East Asia emissions to the southeastern Tibetan Plateau during non-monsoon season and on an annual basis, respectively. This was written in the context of the sentences ahead of it. To avoid confusion, this sentence has been revised as follows:

The model results show that South Asia has the largest contribution to the present-day (1996-2005) mean BC deposition at the ice core drilling site during the non-monsoon season (October to May) (81%) and all year round (74%), followed by East Asia (14% to the non-monsoon mean and 21% to the annual 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.

Response: The text has been revised.

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

Response: Following the referee's comments, we have made some of the statements regarding the OC radiative forcing more precise in the main text.

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.

Response: We have added some references on this in the main text.

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

Response: The reference has been added.

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.

Response: Thanks for pointing this out. We have now made the point and added the reference.

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

Response: The text has been corrected.

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).

Response: Thanks for pointing this out. We have noted this in the paper and added the reference.

10. Line 11 in Page 19725. The "sink" should be "deposition".

Response: The text has been revised.

11. Paragraph 1 in Page 19725. This paragraph should be moved to Section 2 (method).

Response: We have moved the first half of the paragraph, which describes the division of BC source regions, to Section 2. As a result of this change, the original Figure 2 and Figure 3 have to be switched. The second half of the paragraph describing the different circulation patterns during monsoon and non-monsoon months does not fit well in the methodology. We decide to keep it in original Section 3.2 where circulation patterns are analyzed for BC transport.

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.

Response: Wang et al. (2014) was the first study that described the BC source tagging capability in the CAM5 model and applied this method to identifying sources of BC in the Arctic. The present study uses the same modeling tool and experimental setup as in Wang et al. (2014) but focuses on the Southern Tibetan Plateau. To avoid any confusion, we have removed the reference at this particular location.

13. Paragraph 1 in Page 19728. The sentence "More recent . . . South Asia" is confusing.

Response: We have rewritten the sentence as follows.

BC and OC emissions during 1996-2010 from Lu et al. (2011) are also illustrated in Figure 6 to extend the emission data to cover the entire time period that the ice core data span. We note that the emission data from Lu et al. (2011) are only for India, which is the largest energy consumer and carbonaceous aerosol-emitting country in South Asia.

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.

Response: We agree that higher OC/BC ratio indicates larger contribution of biomass burning to carbonaceous aerosols. We use the OC/BC ratio results from Cao et al. (2005) to quantitatively support the statement that biomass burning produces the highest OC/BC ratio, followed by coal combustion. However, the value of OC/BC ratio varies with fuel types and combustion processes, especially for biomass burning (Yanju Chen, 2014, Yu Zhao, 2014, personal communication). We also agree that the underestimation of OC in the ice samples is very likely to result in lower values of OC/BC than in the atmosphere but it is less likely to alter the trend of OC/BC ratio in the ice core record, and we have noted this in the paper.

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.

Response: We agree, and the sentence has been rewritten as follows:

Improved combustion technologies may have reduced both BC and OC emissions from the combustion of the same amount of fuels, but the influence on OC/BC ratio is unclear. Presumably improved combustion technologies after 1990 in South and East Asia did not dominate the OC/BC ratio.

16. Line 7 in Page 19731. There should be "ng g^{-1}" after "4.4".

Response: Added.

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.

Response: Thanks for pointing this out. We have added a comparison in this regard, noting the agreement between our result and some others including Ming et al. (2008):

Our estimate of mean BC forcing is lower than the estimated Eurasian radiative forcing (2.7 W m^{-2}) in spring (Flanner et al., 2009), but it's comparable to that in the East Rongbuk glacier over Himalayas, which was in the range of 1-2 W m^{-2} (Ming et al., 2008). Kaspari et al. (2009) reported a three-fold increase in radiative forcing from BC in snow over Himalayas after 1975, which is consistent with the increasing trend in our results.

18. Summary and conclusions. This part should be shortened.

Response: We meant to include a summary of motivation, methodology, results and conclusions in this section, which is just a different style of presentation. The entire section is not too long, so we decide to keep it as it is.

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.

Response: The original Figure 3 (now Figure 2) illustrates the wind fields at 500 hPa (top panel), which is around 5500 m above sea level, and at the surface (bottom panel). As the areas in Tibetan Plateau having surface pressure lower than 500 hPa are very small and

scattered, the wind fields at 1.9° × 2.5° resolution (at which small-area high elevations are smoothed) would not be blank over the Tibetan Plateau. As for the surface winds, they always follow the terrain but could be at different elevations spatially depending on the topography.

Referee #3

General Remarks

1. The authors need to go through the paper to clarify the concepts of seasonal variation of BC and OC and the seasonal variation of the ratio of OC/BC. The two seasonal variations should be discussed separately owing to different reasons behind them. For example, the authors listed potential reasons on page 19725 lines 9-14 to explain the seasonal dependence of the relationship between OC and BC shown in Fig.2. However, the explanations help only to explain the seasonal variation of BC and OC, not the ratio of OC/BC.

Response: We totally agree with the referee that the two seasonal variations are very likely due to different reasons. We have followed the suggestion to discuss them separately, and significantly rewritten the relevant section.

2. In additional to the change of emission and atmospheric removal, the change of atmospheric circulation pattern during monsoon and non-monsoon seasons also contributes to the carbonaceous aerosols at studied site. It is good that the authors describe the atmospheric circulation pattern during two seasons on page 19726 lines 12-26. It would be even better if the authors could show the corresponding circulation patterns on a figure, such as Fig 3.

Response: Following the suggestion, we have added the corresponding circulation patterns on Figure 2 (the original Figure 3).

3. Fig. 5 indicates a rapid increase of the ratio of OC/BC in the Zuoqiupu ice core after 1990. However, this increase is not observed from the regional emissions on the figure compiled by Bond et al., 2007 and Lu et al., 2011. Therefore, the conclusion of "Because

of the stronger increasing trend in OC than BC during 1990-2006, the contribution of OC to the total radiative forcing cannot be neglected" (page 19731 lines 17-18) needs to be further checked to its regional representation.

Response: (We believe that this comment is about Figure 6 rather than Figure 5.) We agree that there is a discrepancy between the OC/BC ratio of deposition and the OC/BC ratio of emissions in terms of the increasing trend. Although South Asia has been identified as the primary contributor to the BC deposition at the sampling site, changes in total emissions in South Asia still cannot be directly translated to changes in the deposition. In the model simulation for source attribution and in the plot of emission trends (Figure 6), we used total emissions in the entire South Asia. However, we believe that spatial distributions within major sources regions, including South Asia and East Asia, can also significantly affect the deposition over the sampling site. On the other hand, there are many known and unknown uncertainties in the emission inventories for OC and BC, and the higher order quantity, OC/BC ratio, could be even more uncertain. Therefore, our conclusion in that regard is purely based on the ice core recorded trends of BC and OC deposition fluxes, which indicates an increasing contribution of OC to the carbon mass and radiative forcing. We made it clear in the paper that this conclusion is only for the glacier site rather than South Asia.

Specific Comments

1. Page 19721 lines 5-6: Is this correct for the sentence "During the cold and dry winter monsoon seasons. . ."? The monsoon season should be June-Sept as indicated in the abstract.

Response: We meant the South Asian winter monsoon as opposed to the summer monsoon, but we agree that it is a little confusing to use both of them in the same paper. We have removed "monsoon" here.

2. Page 19721 line 23-24: The emissions of biofuel consumption and biomass burning are typically categorized separately.

Response: According to the referee's comments, we have revised the text as follows:

However, as reliable biomass consumption data are hard to obtain, estimates of BC and OC emissions from biomass burning are ambiguous and incomplete.

3. Page 19722 line 11 and 14: It would be better to use "source types" instead of "combustion sources" here.

Response: The text has been revised.

4. Page 19722 line 15: Is it robust to estimate regional RF impact with only Zuoqiupu glacier ice core data?

Response: We totally understand the referee's concern here. BC and OC concentrations and, therefore, the resultant radiative forcing vary spatially and temporally, especially at places having sharp changes in elevation and/or in close proximity of emission sources. Without doing a comprehensive survey over many more sites, we cannot conclude on how large area the Zuoqiupu glacier ice-core data can represent. Nonetheless, according to a comment by the other referee, the magnitude of our BC forcing estimates is very close to that by Ming et al. (2008) for a different glacier. The magnitude of BC-in-snow forcing is also comparable to the springtime Eurasian forcing estimated by Flanner et al. (2009). Meanwhile, a similar increasing trend of radiative forcing from BC in snow was shown in Himalayas region by Kaspari et al. (2009). We have added such comparisons with these previous studies to the paper, and made a note in the paper saying that the estimated forcing is only for the Zuoqiupu glacier. Please also refer to our response to the comment (#17) by the other referee.

5. Page 19723 lines 6-7: Are there any differences between EC and BC in terms of their concentrations? Do the authors need to convert the measured EC to BC concentration in order to compare with model simulation?

Response: Measurement data on EC and BC concentrations are method specific. EC represents thermally refractory carbon in the sample, while BC denotes the extent of light-absorption by the sample as measured using optical methods. Thus they can have large differences, for example, when the sample contains other light-absorbing material that is likely to be attributed to BC. This is not an issue in model simulations where BC is

actually defined more like the measured EC. We didn't need to convert the measured EC to BC, and we use the term "BC" to be consistent with that in the model and in most previous studies.

6. Page 19723 line 8-9: Why can the ice-core measurements only account for the water-insoluble part of OC from aerosol emissions?

Response: This is because the filter-based method we used for sample analysis cannot capture OC dissolved in the liquid samples (after the ice samples melt). Most of water-soluble OC can move through the filter. We have noted this more clearly in the paper.

7. Page 19723 lines 23-25: Could you elaborate on the approach of "offline mode"? Do you mean the simulation used an approach typically used for chemistry transport model (CTM)? How does this approach provide dynamic feedback between cloud/precipitation and aerosol?

Response: This approach is similar to that typically used for chemical transport models (CTMs) in the sense that model meteorological fields (e.g., winds, temperature and pressure) are constrained with reanalysis products, which are supposed to be closer to reality. Therefore, there is no need to consider dynamical feedback between cloud/precipitation and aerosol. However, the microphysical interactions (e.g., impact of aerosol on cloud/precipitation processes, and the feedback from cloud/precipitation on aerosol wet scavenging and removal) are predicted in our model with more sophisticated schemes than in many CTMs. We believe this approach is more suitable for the present study than the conventional free-running approach.

8. Page 19725 lines 2-3: The numbers in text are opposite of the numbers shown in Fig. 2.

Response: Thanks for catching this mistake. It has been corrected in the text.

9. Page 19725 lines 9-11: Other potential reasons, such as the change of transport path and strength, may also have an impact.

Response: We agree, and this has been added to the text. We also added more discussions on the potential reasons, in response to the general comment #1.

10. Page 19726 lines 6-9: This could be simply because the time span of non-monsoon season (Oct-May) is much longer than that of monsoon season (Jun-Sept).

Response: The BC deposition fluxes in Figure 5 are seasonal averages (i.e., mass per unit area and time), not accumulations during the time periods. The seasonal variation of BC and OC deposition fluxes was calculated in a similar way. Both model results and measurements show higher deposition in non-monsoon season than in monsoon season.

11. Page 19727 lines 20-21: Why did BC and OC fluxes drop between mid-1990 to 2002?

Response: This is an interesting point, but we don't have a plausible explanation for it. It was likely due to the decreased energy consumptions in source regions. As shown in Figure 6, the coal consumption, which was the most important fuel in South Asia, stayed at a relative stable level during this period. The correlation between coal consumption in the primary source region and the response in BC and OC deposition fluxes at the sampling site could potentially indicate a causal connection, but it may also possibly be a coincidence. After all, changes in the spatial distributions of emissions in the major source regions could also affect the source-receptor relationship.

12. Page 19728 lines 14-15: What about after 2003 during which the OC/BC ratio in ice core still increased but the OC/BC emission ratio decreased as shown in Lu et al., 2011?

Response: Please also refer to our response to the general comment (#3) on this. There are many potential reasons for this discrepancy, among which we believe uncertainties in the total emissions and the spatial distribution of emissions within the major source regions likely play a key role.

13. Page 19729 lines 7-8: Why would including water-soluble OC lead to increased bias?

Response: As the filter-based method used in the analysis of liquid samples (melted from ice samples) cannot capture water-soluble OC, the OC herein has been underestimated. As a result, the OC/BC ratio was very likely underestimated, which is what we meant by the "low" bias. To avoid such confusion, we have changed the phrase "low bias" to "been underestimated".

14. Page 19729 lines 11-14: Is it possible that there is a positive trend of influence from South Asia?

Response: The model simulation with fixed emissions but varying meteorological conditions shows that meteorology alone (and associated transport and removal processes) didn't cause a discernable trend during the 10 years (1996-2005) in BC deposition (Figure 5) and OC/BC ratio (figure not shown). Thus the increasing trend of BC and OC fluxes is very likely due to changes in emissions from major source regions, which is consistent with the trends in South Asia emissions. Along the same line of reasoning, OC/BC ratio is likely due to the relatively more contribution of coal and/or biomass than oil to the total emissions from South Asia. The evidence seems to support a positive trend of influence from South Asia, but a concrete conclusion cannot be drawn because of the disagreement with OC/BC ratio of emissions.

15. Page 19729 line20 (also see Figure 6): According to the authors' explanation in section 3.4 first paragraph, the more coal combustion relative to oil consumption, the higher OC/BC would be. However, Fig. 6 shows that despite the increase of coal combustion being much slower than the increase of oil consumption during 1996-2002, corresponding OC/BC increased dramatically.

Response: First of all we note that we made a mistake in the units of oil consumption in the original Figure 6 (lower-right panel), which were supposed to be in "thousand barrels daily". We have now revised the plot using same units of "million tonnes" for both oil consumption data and coal consumption data. This change however does not affect any of our interpretation of the results.

The increasing trend of OC/BC ratio in Figure 6 could be due to an increased contribution of coal and/or biomass compared to oil in terms of source type, as emissions from coal and biomass combustion both produce higher OC/BC ratio than from oil (Cao et al., 2005). IEA (2009) reported that coal, biomass, and oil accounted for 41%, 27%, and 24%, respectively, of the primary energy demand in 2007 in India. Although the consumption of biomass was lower than coal, the OC/BC ratio for biomass burning emissions is much higher than from coal burning (60.3 vs. 12.0). BC emission factor is also higher for biomass burning (varying from 0.48 ± 0.18 g kg^{-1} for savanna and grassland burning to 1.5 g kg^{-1} for charcoal burning) than coal (0.2 g kg^{-1} for most combustion conditions) and oil combustions (0.3 g kg^{-1} on average, varying from 0.08 g kg^{-1} for heavy fuel oil to 0.66 g kg^{-1} for diesel) (Andreae and Merlet, 2001; Bond et al., 2004, 2007). The consumption of biomass, which is also mainly influenced by population, has potentially increased with the accelerated growth of population in South Asia. Thus, although the increase of coal combustion was slower than oil consumption during 1996-2002, the expansion of biomass usage may have determined the increase of OC/BC ratio. Similar points have been discussed in the third paragraph of section 3.4, and in addition, we made note of some of the above discussions.

16. Page 19730 lines 1-15: How does this knowledge help to interpret the measured and simulated data?

Response: As mentioned in the response to the comment (#15) above, this paragraph is supposed to address the role of biomass burning as an energy resource in South Asia. The possible growing emissions from biomass burning may have been another important contributor to the increasing trend of OC/BC ratio, but we don't have the long-term consumption data like those of coal and oil.

17. Page 19730 lines 26-27: Could the authors explain what the MAC scaling factor within spectral broadband is and how to get these factors for BC and OC?

Response: The mass absorption cross-section (MAC) is a wavelength-dependent parameter that is generally adopted to derive optical properties of snow impurities and calculate their radiative forcing in snow. In the online SNICAR model

(http://snow.engin.umich.edu), MAC is normalized within spectral broadband with a mass absorption cross-section of 7.5 m^2 g^{-1} at 550 nm for BC. There is another input parameter, called the MAC scaling factor, which can be used to adjust/rescale the wavelength-dependent MAC for BC in each broadband. A MAC of 7.5 m^2 g^{-1} at 550 nm is commonly used for uncoated BC particles (Bond and Bergstrom, 2006; Flanner et al., 2007), so we use the MAC scaling factor of 1 (i.e., 7.5/7.5). For OC, we adopted the MAC value of 0.6 m^2 g^{-1} at 550 nm that is typical for light-absorbing OC measured in Asia and from biomass burning emissions (e.g., Kirchstetter et al., 2004; Yang et al., 2009). The MAC of OC does show different spectral dependence from that of BC (Barnard et al., 2008), but for simplicity we just use the same spectral dependence of MAC for OC in the SNICAR model calculation, which makes the MAC scaling factor to be 0.08 (i.e., 0.6/7.5). We made a note in the paper that with such assumptions the estimated magnitude of OC radiative forcing may have large uncertainties but the trend in OC forcing is more robust.

18. Page 19732 line 6-7: This site is located in complex terrain, affected by both South Asia and East Asia. There may be a need for further investigation of its regional representation in terms of concentration and deposition before using the ice core measured carbonaceous aerosols to infer regional radiative forcing.

Response: We agree that further investigation is needed to understand the spatial variations of BC and OC concentrations in snow/ice and their influences on the regional radiative forcing. Here we have made it clear that the estimated radiative forcing is only for the Zuoqiupu glacier.

19. Page 19732 lines 14-15: Again, we cannot exclude changes in atmospheric transport path and strength as potential reasons.

Response: Agreed and the text has been revised accordingly.

20. Table 1: Adding BC emissions over these regions would help to explain the impact per unit emission in source regions.

Response: Following the referee's suggestion, BC emissions over the four source regions have been added to Table 1.

21. Technique corrections:

Page 19723 lines 18-19: "wet scavenging" and "removal by precipitation" are redundant.

Response: They are different processes (i.e., nucleation scavenging and wet deposition) in the model, but for simplicity in the description we have changed to "wet removal by precipitation".

Page 19731 lines 5-6: This repeats page 19730 lines 20-23.

Response: Following the referee's suggestion, we have deleted the repeated sentence.

Figure 2: Add the time period of the ice core measurements.

Response: The time period has been added in the caption of Figure 2.

Figures 3 and 4: Change location of ice core from color pink to black.

Response: The color has been changed in revised figures.

Figure 6: The dashed lines on subfigures are extended beyond 1979.

Response: The dashed lines are meant to be reference lines to show the increasing trend of OC/BC ratio, BC, and OC after 1980.

References:

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