

We thank the two anonymous referees for their insightful comments to the manuscript and helpful suggestions for improving the presentation quality. Below, we explain how the comments and suggestions are addressed (our point-by-point responses in blue) and make note of the changes we have made to the discussion paper, attempting to take into account all the comments raised by both referees.

Referee #1

This paper presents the results of a one-year's model simulation of black carbon aerosols over the Tibetan Plateau. The authors use NCAR's CAM5 model, implemented with a source tagging technique, to quantify the BC over different regions of the Tibetan Plateau from various geographical regions (the surrounding areas in particular) and two major source sectors (biofuel/biomass and fossil fuel). They also characterize the seasonal variations of BC concentrations, deposition and radiative forcing on the plateau as well as their source attribution, and analyze the model results in very detail. The paper is interesting and should be a welcome addition to the literature. I would suggest the paper to be published after the following questions/comments have been well addressed.

General comments:

1) The tagging method used in this study is not well introduced. Although the title of Sect. 2.1 is written to comprise "the source-tagging method", no content related to the method can be found in this subsection at all. In Sect. 2.2, several equations are given, but these equations are far away from the model tagging technique. The authors refer to Wang et al. (2014) for the source-receptor relationships. However, only the similar equations were presented in that work. I would suggest that the authors give much more detailed description about the treatment of BC in CAM5, especially the tagging method. For example, in which aerosol modes BC are taken into account? Are they all assumed to be hygroscopic and internally-mixed? How many tracers are added in the model to tag the BC from a specific region? Is there a tracer added for each mode? Is there a tracer for BC in snow? Is the tagged BC assumed to undergo the same dynamic and microphysical processes as the normal BC does in the model? Perhaps, you do not need adding a tracer to tag the BC, but it should be described clearly how to achieve that.

Response: CAM5 employs a modal aerosol module (MAM) to represent aerosols in multiple log-normally distributed modes, with internal mixing assumed for aerosol species within each individual mode, including a 3-mode standard representation (MAM3) and a more complex 7-mode representation (MAM7). The major difference between MAM3 and MAM7 related to carbonaceous aerosols lies in the treatment of aging. In MAM3, black carbon (BC) and primary organic matter (POM) are emitted

into the accumulation mode that contains highly hygroscopic species such as sulfate and sea-salt, while in MAM7 BC and POM are emitted into a primary carbon mode, which contains no other species. BC is hydrophobic upon emission, and thus the hygroscopicity of the primary carbon mode depends on the assumed hygroscopicity for POM. As hygroscopic species condense onto the primary-carbon-mode particles, the particles are become more hygroscopic and are transferred into the MAM7 accumulation mode. The rate of transfer is controlled by uncertain aging parameters, and the availability of gas precursors (Liu et al. 2012).

In this study, we apply the direct source tagging technique developed by Wang et al. (2014) to the accumulation-mode BC in the MAM3 treatment. BC particles emitted from sixteen geographical BC source regions and two emissions sectors (i.e., biomass burning & biofuel emissions and fossil fuel emissions) in each of the regions are tagged and explicitly tracked. Instead of using the global emissions from all sectors for the original one BC variable, the thirty two regional/sectoral emissions provide sources to the respective tagged BC mass mixing ratio variables that are all added to the accumulation mode, including both interstitial and cloud-borne states. All physical and dynamic tendencies (e.g., transport, dry and wet removal) are calculated explicitly for the tagged BC variables in the same way as the original BC mass mixing ratios. Also, when aerosol optical properties are calculated, all of the tagged BC mass mixing ratios contribute to the volume-mean refractive index of the accumulation mode that is used in the radiation calculation.

We have now added such detailed descriptions to the revised manuscript.

2) While the paper focuses mainly on the quantification of the contributions to BC on the Tibetan Plateau from different source regions, the analysis of various physical processes is relatively weak. It is stated that the study is to “characterize the fate of BC particles emitted from various geographical regions” in both the Abstract and Conclusions. However, the lifetimes of BC from different regions are not investigated as expected. With the definition given in Page 86 (the equation should be numbered), the authors investigate the efficiency of tagged sources in affecting the BC on the Tibetan Plateau (Fig. 7). In addition to the geographical distance or atmospheric transport pathway between the receptor and a source region, are there any other factors (e.g., aerosol chemistry, microphysical processes and dry/wet deposition) affect the estimated efficiency?

Response: The main focus points of this paper do not include the analysis of various physical processes that contribute to aerosol removal and lifetime in CAM5, which have been extensively evaluated in previous studies (e.g., Wang et al., 2013, 2014), including the aging, wet deposition, and lifetime of regional BC. However, to address

the referee’s concern, here we calculate the annual and seasonal mean lifetime of BC from different geographical regions and sectors. These results are summarized in Table R1. On the globe average, BB BC has a longer lifetime than FF BC in all seasons, especially in winter (6.9 vs. 3.1 day), which is likely because biomass burning emissions in the BB category have initial injection heights of up to 6 km, resulting in less removal at lower altitudes. The availability of co-emitted hygroscopic species (in the same accumulation mode of the MAM3 aerosol treatment) also impacts the scavenging and wet removal rate of BC. This also partly explains the variability of lifetime of BC originating from the different source regions and sectors. Regarding the seasonal cycle, BC emitted from the major source regions (e.g., SAF, EAS, SEA, SAS) has substantially lower lifetime in summer (JJA) than in the other seasons, likely due to relatively strong removal by summer monsoon precipitation. The table has been included in the Supplement (Table S2) and the main message is summarized in the paper.

According to its definition in Eq. 2 (now numbered), the efficiency can be affected by regional emission rate and factors that influence the amount of BC emitted from the specific source region reaching the receptor region (i.e., HTP). The main factors are the transport pathway determined by large-scale circulations and convective lifting and dry/wet removal rate during the transport that is determined by aerosol properties, aerosol microphysics, and cloud microphysical processes. These processes are all represented in the prognostic equation for aerosols. We just did not focus on analyzing the individual budget terms in this study.

Table R1. Global annual and seasonal mean lifetime (day) of BC emitted from the 32 tagged source regions/sectors, as well as from BB and FF sector over the whole globe (all source regions combined).

		DJF	MAM	JJA	SON	ANN
ARC	BB	2.2	3.0	4.7	3.9	4.5
	FF	2.1	1.9	1.6	1.4	1.7
NAM	BB	2.2	3.0	4.1	3.6	3.4
	FF	2.1	2.8	3.9	3.2	3.0
CAM	BB	2.6	4.9	2.8	1.7	3.9
	FF	3.5	5.0	2.4	2.3	3.3
SAM	BB	3.5	3.7	8.4	6.8	6.8
	FF	2.9	3.0	4.1	3.9	3.5
EUR	BB	2.0	2.7	6.4	4.0	4.0
	FF	1.9	2.5	4.9	2.9	3.1
NAF	BB	8.8	10.8	10.2	10.0	10.0
	FF	5.4	8.9	12.9	9.1	9.1

SAF	BB	9.3	6.1	5.1	7.1	7.2
	FF	4.4	4.1	4.3	3.9	4.2
MDE	BB	5.9	8.9	12.6	11.3	9.9
	FF	6.2	9.1	12.4	11.5	9.8
CAS	BB	2.8	4.7	7.5	4.7	5.9
	FF	2.7	5.4	9.7	5.8	5.9
SAS	BB	7.4	7.5	1.9	4.4	5.5
	FF	7.5	7.6	2.3	4.9	5.6
EAS	BB	3.2	3.6	2.5	3.1	3.1
	FF	3.1	3.3	2.4	3.0	2.9
SEA	BB	2.7	3.5	1.8	1.8	2.5
	FF	2.2	1.8	1.4	1.5	1.7
PAN	BB	5.7	4.5	5.6	6.8	6.1
	FF	4.2	3.3	2.5	2.9	3.2
RBU	BB	2.2	3.8	4.4	3.4	4.0
	FF	2.0	2.5	4.9	2.6	3.0
HTP	BB	6.6	6.0	4.0	6.5	5.8
	FF	6.1	6.2	4.9	6.4	5.9
ROW	BB	2.2	2.6	4.1	2.7	2.9
	FF	2.3	2.5	2.4	2.5	2.5
Globe	BB	6.9	5.0	4.6	5.2	5.4
	FF	3.1	3.6	3.8	3.5	3.5

3) Comparisons with previous studies, e.g. the work of Kopacz et al. (2011) and Lu et al. (2012), are not sufficient in the current version of the paper. What advantages and limitations of the methods used in these studies, where the same topic are addressed?

Are there any disagreements or uncertainties for the BC source contributions and radiative forcing over the Tibetan Plateau based on these studies?

Response: Kopacz et al. (2011) employed a global chemical transport model, GEOS-Chem, and its adjoint to identify the originating locations of BC arriving at five glacier sites (i.e., five model grid-cells as the receptors) in the Himalayas and Tibetan Plateau (HTP) in year 2001. This method can provide a global distribution of emissions that directly contribute to BC concentrations at receptor locations. While the adjoint model accounts for nonlinearities in the relationships between aerosols and emissions, the results are still merely tangent linear derivatives (gradients). In contrast to our source tagging approach, the adjoint model results are not source attributions but rather the source-receptor sensitivities, which can be interpreted as the effectiveness of incremental changes to existing emissions. Our direct tagging method

can attribute sources to predefined geographical regions as well as emission sectors. While the adjoint approach has the advantage of not predefining source regions, it does need to perform simulations for each defined receptor region. Our tagging method also has the flexibility to do source attribution of BC mass mixing ratio at any model layer and the surface dry/wet deposition within a single simulation for any receptor regions.

Lu et al. (2012) used an improved back-trajectory approach to analyze the origin of BC transported to the HTP during 1996-2010. Based on a large set of seven-day back trajectories arriving at the given receptor locations in the HTP, BC emissions, and transport efficiencies (representing the transport ability of BC from source to receptor by taking into account advection, aging and removal processes of BC), they derived the overall transport characteristics of BC to the HTP and showed the spatial distribution of sources for BC reaching the HTP region. The statistical analysis of trajectories has good accuracy on short time scales for source regions with close proximity to the receptor, but this approach has limitations in determining contributions from distant sources. The seven-day back trajectories (spanning the average BC lifetime) might be sufficient to characterize the source origins of air masses arriving at the boundary layer of HTP (e.g., 500 m arrival height in Lu et al.'s study), but are probably not adequate for BC being transported in the mid- and upper-troposphere that could contribute significantly to the total column burden but less to BC deposition and boundary layer concentrations.

With the different approaches, Kopacz et al. (2011), Lu et al. (2012) and the present study all show that South Asia and East Asia are the main source regions for BC transported to the HTP, while the magnitude of contributions from each of the source regions varies with season and receptor location. Although all of the three studies can provide quantitative contributions of emissions from the various source regions to BC in the HTP, a quantitative inter-comparison of the findings is quite difficult, given the differences in the definition of geographical source/receptor regions, emission inventories, time periods for model simulation, and analysis methods. Nevertheless, in addition to quantifying the contributions of source regions, our direct source tagging approach allows us to further break down regional contributions to sectors (i.e., fossil fuel vs. biomass & biofuel) and to characterize the transport pathways of individual regional/sectoral emissions. Compared to the way we calculated the spatial and temporal mean BC radiative forcing (in both the atmosphere and snow), Kopacz et al (2011) did offline calculations of instantaneous radiative forcing in the snow-covered regions only, while Lu et al. (2012) did not include any radiative forcing calculations. In addition, we also did source attributions for BC-in-snow forcing.

We have added a summary of the response here to the manuscript.

Referee #2

General comments:

The results provide an interesting way to view the relatively pristine HTP that is embedded among major carbon emissions hot spots. My comments reflect a general critique that the authors do not sufficiently motivate the finer points of the discussion. The most confusing points are related to efficiency and finer source-receptor relationships within the HTP, and I think these need clarification before the paper should be published. Otherwise, there are some very interesting emissions impacts results on a very sensitive part of the world (ie. the Third Pole). Figure 4 is fascinating!

There is some repetition in the first 200 lines of text that I would recommend streamlining. One way to do this would be to clarify why some of the studies are mentioned in this study. Bring their relevance to the foreground.

I have some issues trying to understand the utility of the efficiency metric that I think may require some further discussion by the authors before the paper should be published. My recommendation is that the entire efficiency discussion be deleted – it seems underdeveloped and seems to not support the main points of the paper. I also note, however, that I may have misunderstood the calculation, but either way, I requested a direct response to this.

Also, the source-receptor relationships within the HTP are interesting, but I do not understand why I am reading about them.

Response: Thanks for the general comments and suggestions. We have now made changes to the introduction section according to the specific suggestions below and other changes to streamline the flow. Regarding the use of the efficiency metric and why we further divide the HTP into five finer receptor regions, please see our responses to the same but more specific comments below.

Specific Comments:

Line 100: Citation for sentence starting with “A large fraction . . .”? Could this be better quantified to say approximately what fraction?

Response: changed to “Over 60% of BC in the present-day atmosphere originates from anthropogenic activities (e.g., Bond et al., 2007; Lamarque et al., 2010)”

Line 102: “Road map” is an odd choice of words for this study. Perhaps just be more specific and less flowery about what makes some scientists think that BC mitigation is a low-hanging fruit.

Response: changed “manage the road map of climate forcing” to “slow down the climate change”

Line 159: Repetitive with line 117. I recommend deleting one and adding to the one that remains in the text a preview of why this study is highlighted in your study (ie. as a basis of comparison about source regions of BC as cited later in the manuscript).

Response: deleted the repetitive sentence in line 159 and added more context near line 117 to explain why Kopacz et al. (2011) is highlighted in the present study. Please also see our response to comment #3 of referee #1 regarding a more detailed comparison between our study and two other ones including Kopacz et al. (2011).

Line 165: What do you mean by “different inventories” since I’m only aware of the CMIP5 inventories, and this is the only one cited?

Response: We meant that there are “top-down” and “bottom-up” global emission inventories, and some emission data sets like the ECLIPSE include newly identified emissions from gas flaring and residential heating at high-latitudes. To avoid such confusion, we have deleted the sentence “BC emission datasets have large uncertainties (e.g., Bond et al., 2013), and there are different inventories available for climate modeling.”

Line 173-174: I don’t understand. Why is a ratio of biofuel to fossil fuel needed? Doesn’t Lamarque et al (2010) emissions include a biofuel category? If not, maybe simply stating that this is why a ratio is needed would be clearer.

Response: Lamarque et al. (2010) doesn’t directly provide biofuel and fossil fuel BC emission sectors, so we need a ratio of biofuel to fossil fuel. This has now been clarified in the text.

Line 200, 203: Does mass mixing ratio, deposition flux, surface mixing ratio as a BC property produce similar results as using column burden? It seems like MMR C value would be much different than column burden, unless this is a z-dependent C calculation. Either way, and similar to other comments, introducing this myriad of metrics is interesting, but it would be helpful to clarify why they are all needed. For example, are C values for MMR and SMR even discussed in this study? From Fig 6, I see deposition and column burden C values.

Response: The referee is correct that the relative contribution by a specific source could be significantly different between the BC column burden and mass mixing ratio (MMR) at a certain height (e.g., near the surface). The model simulated surface MMR is often compared to observations made at surface stations for model evaluation. For example, we did such a comparison in Figure 2. In Figure 4, we use BC vertical distributions to illustrate its transport pathways on the latitude-height cross-section.

The direct source tagging approach does enable us to do BC source attribution at any height (or model pressure level) in addition to that for the total column burden and deposition flux, and thus it is more flexible than the other approaches employed in previous studies (e.g., Kopacz et al., 2011; Lu et al. 2012). The total column burden is more relevant to the calculation of atmospheric BC radiative forcing, while the deposition flux largely determines BC-in-snow radiative forcing. Therefore, the source attributions for these two properties are presented in the paper (e.g., Figure 6). Although the source attribution of BC at the surface or any given height is not the focus of this study, we believe this capability is worth noting when introducing the metrics here.

Line 261: What emission uncertainties? Are these quantified in a peer-reviewed source?

Response: Using bottom-up inventory methods, Bond et al (2013) estimated an uncertainty range of 2 to 29 Tg yr⁻¹ along with a best estimate of about 7.5 Tg yr⁻¹ for the global BC emissions in year 2000. Cohen and Wang (2014) derived an optimized top-down estimate of global BC emissions, 17.8 ± 5.6 Tg yr⁻¹, a factor of two higher than commonly used global BC emissions. We have added a reference to this in the paper.

Line 285: Can this improvement for HTP be quantified in some way? The discussion around CAM5 simulated SCF and MODIS SCF uncertainty is muddled and missing a simpler metric of comparison. For example, the average correlation of the SCF in the study area for CAM5 vs MODIS 2001 and MODIS 2000-2013 should be illustrative. What is the average SCF in each season for the different comparisons? Can the improvement from CAM3 to CAM5 be better quantified beyond the citation to Qian et al. 2011? As it is, it's not very convincing to read about dramatic improvements without a number.

Response: Following the referee's suggestion, we have now calculated the correlation coefficient of SCF between CAM5 and MODIS and performed the statistical significance test (see Table R2). There are a total number of 52 model grid-cells over the HTP, and note that MODIS retrievals were mapped to the CAM5 grid. It shows that the CAM5 SCF is highly correlated with that of MODIS (both 2001 and 2000-2013) with the statistical confidence level greater than 99%, except for summer (JJA) when the linear correlation is significant only at 80% level. The results have been added to the paper.

We have also calculated the annual and seasonal mean SCF over the HTP for CAM5 and MODIS and added the numbers to the corresponding panels in Figure 3. The standard deviation of the MODIS SCF climatology, which indicates intra-seasonal

and inter-annual variations, is also calculated based on monthly mean SCF during 2000-2013.

The CAM3 model used by Qian et al. (2011) overestimates SCF by 20-100% during the cold season (November to April). The CAM3 spring (MAM) mean SCF is greater than 35%, while the CAM5 SCF (21%) in the present study that is in good agreement with the MODIS SCF (18±5%). We have added the quantitative comparison to the paper.

Table R2: Correlation coefficient (R) and its statistical significance (p) of SCF between CAM5 and MODIS

	CAM5 (2001) vs. MODIS (2001)		CAM5 (2001) vs. MODIS (2000-2013)	
	R	p	R	p
DJF	0.61	0.00000	0.61	0.00000
MAM	0.76	0.00000	0.83	0.00000
JJA	0.17	0.22496	0.18	0.20823
SON	0.88	0.00000	0.82	0.00000
ANN	0.78	0.00000	0.80	0.00000

Line 294: What is the mean SCF? This is never stated, and “very close” is too vague.

Response: The DJF mean SCF is 50%, which is now added to the text and also shown in the revised Figure 3. The “very close” wording has been removed.

Line 306, Figure 4: This is a very complex graphic, but I think very useful, provided some revisions are made to both the figure and the manuscript text. The figure either should be larger or broken into multiple parts. I think larger would work well given that the orientation of the figure currently does a great job with side-by-side comparisons of source regions impacts on HTP as a function of two seasons. The wind vectors right now are very challenging to read, but maybe a larger figure solves this? Also, somewhere, it should be stated why these 6 source regions were selected from the 16 on Fig 1. This is mentioned on Line 306 (“six major source regions”) but I would think that RBU would have some seasonal impact, especially during JJA when fire activity is high. I do not have the supplemental figures in my version of the manuscript, so I could not judge this.

Response: As the referee has correctly pointed out, the main reason to place the DJF and JJA panels in the same figure is for side-by-side comparisons. Also, the figure was designed to occupy a whole page for the portrait layout of final ACP publication,

which we will work on with the production editor at the typesetting stage.

The six source regions shown in the figure were identified according to the annual mean contributions. However, the referee is correct that RBU has a larger seasonal contribution (during JJA) than SAF. This has now been discussed in the text (Sect. 4.3). Sorry that the supplemental figures were not appended to the manuscript, but they are in a separate file that has a link on the webpage for the manuscript.

Line 328: This caveat (no seasonality in FF) should be discussed when talking about what FF means in Section 2.2.

Response: We have added a sentence in Section 2.2: “Note that emissions in the BB sector have seasonal variations, but the FF sector emissions used in this study have no seasonal variation at all.”

Line 334-337: This text would benefit from referencing your own Fig. 4

Response: added reference to Fig. 4 in the text.

Line 377, Section 4.3: This is a key section, but missing from the discussion as to why HTP is broken into multiple source regions. I expected to see this discussion before the conclusion (see my comment line 578) but nothing appeared. To me, the more interesting points of this study are the source-receptor analysis of HTP as a whole. Why should I be interested in more detail? What are the ramifications?

Response: As shown in Figure 2, both modeled and observed near-surface BC concentrations at different sampling site locations over the HTP have quite different seasonal mean values and variations. Model simulated seasonal BC column burden and deposition flux also vary with location on the HTP (Figure 4). Snow cover over HTP also has large spatial variability (see Figure 3). As a result, the annual and seasonal mean BC radiative forcings (in both the atmosphere and snow) vary significantly with location (Figure 8). We intend to quantify source contributions to BC in each of the sub-regions over the HTP. It turned out that the source-receptor relationships for BC burden, deposition and radiative forcing have large spatial variability as well (Figures 6 and 8). We believe this is more interesting than focusing on the HTP region as a whole. The HTP contains a large number of glaciers that distribute at many different locations (e.g., Figure 1 in Yao et al., 2012). Many studies have suggested that the BC in snow/ice may be partly responsible for the observed acceleration of glacier retreat in the HTP. The BC source attribution information for finer regions will be of more interest to researchers using ice-core BC retrievals at individual glaciers. Kopacz et al (2011) used the GEOS-Chem model and its adjoint to identify the originating locations of BC arriving at five glacier sampling sites in the HTP, even though they needed to perform simulations separately for each of the sites.

They also found that the magnitude of the contribution from each source region varies strongly with receptor locations (e.g., sampling sites). Our explicit source tagging method has the advantage of doing source attribution of BC for any receptor regions without rerunning the model simulation. Therefore, it is scientifically important and technically efficient to perform the source-receptor analysis for the HTP not only as a single receptor but also as multiple finer receptors.

We have now added this motivation in the paper.

Line 459: Perhaps I have misunderstood this efficiency metric, but here is where I stand on this and I would appreciate a defense/clarification: Section 4.4 seems unnecessary to support the main points of the study. I do not see how this efficiency metric is of much use, especially for HTP. HTP has practically no emissions and I do not see how anyone could practically expect HTP to develop major emission sources. SAS and EAS have enormous emissions. To standardize to present day emissions seems to minimize the impact of the largest emitters on HTP at least. In other words, all this efficiency metric highlights is that the S values in line 207 are divided by a very very small number. To make a more effective efficiency metric, wouldn't all emissions have to be uniform across the globe and source region perturbations of equal magnitude be applied to study the actual efficiency at which an equivalent emissions increase would have on a receptor region? Again, pardon any misunderstanding if I have missed something.

Response: The fractional contribution metric helps quantify the relative importance of individual source regions/sectors in affecting BC over the HTP and estimate the response to percentage change in sources, while the efficiency metric is more useful to characterize the sensitivity of BC in the receptor to absolute change (or per-mass perturbation) in regional/sectoral emissions. If the emissions were uniformly distributed over the globe as the referee suggested, the efficiency metric would be equivalent to the contribution metric. Because the efficiency is essentially the contribution normalized by emission strength, it is less dependent on emission rate in source regions than on removal processes and transport. The referee is correct that we mean to use the efficiency metric to measure the impact of equal magnitude of perturbations in different source regions on BC in the receptor region. Although the HTP local emissions are very minimal (see Figure 1b), their contribution to BC burden and deposition at all of the sub-regions is quite significant, even comparable to East Asia's in some of the regions (see Figure 6). In other words, the efficiency of local emissions in affecting HTP BC is very high (Figure 8), which means that the impact of per-unit-mass (or equal-magnitude) perturbation in emissions on BC over HTP is much stronger if the perturbation occurs within HTP than in any other source regions including SAS and EAS. Certainly, this metric is also useful for comparison

between other source regions and different seasons.

The efficiency metric is of more interest to policy makers for the purpose of mitigation action, which is not the focus of this study but is definitely worth mentioning. Therefore, we only use one figure and a sub-section to describe it, and we would like to keep it in the paper.

Line 565: This is a very clear summary of what I think are the key results – nicely written!

Response: Thanks!

Line 578: Similar to Line 377, can text be added to this paragraph clarifying why this regional receptor analysis is generally important? Otherwise, I would suggest eliminating this paragraph from the Conclusions section.

Response: please see the response to the line 377 comment above. We have now added a sentence here to clarify this.

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