Comments on "A global 3-D CTM evaluation of black carbon in the Tibetan Plateau" by C. He et al.

This manuscript evaluates a chemistry transport model of black carbon based upon surface observations of BC in surface air and in snow and BC AAOD over the Tibetan Plateau. The authors have evaluated the modeling of BC concentrations in snow, which is helpful to study the effect of BC on snow and ice. However, I suggest this paper can be considered for publication if it can fully address the major comments below. Most importantly, I have not found enough novelty in the present work. In the present manuscript, "using updated Asian anthropogenic BC emissions and global biomass burning emissions" seems to be the only improvement by this work, although it's still questionable that they have treated very well with the emissions. Aging of BC is from a published reference, but there is another study based on the same model but with a more complicated method to consider the aging process. Many important studies have not been discussed. I suggest the authors should explain clearly what have been done in previous studies, and what is being done for the first time in their work. See also some specific comments suggested to improve the manuscript.

Major comments:

(1) The authors have done a good job at evaluating the modeled results against observations, especially by considering the BC in snow. I am convinced with the quality of the work. However, the surface concentrations and BC AAOD have

been evaluated by many previous studies. For example, Fu et al. have evaluated the surface BC concentrations using the same model (Fu, 2011). The evaluation of BC AAOD has been done by Bond et al (2013). So, it's not very clear what is the novel contribution of this work. For aging of BC, there is another study, which is based on the same model but with a more complicated method (Huang, 2013). These important studies have not been discussed in the paper. The BC in snow is likely a new part in this study. However, it's not well documented, and some important relevant information are missed (e.g. where is the major source region for the snow BC over the Tibet Plateau?).

(2) The authors are using a global 3-dimensional chemical transport model at a horizontal resolution of 2 degree by 2.5 degree (or close to 200 km). It should be noted such a coarse resolution is difficult to capture the high BC concentrations at local scale, especially for urban sites. There is a recent study which quantified the effects of model resolution on simulating the surface BC concentrations in East Asia and South Asia (Wang, 2014). This spatial scale effect is important when comparing the modeled concentration over a large model grid to the observed concentration at a local site. There is a nested version of GEOS-Chem, which has been used by Fu et al. (2012). The authors should test the effect of using a higher-resolution model in their study.

(3) The authors states correctly in the abstract that there are deficiencies in emissions, but they failed to give sufficient discussion and consideration for that. In fact, there are many update of the emission inventory of black carbon in Asia

(Lu, 2011; Qin, 2012; Kurokawa, 2013; Wang, 2014). Notably, the spatial pattern of black carbon emission has been improved in these inventories. However, these progresses seem not to be noticed by the present study. Page 7324 (Sensitivity to BC emissions): In Lu's paper, the uncertainty of BC emission has been quantified by a Monte Carlo method. However, this important point also has not been discussed in the paper. Without considering the associated uncertainties, it's unreasonable to conclude whether an inventory underestimates the emissions or not. This uncertainty should be considered in the study and quantified by running the model with the lower and upper bounds of the inventory. This is especially important for BC AAOD, which is now underestimated by a factor of 2-4 in the model. In addition, there is a lack of uncertainty analysis for most discussions in the present paper.

Specific comments:

1) Abstract, Line 5: The authors state that they are using "updated Asian anthropogenic BC emissions". It's not clear which inventory they are updating against? The Lu and Zhang inventories have already been used in previous modeling studies.
2) Abstract, Line 7: The authors state that the model results are in good agreement with observations. However, this is not the case for surface concentrations at urban sites and BC AAOD.
3) Page 7312 (AERONET AAOD): The methodology for retrieving the BC AAOD from the AERONET observations is not very clear. First, which

version of the AERONET data are used in the study (level 2.0 or 1.5) ? Second, the AAOD are observed under conditions (clean-sky, daytime, ...) by AERONET, which are available for only part of the days throughout the year. However, it's not clear how the monthly and annual means are calculated in the model and observation? In fact, it makes senses when comparing the modeled and retrieved BC AAOD at the same days. Third, SSA is only available for days when the AOD is larger than a value. How do you get the SSA for low AOD days? The details should be provided.

4) Page 7312 (Model description and simulations): Since you are evaluating the modeled BC AAOD, what optical parameters (e.g. mass absorption cross-sections) are used in the model? It should be explained with the associated uncertainty discussed.

5) Page 7315 (BC aging): The authors state that "in the absence of nucleation and coagulation, the BC aging rate can be parameterized as a linear function of OH concentration." However, there is no evidence that the nucleation and coagulation are not important in the studied region, especially close to the source regions.

6) Page 7315 (BC aging): In fact, in addition to Liu et al.(2011), there is another recent study of the aging of BC (Huang, 2013). Huang et al. have improved the parameterization for the BC aging in the GEOS-Chem model (the same model as that used in the present study). According to Huang et al., there should be at least two parts: oxidation effects;

condensation-coagulation effects, and the aging rate should be the sum of the two effects.

7) Page 7319, line 15: The residual error doesn't make sense for low concentrations (also for Fig. 4).
8) Page 7324 (Sensitivity to BC aging parameterization): In Liu's paper, the new parameterization of aging has a significant impact on the seasonality. Does it also influence the seasonality of surface concentrations and BC AAOD in your study? It should be discussed.
9) Table 6: Units are missed for mean error, mean absolute error, and RMSE.

Reference:

Bond, T. C., Doherty, S. J., Fahey, D. W., et al. Bounding the role of black carbon in the climate system: a scientific assessment, J. Geophys. Res.-Atmos., 118, 1–173, doi:10.1002/jgrd.50171, 2013.

Fu T M, Cao J J, Zhang X Y, et al. Carbonaceous aerosols in China: top-down constraints on primary sources and estimation of secondary contribution. Atmos. Chem. Phys., 2012, 12(5): 2725-2746.

Huang Y, Wu S, Dubey M K, et al. Impact of aging mechanism on model simulated carbonaceous aerosols. Atmos. Chem. Phys., 2013, 13(13), 6329-6343.

Kurokawa J, Ohara T, Morikawa T, et al. Emissions of air pollutants and greenhouse gases over Asian regions during 2000–2008: Regional Emission inventory in ASia (REAS) version 2. Atmos. Chem. Phys., 2013, 13(21):

11019–11058.

Qin, Y. and Xie, S. D. Estimation of county-level black carbon emissions and its spatial distribution in China in 2000, Atmos. Environ., 2011, 45: 6995–7004.

Wang, R., et al. Exposure to ambient black carbon derived from a unique inventory and high resolution model. Proc. Natl. Acad. Sci. U. S. A, 2014, 111(7), 2459–2463.