This paper attempts to update an emission inventory of anthropogenic organic carbon (OC, including primary OC and secondary OC ) and elemental carbon (EC) based on previous reported observational studies. I have serious concerns about the section 3 especially on SOC part because of the limitation of SOC estimation method used in the study. Without more comprehensive understanding of assumptions and limitations of EC tracer method, which is used to be estimate SOC, the present work towards SOC part is inaccurate or even erroneous. The paper should clear state in the paper what are new findings about carbonaceous aerosols in China. I suggest including more new findings towards carbonaceous aerosols studies (observations and source apportionment studies) in China. Discussion of source and POC/SOC source apportionment based on OC/EC ratios alone is not sufficient. From the method used in the current work, SOC and biomass burning OC could not be separated, therefore, the conclusion of the enhanced SOC across in China can be misleading. Similarly, the trend of OC/EC ratios should be carefully examined by re-considering measurement uncertainty in different studies by different analytic methods. In my opinion, the methodology applied by the authors does not allow distinction of SOC and POC in a national scale.

Major comments:

Table 1 does not include EF for open biomass burning. Because field burning of agricultural residues is a very common practice during China's harvest season and is thought to be major sources of OC and EC in specific seasons, this should be considered into the inventory. EF in small stoves would not be the same as that in open burning. I suggest also list the other EFs (transportation) in this table.

Sec 2.3: "…residential sector is estimated to have accounted for 52 ±3 % of total EC emissions..". The uncertainty of 3% seems very small. Did the authors use error prolongation from all possible uncertainties from all factors in the equation? Or 3% only represent standard derivation of results from different years in 2000-2012. Please clarify it and show the detailed approach to estimate uncertainty in the SI.

"During the period, emissions from the residential sector increased by 34 %, principally due to the growth of coal consumption". Uncertainty should be given after 34%. How about the variation of EF from coal combustion during 2000-2012? If EF from coal is decreased due to high combustion efficiency in the modern stove, the total emissions will not simply dependent on coal consumption.

Line 28 in 8991 line 1 in Page 8992: the trend of OC/EC ratios in different years is very likely smaller than the uncertainty. For example, OC/EC uncertainty alone in any thermal-optical methods will not be better than 10% (Schmid et al., 2001; Schauer et al., 2003; Hitzenberger et al., 2006). The EF in the current study includes literature values from previous studies in which different OC/EC method were used. As a result, the uncertainty in OC/EC ratio alone will be easily higher than 20%. One should carefully evaluate such a trend when OC/EC is decreased from 1.58 in 2000 to 1.52 in 2012 by <4%.

Sec 2.3. In this section, I think only primary OC is estimated, so it is better to use POC instead of OC when only primary OC is considered.

Page 8995 lines 19-22: Please clearly OC and EC uncertainty from different analytical methods. If uncertainty of OC and EC is larger than 25%, it indeed would sometimes lead some statistical errors for comparison in this work. For examples, without evaluation of uncertainty from OC and EC measurements in different studies, it is not scientific sound to conclude that "ambient EC concentrations in north are higher than those in south" (page 8998 lines 13-15) because the difference between north and south in urban and remote regions may be not statistical significant if the uncertainty of EC measurement is included in the comparison. The difference of OC/EC ratios between the north and south may be smaller than the uncertainty from different analytic methods conducted in different years.

Page 8998 lines 14-16: it is not meaningful to compare OC/EC in north and in south because these results have been obtained in different years (even sometimes in different seasons) and by different methods. There could be many other factors (year/season to year/season variability and/or measurement uncertainty) controlling OC/EC ratios.

Page 8999 line 1-5: the statement needs references. Biomass burning and coal combustion emissions also could contribute substantially (sometimes may be dominant over vehicle emissions) in both the JJJ and YRD regions. The source apportionment in urban regions of China is still not clear; such a statement should be avoided. More discussions and new literatures should be included in this section.

Page 8999 Lines 17-28: EC-tracer method is a traditional (and very old) method to estimate SOC which is often used in the same campaign by the same OC/EC analytic method. There are several disadvantages in the SOC estimation as already discussed in the current paper. However, such an

approach could not be applied in different regions of China when OC and EC results have been derived from different campaigns with different methods.

Page 9001 Lines 17-25: A high SOC fraction in OC in winter could be also due to high OC/EC ratios in aerosols from biomass burning and/or coal combustion. This part should be POC, which is mistaken as SOC by the EC-tracer method.

Page 9001 lines 26 to Page 9002 lines 2: Huang et al., (2014) revealed that a possible mechanism of high SOC formation in winter, but this does not mean that SOC formation in summer (under condition of high temperature and more sunlight) is smaller than SOC in winter. Can we consider enhanced condensation of semi-volatile organic compounds at colder temperatures simply as an increase in SOC? SOC generally should imply some photochemical transformations prior to aerosol partitioning, which may not be the case here. Please clear give the definition of SOC.

Specific comments:

Page 8986, Lines 4 Q. Zhang et al., 2012 should be changed to Zhang et al., 2012. The similar mistakes should be corrected in the MS.

Page 9006 Lines 1-3: This contrast could be also due to the uncertainty from OC/EC measurement and inventory estimation. And the inter-annual trend can be easily biased from the selection of literature values used in the paper.

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