We appreciate Prof. Eleanor Browne for her careful review and thoughtful comments. The manuscript has been revised accordingly and the comments are responded point-by-point. The comments are in black and our response in blue or red.

Comments:

(1) Line 222: delete “chemical”

Done.

(2) Line 314-315: In the response to my earlier comments it was stated that 22-23 January was included in the PMF analysis. Please delete this sentence if that is true.

Sorry about that. This sentence has been deleted.

(3) Fig 2b: The right axis shows the % not the accumulated data number.

Thank you very much! We have reworded this sentence as following (line 1298-1299):

"The right axis in (b) shows the percentage of the data number in each bin to the total data number."

(4) Fig 5: The inconsistencies between O3 and NO (non-zero O3 at night with 50 ppb NO) need to be acknowledged in the text. This was provided in the response to earlier comments, but could be expanded upon in the text.

Agree. We add a sentence in the updated manuscript as following (line 503-506):

"Note that during night time the diurnal variation of O_3 still showed a background (~10 ppb) although the concentrations of NO were up to 50 ppb. This inconsistent was likely due to the instrument drift in the MEP station during long-term observation, however it seemed that the pattern and the amplitude of the diurnal variation of O_3 were reasonable."

(5) Line 523: Is this really a smaller peak or is there just a constant offset? It is hard to tell from the figure. It seems that an offset could be explained by background/residual aerosol being more oxidized in the summer than it is in the winter. This should be clarified in the text.

The morning peak of O/C during summer 2012 slowly increased from 8:00 am, while the peak during winter 2014 increased from ~12 pm. It is indeed that there was a significant offset between these two O/C diurnal variations and we add a sentence in the updated manuscript to mention this phenomenon as following (line 529-531):

"In addition, an offset was existed for the O/C diurnal variation between the 2012 and 2014 studies especially for night time, which suggested that background/residual aerosol in the summer were more oxidized than in the winter."

(6) Line 660-661: Were the PAH signals observed in this data set? If so, an extended mass spec showing the PAH ions and the differences between BBOA and CCOA should be included in the supplement. Similarly, how is the PAH entry in Table 2 derived. This must be clarified.

Sorry for not clear on PAH data. We obtained the PAH data using V-mode data from SQUIRREL panel based on the default fragmentation table. We add a sentence in the updated manuscript to clarify this as following (line 298-300):

"In addition, the concentration of PAH was generated in SQUIRREL panel based on the default fragmentation table (Dzepina et al., 2007)."

For PAH signals in BBOA and CCOA, because we performed PMF analysis below m/z 120, no PAH signals were observed in these two spectra. In the original sentence, we tried to emphasize this phenomenon on other studies. We have rephrased this sentence as following (line 668-670):

"In addition, high PAH signals had been observed in the CCOA MS (Sun et al., 2016), and this is consistent with previous results that the coal combustion could be a dominate source of PAHs in China (Okuda et al., 2006)."

(7) Line 734-735: I find this wording very confusing and the figure hard to interpret. There are so many data points in this figure that it is unclear whether the reader can truly see the data points or if some points are hidden under others. Additionally, I believe the text means to say that when Org/PM is less than ~0.5 AND POA is < 15 ug/m3 there is a tighter correlation.

Yes, the figure is indeed a bit complex due to the overlap of data points. Nevertheless, the aim was to show that when POA is low and Org/PM_1 fraction is low, SOA and POA correlated relatively well, while when POA mass concentrations and Org/PM_1 fractions were large, the correlation became weak, so as to qualitatively represent the significant role of POA for heavy pollution events, compared to that during summertime. Also, we agree with the editor, and we have further make it clear what we mean is for the case of POA < 15 μg m^{-3} and Org/PM_1 < 0.5, and calculated the R^2. The sentences have been rewritten as following (line 742-747):

“Overall, the SOA and POA correlate weakly but when POA concentrations were less than ~15 μg m^{-3} and the OA/PM_1 mass fractions were less than 0.5 (data points with green/blue colors), they have relative tight correlation (R^2 = 0.2). When POA concentrations and Org/PM_1 fractions were large, POA and SOA show almost no

correlation, indicating the importance of POA in the severe aerosol pollutions in Lanzhou during winter."

(8) Line 764: The filter (PM2.5) and AMS (PM1) measure different sizes of aerosol. This needs to be addressed here. Is this agreement still good when this is taken into account?

We add a sentence to mention the different size cute for these two sampling methods. In order to address the reviewer's concern, we also explain the high ratio of OC_{AMS}/OC_{filter} due to the negative artifact of filter samples as following (line 772-777):

"The average ratio of OC_{AMS}/OC_{filter} was ~1.5 for these two filters although the smaller size cute for AMS than filter sampler (PM1 vs. PM2.5). The possible reasons were likely due to the analytical uncertainties of different instruments (30% for AMS and 20% for OCfilter), which was also observed in other studies (Zotter et al., 2014), and negative artifacts for the filter samples."

(9) Line 862-863: This number is actually quite variable (44% on 23 January and 68% on 15 January). Please revise to acknowledge this, particularly given the limited variability of the C-14 measurements.

Agree. We add the uncertainty for the average contribution of fossil and non-fossil carbon in SOC based on the standard deviation of f_{NF} as following (line 788-789):

"For all AMS data, the f_F and f_{NF} in POC were 50% and 50%, while for SOC were 34 ± 10% and 66 ± 10%."