Review of “Sources and Characteristics of Summertime Organic Aerosol in the Colorado Front Range: Perspective from Measurements and WRF-Chem Modeling” by Bahreini et al.

In this study, the authors combined measurements and model to evaluate the role of oil and natural gas (O&G) emissions on the concentration of various gas species and SOA production in the Colorado Front Range. Firstly, the authors implemented top-down estimated O&G emissions in WRF-Chem model and revealed that including O&G emissions results in better agreements in ethane, toluene, OH, O_3 between model and measurements. Secondly, based on different simulation scenarios, the authors showed that the best agreement between measured and modeled OA is achieved by (1) assuming primary OA is non-volatile and (2) limiting the extent of biogenic VOCs aging and subsequent biogenic SOA formation. Thirdly, based on model results, the authors showed that O&G emissions contribute to <5% of total OA in this region. Overall, these findings are important to assess the roles of O&G emissions on air quality. The manuscript is clearly written. I recommend publication after minor revisions to address the main comments below.

Major Comment

In section 3.2, the authors showed that the mean OA concentration was 40-48% higher in O&G influenced plumes compared to urban plumes, which is hypothesized to be due to SOA formation from higher concentrations of aromatics and larger alkanes from O&G emissions. However, in section 3.3.2, including the O&G emissions in the model only increases the OA concentration by ~0.4 ug/m^3 compared to that without O&G emissions. This enhancement amount is not sufficient to explain the observed difference between O&G plume and urban plume (i.e., roughly 1.5 ug/m^3 according to Figure 4A). Similarly, in Figure 10(a) and (b), none of the model scenarios can reproduce the observed OA enhancement in O&G plumes compared to urban plumes. Thus, the authors’ hypothesis is not well supported. Please comment on this.

Minor Comments

1. In the manuscript, there are many fractional values. For example, O&G sector contributed to <5% of OA; O&G sector contributes up to 38% of anthropogenic SOA; biogenic SOA accounts

for 40-54% of total SOA; etc. It is not straightforward for readers to get the whole picture about the OA sources in the studied region. I suggest to include a bar chart to summarize the contributions to total OA from different sources. For example, what is the contribution of anthropogenic SOA vs. biogenic SOA. In anthropogenic SOA, what is the contribution of O&G emissions vs other anthropogenic sources.

2. Page 7 Line 35-37. It is stated that the OA concentration at background CO level was 0.8 – 2.3 ug/m^3 based on the fitted lines in Figure 1. However, by eyeballing, the OA concentration is about 2 ug/m^3 for both lines at 100ppb CO. It is not clear how the reported range is obtained.

3. Page 11 Line 24-25. It has been well established that POA is semi-volatile. Are there any reasons, rather than that the non-volatile assumption leads to better model and measurement agreement, to support the non-volatile POA conditions for the studied environment? Also, as mentioned in the manuscript, there are many other reasons why modeled POA is lower than measurement. Without ruling out other possible reasons, it is bold to draw the conclusion that POA is non-volatile in the studied environment.

4. Page 12 Line 8-9. Please elaborate on why assuming POA non-volatile would reduce anthropogenic SOA, but increase biogenic SOA.

5. Figure 6. Are the data shown here from O&G plumes or urban plumes?