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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Sources and Characteristics of Summertime Organic Aerosol in the Colorado Front Range: Perspective from Measurements and WRF-Chem Modeling" *by* Roya Bahreini et al.

Anonymous Referee #2

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Several particle and trace gas measurements made from the C-130 over the Colorado Front Range are used to estimate contributions to OA (SOA and POA) from the Oil and Gas (O&G) sector, relative to urban and biogenic contributions, for that region. Further, the authors contrast the results from the observations with simulations of OA (SOA and POA) from the WRF-Chem model. This is a challenging study because it means getting the relative contributions to OA about right from three main sources (biogenic, O&G and urban) in both the measurements and the model. Further, the OA is split between POA and SOA, and the terrain and source distributions are complex,

resulting in the potential for many uncertainties. Still, the results of the comparisons are reasonable. With one major exception, I think the work is worthy of publication in ACP.

Major comment:

Slowik et al. (Figure 5, ACP, 2010, www.atmos-chem-phys.net/10/2825/2010/) showed a large slope for biogenic OA versus CO, at low NOx, that is very similar to many of the orange and yellow points representing the steepest slope of OA vs CO in your Figure 1. Also, their OOA-2 spectrum (possibly representative of semi-volatile OOA) is quite similar to your OOA spectrum (Fig. 3), which means that PMF may not identify a difference between O&G OA and biogenic OA. The assumption of low CO underestimates the background OA contribution, and a couple of statements in the conclusions ("with the best match obtained in runs when consecutive aging of bVOCs and bSOA formation was turned off"; "A large fraction (∼40-54%) of OA in the Front Range was predicted to be from bSOA") suggest the use of a constant "background" OA may be a substantial weakness in the observational analysis. The authors obviously recognize this weakness, as demonstrated by their statement on page 13: "It is worth noting that not considering variable background levels of OOA and CO and the uncertainties associated with PMF analysis might have also impacted the comparisons discussed here." I would like to see some improvement in the discussion of the biogenic contribution derived from the observations, including if or how well they can distinguish between biogenic OA and O&G OA. Further, it would be nice to see the authors improve their representation of the biogenic contribution to their observations in their analysis. Related, I would like to see an improved discussion of the synoptic situation, including temperatures and wind directions in the region. Towards the bottom of page 3, you mention that easterly wind cases were sampled up to 5500 m, but it's unclear what the general flow was for the cases below 2500 m. What are the main sources of BVOCs to the region: local vegetation, mountain valleys to the west, etc.?

Minor comments:

1) Page 3, line 6 – Is “Summer 2015,” an acronym or should it be summer, 2015?

2) Page 4, line 32 – A total. . .

3) Page 5, lines 4-6 – Did the regional BB show up as a separate factor in the PMF?

4) Page 9, line 23 – 4e rather than 4d, and 4f instead of 4e.

5) Page 9, line 32 – “. . .Emissions: Modeling”?

6) Page 9 and Figure 9 – I don’t see the advantage of the cumulative plot vs a frequency plot that is simpler to digest.

7) Page 9, lines 29-32 - Is the 4 km resolution over the sampled region for the specific time of year sufficient to compare with the airborne observations without contributing to differences in extremes?

8) Page 13 – The reduction of bSOA implies that many of the BVOC sources are mixed with the urban sources. Is transported bSOA unimportant?

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