

Interactive comment on “Determining the spatial and seasonal variability in OM/OC ratios across the US using multiple regression” by H. Simon et al.

Anonymous Referee #2

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The manuscript by Simon and coworkers presents the results from a method to estimate the organic mass to organic carbon ratio (OM/OC) using ambient measurements from the IMPROVE network. The authors modify a previously published method and present averages and trends of OM/OC across the US and for different seasons. OM/OC is an important parameter in aerosol characterization and measurement, and the paper is generally well-written and thorough. The modified method may also be useful to other researchers. I recommend publication in ACP after the following issues are addressed (including re-running the analysis if needed).

Major Issues

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1) Line 11 on page 24652 (abstract) and later in the manuscript: the values of OM/OC of 1.29 and 1.32 for the winter in the Western and Central states appear unrealistically low. Similarly for the value of 1.37 a couple of lines later. The lowest OM/OC is associated with combustion of fossil fuels, which are thought to be comprised of lubricating oil and unburned or partially burned fuel (Tobias et al., 2001), which have an OM/OC of about 1.25 (as discussed in page 24659 of the present paper). Wood burning for heating is thought to be an important OM source in these regions during the winter, and the OM/OC of that source is quite a bit higher (the authors quote 1.7 in page 24659, but Turpin and Lim (2001) report 1.9-2.1 in their Table 4). Although much smaller than in summer, some SOA formation from fossil fuel and wood combustion will still occur in winter, increasing the OM/OC a little. The authors should comment whether they think such low ratios of 1.29-1.32, which would seem to correspond to OM dominated by fossil fuel combustion without barely any impact of wood burning or other sources, are realistic for the IMPROVE regional sites, or whether there could be some alternative explanation for them, including a low bias on the method. See also comment 4 below about filter artifacts.

2) Line 13 on page 24657. It is well-known that HNO₃ reacts with NaCl in sea salt forming NaNO₃ and displacing the Cl into the gas-phase, and the reaction is quite rapid (Gard et al., 1998; Bardouki et al., 2003; Finlayson-Pitts, 2003). For this reason estimating the total sea salt mass from Cl is a very poor choice. Would it be possible to estimate it from Na instead? If yes, the analysis should be redone in that way. If not, this limitation needs to be highlighted in the manuscript.

3) Line 19 of page 24657. Non-soil potassium is likely to be due mostly to biomass burning sources, in which the potassium chemical form is typically KCl, which can react to form KNO₃ or K₂SO₄ (Gaudichet et al., 1995; Li et al., 2003) all of which have much higher species/K ratios. This needs to be accounted for in the revised manuscript.

4) Page 24678: the caveat given here about filter artifacts is very important and a key limitation of this work, and it should be added to the abstract and conclusions.

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Minor issues

- 1) Page 24653, a very relevant paper for this FTIR discussion is Polidori et al. (2008).
- 2) Line 12 of page 24659, another good reference for the OM/OC of laboratory SOA is Chhabra et al. (2009). Several papers have reported OM/OC of ambient SOA which should also be cited here (Aiken et al., 2008; Sun et al., 2009; Huang et al., 2010).
- 3) Line 16 of page 24672: could the higher values of b_{nit} be due to the presence of other forms of nitrate such as KNO₃ from the reaction of KCl in wood burning emissions?
- 4) Line 19 of page 24673: is the presence of acidic sulfate during quarter 1 consistent with the annual cycle of NH₃ emissions? Could some acidic sulfate be neutralized in the filters when plumes of NH₃-rich air are sampled later?

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