

The manuscript details the use of a non-reactive tagging method of tracking primary organic matter (OM) using the model COSMO-MUSCAT to investigate the contribution of residential heating to OM during the winter of 2021, at 3 measurement stations across central Europe. The findings indicate that the modelled OM is underestimated at these sites, which is mainly attributed to the current under-representation of wood combustion SOA. The study is well framed, and conclusions are adequately presented. I recommend the publication of this work, before the authors clarify and accommodate the following questions/recommendations.

L6:8: “Although the magnitude and temporal changes of the model results mostly agree with total OM values at two measuring stations, it appears to underestimate measurements at a site in the central Czech Republic.” It would be better to mention earlier in the introduction that there was 1 measurement site in Germany and 2 in CR to avoid confusion.

L18: “ wider societal costs associated with it”. interesting point but would benefit from stating the wider societal costs. The values can be presented here to underline the magnitude of the costs and losses.

L71: “The second main wind direction is East (about 17% of the time), with dry continental air masses influenced by long-distance transport from Poland, Belarus, Ukraine, Slovakia and the Czech Republic (Spindler et al., 2001, 2012, 2013).”. Is this during summer or winter?

L135: Add the values of splitting profiles in the appendix.

L163: wouldn't it benefit to use a 2-d VBS method over the 2 product Odum parameterization?

L196: add a table in the appendix detailing the main tagged species

L208-209: Since gas phase species and aerosol chemistry is not considered, can the authors quantify how much the SOA will be under-predicted based on the lack of these processes in the model.

L249: is there a comparison of simulated and measured boundary layer height?

L253-254: provide value for the “slight difference”.

Figure 4: simulated PM_{2.5} and AMS PM₁ are not directly comparable. for e.g if we consider Nitrate how can one interpret high model PM_{2.5} conc and low AMS PM₁ concentrations. Both the values could be in similar if we consider only modelled PM₁. Also, 4d is comparing PM₁, PM_{2.5} and PM₁₀. Are the PM₁, pm_{2.5} and PM₁₀ masses correlated? It would perhaps be better to compare, relative or normalized nitrate, sulphate, OM and EC concentrations if one must compare PM₁, PM_{2.5} and PM₁₀ conc.

L297: “The model underestimates the OM concentrations in Košetice (RMSE: 6.48 $\mu\text{g m}^{-3}$) while for Melpitz and Frýdlant the overall fit is good (RMSE: 1.17 and 2.01 $\mu\text{g m}^{-3}$).” doesn’t this negate the earlier claim that the OM is underestimated in the simulations?

L310-315: Since the winter values are heavily meteorological dependent one must mention if the weather patterns during the said years matched 2021. From the description above the year 2021 seems to be an odd one considering the long Sahara dust events and the cold periods. I would suggest making such comparisons to more tangible SOA concentrations, which can then present a broader picture of a trend in SOA underestimation.

L 345: Public power contribution. This is interesting. One would expect higher contributions from public power at Kosetice especially in the cold period as the air masses is stagnant. Can you explain why is the contribution of public power low during the cold period?

L 358-359: Melpitz cross border transport.

Is this during the cold or warm period? Looking at Figure 7, it appears that Kosetice and Frydlant have larger cross-border (Poland) contribution to public power than Melpitz. Also, it appears that at Melpitz the cross border contributions is more in the warm period but at kosetic the polish and german contributions are significant even during the cold period.

Figure 7: I would suggest removing the wind barbs since it doesn’t add any information’s. or did the authors miss the y axis with the degrees?

Table 3: why is alpha 1 same for S1 and S3? Shouldn’t it be same for S2 and S3? Some explanation is needed in the main body or the table caption.

L496-497: it would be interesting to see if the increase in OM holds true during the cold period of stagnant airmass at these sites.

L 537-538: I would suggest saying that the diurnal profiles are reproduced, not the total OM magnitude.