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Comment on acp-2021-516

Anonymous Referee #2

Referee comment on "Chemically speciated mass size distribution, particle density, shape and origin of non-refractory PM_1 measured at a rural background site in central Europe" by Petra Pokorná et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-516-RC2, 2021

The authors report data from a summer and a winter campaign at a monitoring site in Central Europe. The data are valuable and seem to meet the required quality standard. However, the presented analysis and interpretation is not sufficient and does not meet the standard of a publication in ACP. Many things are mentioned and connections are suggested without supporting arguments. There is no focused line that guides the reader from the measurements to the lessons that can be learned. What are the new insights and which results of the study confirm observations from previous studies? Unfortunately, I found no answers to these question, but reading this manuscript left me behind in confusion and I still wonder: What exactly can be learned from this study? Therefore I cannot recommend the publication. But I encourage the authors to improve their analysis and I hope that they find the comments below helpful.

Major comments:

The PMF analysis has been used to identify episodes of high number concentration. However it remains obscure how exactly this has been done. More explanation is needed (maybe in the appendix). Therefore, it remains unclear on which criteria the 10 summer and 13 winter episodes have been selected. In addition, these periods should be marked in Figure 2 (not in the appendix)!

The discussion of the episodes is not convincing and too strong conclusions are drawn from the trajectory calculations without providing additional evidence. In lines 314/315 the authors link high nitrate concentrations to marine air masses. What are the arguments here? Do the authors suggest a marine source of nitrate? I do not see evidence for such a claim. In lines 328/329 maritime influence is suggested again. Given that the air crossed half a continent at low altitudes before being sampled at the site, the claim of marine influence is not convincing.

The discussion in lines 327-334 is confusing. Too many different things are mentioned nothing is followed up and or backed by arguments. For example, what is the evidence for inversion conditions in Central Europe during episode 6a? Why would this lead to higher NO3- without increasing organics as well? Moreover, the mixing layer height in Fig A3 suggests stronger inversion during W6b. What do the aauthors concude from f60 and Figure A8 that is mentioned but no interpretation is provided.

Lines 335-337: Why does the oxidation state in winter point out the importance of local sources? Hydrocarbon aerosol can be transported as well and will remain hydrocarbon in the absence of significant photochemistry.

Lines 338-339: How were MOOAs and LV-OOA retrieved? There is no information on this in the manuscript.

Lines 347-353: This may be interesting. I understand that levoglucosan has been measured (although this is not -but should be - mentioned in the method section) about 10 times higher in the winter samples than during summer, but the f60 parameter barely indicates biomass burning influence. In my opinion this could be explored more. Apparently there is a discrepancy and something can be learned here!

lines 353-358: I do not understand this discussion and I do not agree that such conclusions can be drawn from Figure A9.

The discussion in lines 359-377 is not convincing. Are the differences observed between the clusters statistically significant at all? In most cases it seems that they are not.

Section 3.4 (lines 388-442) could use more focus and I wonder if all the evidence from other studies for larger particles in winter is needed. More space should be dedicated instead to interpret the findings of this study. The potentially interesting Figure 5 is discussed in less than 3 lines! Certainly much more can be inferred here.

In section 3.5 the authors focus on density retrievals during the defined episodes. The purpose of this is not clear. It is stated that a density of 1.85 g/cm3 corresponds to black carbon, but certainly nobody is claiming that these particles were predominantly black carbon. So what can be learned from this analysis? Moreover, there is no discussion of uncertainties of this analysis. is a value of 1.45 different than a value of 1.55? or is this still in the range of uncertainty?

Minor comments:

Abstract: abbreviations 'SE' and 'SW' not explained. Better to spell this out...

The abstract is a list of observations. But what are the lessons learned from the study? What can we conclude from the observations.

Showing the two almost identical charts in Figure 1 is not necessary in my opinion. In theory the volume based approach should be a little bit more advanced because it takes the particle composition into account to some extent, while the other approach uses a constant density for all particles and seasons.

Fig A5: add a legend. The reference "common color code" is not sufficient.

Fig A14: incomplete caption.

Fig A10: remaiuns unclear in the current form. More explanation is needed.