In this work, Chrit et al. used the air quality model Polyphemus to describe the organic aerosol formation and properties (notably oxidation state) at a measurement site in Corsica during the winter campaign of 2014. The OA concentrations are well simulated by the model, however, their oxidation state is systematically underestimated compared to observations. They also stress the importance of an accurate characterization of emissions since they found that the volatility distribution at emissions is the prime factor that control the simulated OA concentration levels. Overall, the manuscript is well written and scientifically sound. I recommend this study for publication after taking the following comments into account.

Specific comments:

1. Page 1, lines 15-16: What is the difference between oxidation and oxygenation state of OA? If there is no difference please remove the term "oxygenation state" from this sentence

2. Page 1, lines 16-17: Why do you assume that only the multigenerational ageing of the residential heating OA can improve substantially the results? Is this the dominant sector in the area? What about the multigenerational ageing of OA from other sources?

3. Page 2, line 7: Please replace OA with POA.

4. Page 2, line 3: You can also add the work of Jathar et al. (2014) and Tsimpidi et al. (2017).

5. Page 2, line 30: You can also add the work of van der Gon et al. (2015)

6. Page 4, line 4: What is the difference between highly oxidized and highly oxygenated OA?

7. Page 4, line 8: These studies are not recent. Please add more recent studies, e.g. (Aiken et al., 2008; Tost and Pringle, 2012; Canagaratna et al., 2015; Tsimpidi et al., 2018)

8. Page 5, section 2.1: OA formation from alkanes, olefins, S/I-VOC from open biomass burning, and marine OA is missing from the model setup. Can you please add a comment on their potential importance in the examined area?

9. Page 5, line 13: Please change „inorganics and inorgancs" with „inorganics and organics".

10. Page 5, line 14: Please add a reference for the algorithm.

11. Page 5, line 15: According to the presented results (e.g., Fig. 3), the simulation lasts until 2nd of April.

12. Page 5, line 16: It would be convenient if you can state the spatial resolution used here.

13. Page 5, lines 23-24: Please remove the sentence: "Other sea-salt…. not modelled"

14. Page 5 line 34: How much is this constant factor (R_{RH})? And how much is the constant factor R stated later in the text?

15. Page 6, section 2.2: PMF analysis results would be very useful for comparison with your model results. Are they available at the Ersa site? If so, please add this comparison.

16. Page 6, line 10: Are these coordinated the center of the model cell? Does the dimension of the model cell include the coordinates of the station mentioned above?

17. Page 6, line 10: Please define the abbreviation ACSM

18. Page 6, line 12: Did you compare the measurements with the model results with size sections from 0.056 to 1.0 (as they appear in the previous page)? Please clarify.

19. Page 6, line 14: Please correct "he" with "the"

20. Page 7, section 3: What about traditional VOCs? Are they subject to photochemical ageing?

21. Page 8, section 3.3: How do you treat OA from sources other than residential heating in this case? Do they follow the oxidation scheme described in section 3.2? In that case, can you justify why you use a different oxidation scheme especially for residential heating and not for other sources?

22. Page 8, line 11: The carbon number should decrease and oxygen number increases, please correct.

23. Page 8, Section 3.4:Can you provide the actual emission rates (e.g., in Tn yr^{-1}) of your OA precursor emissions (i.e., VOCs, NTVOCs, I/S – VOCs from different types of sources)?

24. Page 9, Tables 1 and 2: Please improve the quality of the tables. For example, you should include names of surrogate species that you assign these numbers, names of sensitivity tests, and what these numbers express (i.e., emission factors, O:C, OM/OC should not be stated only in the caption but also inside the tables).

25. Page 10, line 11: This is not very clear. You apply a factor of 4 in the initial emission inventory, and then, on top of that you apply a factor of 4.75 to account for the NTVOC (which are not part of your S/I-VOC). Can you please clarify and justify your hypothesis of such high additional emissions?

26. Page 10, line 17: Please add in the sentence the average OM concentrations over these cities. Likewise, provide average concentrations for other mentioned areas (e.g., Ersa) later throughout the text.

27. Page 10, line 19: Why do you focus only in these two simulations?

28. Page 10, line 19: Do you mean in both simulations (instead of "in all simulations")?

29. Page 11, Figure 2: Please increase the font size of the fractions. Also, the fractions over the dark blue are not clear.

30. Page 12, line 1: OA is already defined.

31. Is this an assumption or did you actually check that you have a false rain episode in your model?

32. Page 15, line 5: OA is already defined.

33. Page 15, Section 6: Can you comment on the importance of marine OA in your domain? Is this type of OA identified by measurements at Ersa site?

34. Appendix A, table A1: The definitions are not clearly readable.

Aiken, A. C., Decarlo, P. F., Kroll, J. H., Worsnop, D. R., Huffman, J. A., Docherty, K. S., Ulbrich, I. M., Mohr, C., Kimmel, J. R., Sueper, D., Sun, Y., Zhang, Q., Trimborn, A., Northway, M., Ziemann, P. J., Canagaratna, M. R., Onasch, T. B., Alfarra, M. R., Prevot, A. S. H., Dommen, J., Duplissy, J., Metzger, A., Baltensperger, U., and Jimenez, J. L.: O/C and OM/OC ratios of primary, secondary, and ambient organic aerosols with high-resolution time-of-flight aerosol mass spectrometry, Environmen. Sci. & Technol., 42, 4478-4485, 2008.

Canagaratna, M. R., Jimenez, J. L., Kroll, J. H., Chen, Q., Kessler, S. H., Massoli, P., Ruiz, L. H., Fortner, E., Williams, L. R., Wilson, K. R., Surratt, J. D., Donahue, N. M., Jayne, J. T., and Worsnop, D. R.: Elemental ratio measurements of organic compounds using aerosol mass spectrometry: characterization, improved calibration, and implications, Atmospheric Chemistry and Physics, 15, 253-272, 2015.

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Tsimpidi, A. P., Karydis, V. A., Pandis, S. N., and Lelieveld, J.: Global-scale combustion sources of organic aerosols: sensitivity to formation and removal mechanisms, Atmospheric Chemistry and Physics, 17, 7345-7364, 2017.

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