

In this paper, Ciarelli et al. combined a volatility basis set box model with smog chamber wood combustion aging experiments to constrain the parameters that control the description of the organic aerosol formation from wood burning emissions in models. This study has the potential to contribute in the organic aerosol modeling field but major revisions needs to be done before publication. In particular, I have two major concerns regarding the validity of the scientific methodology used and several other comments regarding the presentation of the study. Therefore, I would recommend publication only if these comments are addressed.

Major comments:

1. According to the aging scheme presented in section 3 and based on Table 1 and figure 2 the organic material receives 1-1.5 oxygen atoms (for SVOCs) or 2-4 oxygen atoms (for IVOCs) during the first oxidation step. Then, for every additional oxidation step, the organic material receives only around 0.5 oxygen atoms. However, according to Donahue et al. (Donahue et al., 2013)(2013) the oxygen number should be proportional to generation number and they have reported 1-2 oxygen atoms being added per generation of oxidation for SOA from biomass burning. The authors should provide experimental evidences to support their assumption that the number of oxygen atoms added during the first generation step is much higher than the number of oxygen atoms added after each additional oxidation (and explain why is as low as 0.5). Otherwise, they should reconsider their approach for the aging of SVOCs and IVOCs. Furthermore, the above information can be retrieved only from the tables and figures of the manuscript. The authors must extend the paragraph in the text where they describe their aging scheme to include all the necessary details.
2. The authors are using the emission factors and two different sets of enthalpies of vaporization (ΔH), proposed by May et al. (2013) for wood burning POA emissions, to simulate the OM_{sv} gas/aerosol partitioning in their experiments. However, May et al. (2013) proposed an entire set of parameters (emission factors, accommodation coefficient (α), and enthalpies of vaporization) that must be used together to describe the phase partitioning. If one of the parameters is changed (the ΔH here), then a new set of emission factors has to be used in order to describe the thermodynamic (TD) data within the experimental uncertainty. However, in this work the authors are using the same volatility distribution for both set of ΔH . According to Table S2 in May et al. (2013), for the ΔH used in SOL2 and $\alpha=1$ there are about 35 volatility distributions that describes the TD within the experimental uncertainty. Therefore, in order to use eq.4, the authors should contact May et al. to make sure that the volatility distribution used in SOL2 is acceptable otherwise they have to use a different one.

3. Section 3 needs to be re-organized and split in 3 sections as follow: i) section 3 will present the box model, ii) section 4 will present the set of parameters (ΔH , Y , k_{OH}) that gives the best fit and the methodology used to obtain it, and iii) section 5 will present the results based on the optimum set of parameters.
4. Section 4: This section should be renamed to section 6 if you follow my suggestion in the previous comment. The first paragraph here should be moved to the conclusions which are actually missing from the manuscript. Please add after this section the conclusions of your study.

Other comments

1. Abstract: The first sentence does not provide any information for the study presented here, therefore, it can be deleted.
2. Page 1 line 17: Please briefly explain why the VBS scheme presented here is called hybrid.
3. Page 2 lines 43-44: with the term “semi-volatile” you refer only to organic compounds or to inorganic as well? Please clarify. Similarly, with the term “fine particulate matter” do you imply only POA or total PM_{2.5}?
4. Page 3 equations 1,2: Please move the equations to section 3 where they are actually used.
5. Page 4 line 101: Please add the global models as well that have included the VBS scheme for SVOCs and IVOCs (e.g., Hodzic et al., 2016; Jathar et al., 2011; Shrivastava et al., 2015; Tsimpidi et al., 2014).
6. Page 4 line 118: Correct “Founoukis” to “Fountoukis”
7. Page 4 line 124: At this point it will be very useful to add several studies that have reported a possible underestimation of residential wood combustion emissions (e.g., move Bergstrom et al., 2012 here and also add Kostenidou et al., 2013) and difficulties in models to reproduce OA due to lack of information for BBOA emissions and aging (e.g., Fountoukis et al., 2016; Tsimpidi et al., 2016). Please add even more studies if possible. This will help you highlight the importance of the presented study.
8. Page 5 lines 129-132: This discussion is not needed here. A lot of VBS modeling studies in the past have reported the importance of the chemical aging. However, at this point, you should emphasize in the need to constrain the parameters that control the simulation of the OA formation specifically from the wood burning sources.

9. Page 5 lines 143-144: Remove the parenthesis from Heringa et al. and Bruns et al.
10. Page 7 line 193: Which are these several sets tested? Later in the manuscript you only report two (Eq. 3 and 4).
11. Page 7 line 198: Why the VBS approach presented here is called hybrid? Please explain.
12. Page 7, line 200: “Unlike previous 2D-VBS schemes”: in which schemes do you refer to? Please add references
13. Page 8, line 234: Please add reference for the group contribution approach and the Van Krevelen relation.
14. Page 10 line 285: change “and” with “to”.
15. Page 10 lines 305-307: Why the ranges used here are “physically realistic”? Please add references to support your assumption.
16. Page 10 line 308: Are the yields used here, the total aerosol yields (sum of the aerosol yields of the four volatility bins used)? If so, how you distribute them in the four volatility bins? Do you use a constant ratio? Please add this information here and a reference to support it.
17. Page 10 line 311: i, j , and k are not set. Use ΔH_{vapSOA} , Y , $k_{OH-NTVOCs}$ instead.
18. Page 11, line 324-325: Please provide the values of the set of parameters (ΔH_{vapSOA} , Y , $k_{OH-NTVOCs}$) that gives the absolute best fitting (the yellow diamond). This is the major contribution of this study since it can be used in large scale models and it should be reported clearly.
19. Page 11 line 336-337: Please be more specific by reporting the fraction of SOA that is formed from NTVOCs and SVOCs. Furthermore, I would change the “NTVOCs” to IVOCs in the entire manuscript in order to be more consistent to the terminology used widely in the field, but this is a personal preference.
20. Page 13 line 398: Please remove “our”. It is not the same group of authors.
21. Page 15 Table 1: Table 1 provides the same information with figures 1 and 2. You can add a line for NTVOCs and two columns for emission factors and ΔH in Table 1 and erase Figures 1 and 2. Otherwise just erase Table 1 and keep Figures 1 and 2.
22. Page 16 Table 2: Personally I prefer the use of “IVOCs” instead of “NTVOCs” in the entire manuscript. If you keep using the term “NTVOCs” please change the “measured IVOC” in the first column of the table with “measured NTVOCs”

23. Page 18 Figure 1: I would keep the Table 1 instead of the figures 1 and 2. If you decide to keep the figures do not show the emission factors to all of them. Use a different subfigure for the emission factors and use columns for all the other variables in the rest of the subfigures. Also change the “volatility distribution” to “emission factors” in the y-axis label of the figure. Finally, give a ΔH value for the specie with $C^*=10^{-1}$
24. Page 20 Figure 3: Where is the specie of the set2 with $C^*=10^3$ is coming from? This specie should not exist. Please delete it from table 1 as well.
25. Page 24 Figure 6: Does the underestimation of OA mass in the high temperature experiments indicate that you have overestimate the volatility of OM_{sv} ? According to my second major comment you have chosen a volatility distribution which is not proposed by May et al. (2013) for use with the ΔH used in this simulations.
26. Page 25 Figure 7: Why the measured O:C decreases at the beginning of the experiment No. 4?

References

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