

Authors' response to Anonymous Referee #1

on review of *"Modeling organic aerosol over Central Europe: uncertainties linked to different chemical mechanisms, parameterizations, and boundary conditions"*

by Lukáš Bartík et al. (ecusphere-2025-167)

Dear Anonymous Referee #1,

We sincerely thank you for the time and effort you dedicated to reviewing our manuscript, and for your constructive and insightful comments. Please find below our detailed, point-by-point responses (in black) to the comments you provided (in blue).

The manuscript "Modeling organic aerosol over Central Europe: uncertainties linked to different chemical mechanisms, parameterizations, and boundary conditions" by Bartík et al. combines CAMx model simulations with observations from sites located in Czech Republic. They investigate the sensitivity of organic aerosol concentrations simulated with CAMx on assumptions regarding IVOC and SVOC emissions and model boundary conditions. The topic fits within the scope of ACP. The model evaluation and sensitivity analysis presented in the manuscript are valuable work towards improving model representation of organic aerosols. However, the manuscript contains shortcomings in the description of methods and results and these should be addressed before the manuscript can be recommended for publication.

Major comments

1. The main aspects of the model related to the presented analysis should be included for the reader to be able to understand the work. I find that following aspects of the model should be described better:

- L121-126: Why were two chemistry mechanisms used? Could the authors please explain here some basics of what kind of chemistry these two mechanisms include and what are the main similarities/differences between them or otherwise explain the use of two mechanisms. For example, do the two mechanisms include essentially different precursors and/or reaction products?

We agree that the original version of the paragraph lacked sufficient detail to clarify the rationale and differences between the two gas-phase chemistry mechanisms. In the revised manuscript, we expanded the paragraph to explain the key chemical processes covered by both mechanisms. These include photolytic reactions and oxidation by hydroxyl radicals, nitrate radicals, and ozone, as well as the formation and reactions of hydroperoxyl and organic peroxy radicals. We also mentioned that the CB6r5 mechanism is a lumped-structure mechanism that groups volatile organic compounds (VOCs) based on their chemical structure and bond type, while explicitly treating selected compounds, such as isoprene, formaldehyde, and acetaldehyde. In contrast, SAPRC07TC applies lumping primarily based on VOC reactivity and includes a larger number of VOCs and their oxidation products in explicit form.

We also added a new paragraph to explain the reasons for using both mechanisms. These were selected to evaluate how differences in gas-phase chemistry formulations affect secondary organic aerosol (SOA) production, while keeping the SOA treatment unchanged. Additionally, we clarify that their use reflects practical constraints in the available configurations of the CAMx model, as only certain combinations of gas-phase mechanisms and OA modules are supported without requiring modifications to the model code.

- L127-134: Since aerosols are in focus in this study, please describe the basics of aerosol representation in the model, e.g.: What aerosol dynamics processes are included? Is condensation calculated based on equilibrium partitioning or some other way? It is said that ISORROPIA is used to predict composition and physical phase of inorganic aerosols. Are the organic and inorganic aerosols assumed externally mixed?

We agree that the basics of aerosol representation in the model should be provided and have accordingly revised the relevant paragraph in the manuscript. We now specify that we selected the coarse/fine (CF) aerosol scheme (Ramboll, 2020) to couple aerosol processes with gas-phase chemistry. This choice reflects the fact that it is the only scheme in CAMx that supports all combinations of gas-phase mechanisms and organic aerosol modules utilized in our experiments, which are described in Sect. 2.4 of the revised manuscript.

We clarify that the CF scheme divides the aerosol size distribution into two static, non-interacting modes (fine and coarse), within which aerosols are treated as internally mixed and monodisperse in size. Primary aerosol species can be represented in one or both modes, whereas all secondary aerosol species are modeled exclusively in the fine mode. We also distinguish the treatment of coarse- and fine-mode aerosols. Coarse-mode aerosol species are treated as non-volatile, chemically inert, and subject only to emission, transport, and removal by dry and wet deposition. In contrast, while all fine-mode aerosol species undergo the same physical processes, many of them can also participate in gas-particle partitioning, which is calculated based on the thermodynamic equilibrium assumption and applied separately for inorganic and organic aerosol species.

We also clarify that ISORROPIA version 1.7 is used to predict the composition and physical phase of inorganic aerosols, and that it models the sodium-ammonium-chloride-sulfate-nitrate-water system, including the mutual deliquescence behavior of multicomponent salt particles. We retained and clarified that one of two modules—SOAP (Secondary Organic Aerosol Processor) version 2.2 or 1.5-D VBS (1.5-dimensional Volatility Basis Set)—is used in CAMx version 7.10 to control organic gas-particle partitioning and oxidation chemistry. Lastly, we also retained and clarified that the CF scheme includes aqueous aerosol formation in resolved cloud water, calculated using a modified version of the RADM (Regional Acid Deposition Model) aqueous chemistry algorithm, which accounts for aqueous SOA formation from water-soluble precursors such as glyoxal, methylglyoxal, and glycolaldehyde.

We hope that these clarifications enhance understanding of aerosol treatment in our model simulations and address the referee's specific questions, which we summarize below along with our responses:

- What aerosol dynamics processes are included?

Aerosol dynamics depend on particle size. Coarse-mode aerosol species are treated as non-volatile, chemically inert, and subject only to emission, transport, and removal by dry and wet deposition. All fine-mode aerosol species undergo the same physical processes as coarse-mode species, and many of them can also participate in gas-particle partitioning, depending on their composition.

- Is condensation calculated based on equilibrium partitioning or some other way?

Yes, condensation—as part of gas-particle partitioning—is calculated based on the thermodynamic equilibrium assumption, using ISORROPIA for inorganics and either SOAP or 1.5-D VBS for organics.

- Are organic and inorganic aerosols assumed externally mixed?

No, organic and inorganic aerosol species are treated as internally mixed within each size mode in the CF scheme. However, gas-particle partitioning of fine-mode aerosol species is applied separately for inorganic and organic species.

- [L159-160: What are these concentrations of the chemical species based on?](#)

The default chemical boundary conditions consist of time-space invariant concentrations of ozone and its precursors, including several reactive nitrogen compounds and non-methane volatile organic compounds (NMVOCs). Their values reflect typical background concentrations over Europe and were derived from simulations performed by Huszar et

al. (2020) over a large European domain with a horizontal resolution of 27 km. This information has been added to the penultimate paragraph of Sect. 2.3 in the revised manuscript.

- Table S1: The SOAP and VBS schemes seem to consider monoterpenes and sesquiterpenes but these species do not seem to be included in the boundary conditions. If that is the case, why is it so? At least the overall picture of what is and what isn't included in the boundary conditions should be described in the main text.

Indeed, monoterpenes and sesquiterpenes are not included in the default chemical boundary conditions (CBCs), as these are also not available in the EAC4 dataset. For consistency, we omitted them from the default CBCs as well. On the other hand, the most important biogenic volatile organic compound, isoprene, is included in both datasets.

Regarding the overall picture, we agree that a brief summary and comparison of the species included in each CBC set should be provided in the main text. To address this, we explicitly stated in the penultimate paragraph of Sect. 2.3 in the revised manuscript that the default CBCs contain ozone and its precursors, including several reactive nitrogen compounds and NMVOCs, with the full list provided in Table S1 of the revised Supplement. In the final paragraph of the same section, we added that the EAC4 CBCs include some gas-phase species that are absent from the default CBCs and vice versa, with concrete examples. More importantly, we also explicitly stated that the EAC4 CBCs differ fundamentally from the default CBCs by incorporating aerosol species, namely sea salt, dust, sulfate, hydrophobic black carbon, hydrophilic black carbon, hydrophobic organic matter, and hydrophilic organic matter. The full list of gas-phase and aerosol species included in the EAC4 CBCs is provided in Table S2 of the revised Supplement. We hope that these additions offer a clearer and more complete understanding of the CBCs used in our simulations.

- L180-183: Authors should clarify here that the aging is included in CVb for POA and anthropogenic SOA. In appendix on L574 it is stated that "The gas-phase hydroxyl radical reaction rates for the chemical aging of POA and anthropogenic SOA, except those originating from biomass burning, are assumed to be 4×10^{-11} and 2×10^{-11} cm³ molecule⁻¹ s⁻¹, respectively. In contrast, the chemical aging of biogenic SOA and SOA originating from biomass burning (both anthropogenic and biogenic) is disabled." There authors should make it clear that this disabling of aging refers to the reference run, not for all runs with VBS scheme.

We agree that the distinction regarding which aging processes are included in the CVb and CVa experiments was not sufficiently clear. In response, we revised the main text to explicitly state that the CVb experiment uses the default configuration of the 1.5-D VBS scheme, which includes the chemical aging of POA and anthropogenic SOA (excluding biomass burning), while aging of biogenic and biomass-burning-derived SOA remains disabled. We also clarified that CVa builds upon this configuration by enabling the additional aging of biogenic and biomass-burning SOA using the same OH reaction rate as for anthropogenic SOA.

Additionally, in response to your third minor comment, we have relocated the content previously in Appendix A to a new subsection (Sect. 2.2) in the revised manuscript. In this updated section, we clarified that the disabling of aging for biogenic and biomass-burning SOA is part of the default configuration of the 1.5-D VBS scheme and that this setup was specifically applied in the CVb experiment. By contrast, the CVa experiment extends this default configuration by activating the additional aging pathways for these SOA types. We hope these clarifications will help the reader better understand the chemical aging configurations used in both experiments and their implications for OA formation.

- L185-190: Why were different emissions used for simulations with different SOA mechanisms? Also, were all POA emissions really replaced with POM_{SV} emissions? Does that mean that all POA was assumed to be semivolatile? Use of POM_{SV} in the model is one of the main focus points in the manuscript and Table 2 lists parameterizations used for these, but explanation of how these POM_{SV} are treated in the model is missing. Does all POM_{SV} have same volatility in the model?

Before directly addressing the first question regarding the use of different emissions across simulations, we believe it is important to first clarify the definition of POM_{SV} and several relevant aspects of the 1.5-D VBS scheme.

To address this, we have modified the definition of POM_{SV} in the introduction of the revised manuscript to clarify that it refers to primary organic matter that spans both the semi-volatile and lower-volatility parts of the volatility spectrum. We acknowledge that our previous formulation may have been imprecise in conveying what we intended to express.

We have also made changes to the description of the 1.5-D VBS scheme to better clarify several of its aspects. As mentioned in our previous response, this updated content (originally part of Appendix A) is now presented in Sect. 2.2 of the revised manuscript. In particular, we have not only explained that the basis sets comprise five volatility bins with saturation concentrations of $C^0 = \{10^{-1}, 10^0, 10^1, 10^2, 10^3\} \mu\text{g m}^{-3}$ at 298 K, but also highlighted that, although the properties of the surrogate species in the lowest volatility bin were estimated assuming $C^0 = 10^{-1} \mu\text{g m}^{-3}$, they in fact represent all OA of a given type with $C^0 \leq 10^{-1} \mu\text{g m}^{-3}$ and are treated as non-volatile. This means that whenever the gas-phase surrogate species in the lowest volatility bin is produced in any of the basis sets via chemical aging, it is assumed to immediately condense into its corresponding particle-phase surrogate species, which is treated as non-volatile and does not evaporate.

In this newly integrated section, we also provide a more precise explanation of how the 1.5-D VBS scheme treats POA and IVOC emissions, particularly in contrast to SOAP. The updated text more clearly describes that, unlike SOAP, which treats anthropogenic IVOCs using a single surrogate species, the 1.5-D VBS scheme uses four source-specific surrogate species for IVOC emissions, corresponding to gasoline vehicles, diesel vehicles, other anthropogenic sources, and biomass burning. Similarly, we clarified that POA emissions are not mapped to a single non-volatile species (as in SOAP), but are instead allocated to one of three basis sets representing freshly emitted OA, depending on the emission source. Within the assigned basis set, these POA emissions are further redistributed across all volatility bins using source-specific volatility distribution factors. The scheme distinguishes between POA emissions from gasoline vehicles, diesel vehicles, meat cooking, other anthropogenic sources, and biomass burning, applying a separate set of volatility distribution factors to each of these source categories.

We hope that these revisions to the manuscript provide a much clearer understanding of what we originally intended to express under the term POM_{SV} , as well as of the representation of the 1.5-D VBS scheme and its inputs. With this clarification in place, we now return to the original question regarding the use of different emissions across simulations.

To address this question, we have clarified this point in the revised manuscript. Specifically, we have expanded the explanation in Sect. 2.3 of the revised manuscript (formerly Sect. 2.2) to emphasize that the traditional POA emissions from inventories were retained only in experiments using the SOAP mechanism, where POA is treated as non-volatile. In these experiments, we assumed—consistent with the approach adopted in many previous studies cited in the Introduction—that traditional POA emissions do not account for missing SVOCs. However, as described in Sect. 2.2, POA emissions in the 1.5-D VBS scheme are redistributed across all the volatility bins within the appropriate basis set, based on their source, and should therefore include the missing SVOCs. Consequently, in the experiments employing the 1.5-D VBS scheme, we replaced the traditional POA emissions with those for POM_{SV} to ensure inclusion of the missing SVOC fraction.

In response to the second question — *“Also, were all POA emissions really replaced with POM_{SV} emissions?”* — we confirm that the answer is yes. In all experiments utilizing the 1.5-D VBS scheme, traditional POA emissions were fully replaced with the corresponding POM_{SV} emissions, using the parameterizations provided in Table 2. To clarify this implementation, we have expanded the final paragraph of Sect. 2.3 in the revised manuscript, where we now describe the specific parameterizations used for each source category, including gasoline vehicles, diesel vehicles, residential biomass burning, and other anthropogenic sources.

In response to the third question — “Does that mean that all POA was assumed to be semivolatile?” — we clarify that this is not the case. As explained earlier in this response and detailed in the newly inserted Sect. 2.2 of the revised manuscript, a portion of POA—and therefore also a portion of POM_{SV} in our case—is still treated as non-volatile. Specifically, emissions assigned to the lowest volatility bin are assumed to irreversibly partition to the particle phase, representing non-volatile material within the 1.5-D VBS scheme.

In response to the remainder of the comments regarding the treatment of POM_{SV} in the model and its volatility distribution, we have clarified these points in the revised manuscript. Specifically, at the end of the penultimate paragraph of Sect. 2.3, we have added a statement noting that, apart from accounting for the missing SVOCs, POM_{SV} emissions are otherwise treated identically to POA emissions within the 1.5-D VBS scheme. This clarifies that POM_{SV} is not treated as having a single volatility, but is handled within the existing volatility-resolved framework of the scheme. Furthermore, in the final paragraph of Sect. 2.3, we now provide details on the volatility distribution factors used to allocate POM_{SV} emissions from each source category to the volatility basis sets. These allocation factors are also listed in the newly added Table S3 in the revised Supplement.

- L560-563: What does "more-volatile" and "less-volatile" mean concretely in terms of volatilities?

The model represents the volatility of these substances using saturation concentrations (C^0). At a temperature of 300 K, the more-volatile and less-volatile products have the following C^0 values: (1) for anthropogenic precursors, $C^0 = 14$ and $0.31 \mu\text{g}/\text{m}^3$, respectively; and (2) for biogenic precursors, $C^0 = 26$ and $0.45 \mu\text{g}/\text{m}^3$, respectively (Ramboll, 2020). These saturation concentration values have now been explicitly included in the newly integrated Sect. 2.2 of the revised manuscript, which incorporates the content previously presented in Appendix A.

2. Description of the observational data used for the model evaluation would need more information:

- A map showing the locations of the observational sites would be helpful for a reader. This could be a separate map or the locations could be marked in e.g. in the Fig. 1.

We agree that adding a map showing the locations of the measuring stations would be helpful for readers. In response, we have added an additional panel (b) to Fig. 1 in Sect. 2.1 of the revised manuscript, showing the area of the Czech Republic together with the locations of all the stations used for validation.

- L237-238: According to the Table S6, the length of each of these measurement campaigns was only about one month. Please mention that in the main text.

We acknowledge that the approximate duration of the measurement campaigns should be stated in the main text. Accordingly, we revised the sentence in Sect. 2.5 of the revised manuscript (formerly Sect. 2.4 in the original manuscript) to note that each campaign phase lasted approximately one month. We also updated the reference to Table S7 in the revised Supplement (formerly Table S6 in the original Supplement), which provides the exact schedules.

- L250-252: Please mention how long time period was considered from these data.

We recognize that the time period covered by the observational data should be clearly stated. Accordingly, we have specified in Sect. 2.5 of the revised manuscript that these OC measurements used for validation were taken at the Košetice station from 1 January 2018, 02:00 UTC, to 31 December 2019, 02:00 UTC. We also noted that the daily OC concentrations were derived as 24-hour averages that follow the station's sampling schedule (i.e., from 02:00 UTC on a given day to 02:00 UTC on the following day). In addition, we revised the paragraph describing the observational data at the Prague–Suchbát station to clarify that sampling initially started at 09:00 UTC, but from 27 March 2018 onward, the start time was adjusted to 08:00 UTC. This information has now been explicitly included in the revised manuscript to improve transparency and accuracy.

In addition to the changes discussed in the three points above, we also revised the final two paragraphs of Sect. 2.5, “*Validation*” (formerly Sect. 2.4), to improve clarity and precision. In the penultimate paragraph of the original version, we incorrectly implied that meteorological variables were not measured at the Prague–Suchdol station. In the revised manuscript, we clarify that Prague–Suchdol is an air quality station where accompanying meteorological measurements are indeed conducted. However, the relevant meteorological data from this site were not included in the dataset provided to us, which was limited to professional meteorological stations. For this reason, we used data from the nearby Prague–Kbely station—a professional station—which we selected as a representative site for Prague–Suchdol.

In the final paragraph, we clarified the rationale for comparing daily OC concentrations—namely, to ensure at least consistency in the duration of sampling periods across all the stations, as the longest period among them was 24 hours (at the Prague–Suchdol station), even though the sampling windows differ between sites. We also specified that CAMx was configured to output hourly averaged concentrations, which allowed us to construct daily model outputs that matched the sampling periods at each station. Additionally, we made clear that for the meteorological evaluation, only the mean daily modeled and observed values from the days with available OC measurements were used, with each daily value constructed to follow the station-specific 24-hour sampling windows.

Importantly, we would like to point out that in the case of the analysis for the Prague–Suchdol and Košetice stations, we made a mistake by comparing the measured daily OC concentrations with the modeled daily concentrations derived uniformly from 00:00 UTC of the relevant day. We apologize for this oversight. We have recalculated the validation results for these two stations using the correct time windows and included the updated values in the relevant tables in the revised Supplement (Tables S10 and S12). We also revised the figures displaying the mean daily OC concentrations (Figs. S4 and S6 in the revised Supplement) and the differences between modeled and observed values at these two stations (Figs. 6 and 10 in the revised manuscript) to reflect these corrections. We made a similar mistake in the analysis of meteorological variables at these two stations. This has now been corrected to follow the procedure described in the revised manuscript, and the corresponding table (Table S8) and figures (Figs. 2 and S1) have been updated accordingly. Based on these updated analyses, we have made several corresponding revisions to the text describing the results to ensure consistency with the corrected values.

3. Some clarifications or explanations would be needed in the results section:

- L367-368: Could you please explain why you have chosen different emission estimates?

We used different IVOC emission estimates because our approach is based on more recent smog chamber experiments specific to biomass burning sources (Jiang et al., 2021; Ciarelli et al., 2017), which suggest higher IVOC/POA ratios than the generic parameterization by Robinson et al. (2007). This clarification has been added to the revised manuscript.

- L430: What does the “similar conclusion” refer to here? Does it refer to the conclusion in the previous sentence about wind speed inaccuracy in the model being possibly the explanation for the underestimated OC? Why would that affect CV_b and CV_a most?

In the original version, the phrase “*a similar conclusion*” was intended to refer to the fact that a similar pattern is observed — namely, that the Košetice station shows better prediction accuracy than the stations in the Kladensko and Třinecko areas during the summer seasons — and not to the explanation involving wind speed differences. We have clarified this in the revised manuscript by rephrasing the sentence as follows:

“During the summer seasons, a similar pattern is observed in that the Košetice station (NMSE = 49.2–291.0 %, IOA = 0.36–0.52, FAC2 = 0–56.7 %) shows better prediction accuracy than the stations in the Kladensko and Třinecko areas (NMSE = 114.2–653.8 %, IOA = 0.36–0.49, FAC2 = 0–31 %; Table S11), which perform even more poorly than during the winter phases.”

- Are the concentrations in the map figures (e.g. Fig. 4) surface level concentrations or, e.g., averaged through the vertical layers of the model?

These are the near-surface concentrations, or more precisely, concentrations in the first model layer, which spanned approximately 50 m in vertical extent. We have clarified this in the revised manuscript by stating it explicitly at the beginning of Sects. 3.2 and 3.3.

- L493-495: “The observed impacts in these simulations are likely linked to changes in other pollutant(s) at the boundaries of the model domain, which influence SOA chemistry.” Could the authors please explain what these other pollutants are, how/why they changed and how that would affect the organic aerosol in the model?

We agree that further clarification was needed and have revised the text accordingly. In the updated manuscript, we now explicitly state that the impacts on SOA concentrations can be directly attributable to changes in the chemical composition of the chemical boundary conditions (CBCs), which were the only factor varied in the relevant simulations (i.e., Sp100s0 vs. CSwl and Vp100s0 vs. CVb). These changes may affect both the oxidative environment — through species such as ozone, nitrogen oxides, carbon monoxide and the hydroxyl radical — and the availability of direct SOA precursors, including toluene, xylene, and isoprene. This clarification has been added in the results section and supported by references to Sect. 2.3 in the revised manuscript and Tables S1–S2 in the revised Supplement, where the full CBC composition is documented.

- L495-496: “The spatial distributions of the mean seasonal impacts on SOA concentrations in Sp50s50 and Sp0s100 (and similarly in Vp50s50 and Vp0s100) exhibit structures akin to those observed for the mean seasonal impacts on POA concentrations in Sp50s50 and Sp100s0 (and likewise in Vp50s50 and Vp50s000) (Figs. 8c and d) during both seasons.” Could the authors please comment if this is an expected result? Or does this point towards the boundary conditions defining too much the concentrations over the simulated area?

The observed resemblance can be partly understood by considering the volatility characteristics of the OA surrogate species at the model boundaries, as discussed in the revised manuscript. In both seasons, POA and SOA are predominantly redistributed into non-volatile and low-volatility surrogate species (Tables S4–S6 in the revised Supplement), which reside largely in the aerosol phase and are therefore efficiently transported. Given that all simulations are driven by the same meteorological conditions, this leads to similar transport behavior and helps explain the observed similarity in spatial patterns. This explanation has been included in the revised manuscript.

- L522-529: Why does the improvement with adding the OA at boundaries differ between the stations and seasons? Also, is it reasonable to assume that the OA at boundaries is only or mostly POA, i.e. do the authors expect that adding the OA as POA at boundaries is getting model results closer to the measured values because it is making the model representation of organic aerosols more accurate, or is the agreement better just because there is a large underestimation in the reference simulation and adding the OA at boundaries as POA happens to increase OA concentration most?

A new paragraph — now the penultimate paragraph in Sect. 3.3.2 of the revised manuscript — was added to address the first part of the comment. It explains that the improvement or deterioration in the modeled mean daily OC concentrations resulting from the addition of OA at the boundaries of the model domain differs between the stations and seasons analyzed due to the combined influence of several interacting factors that vary both spatially and temporally. These include (1) the annual variation in the mean monthly concentrations of the total OA prescribed at the boundaries of the model domain, (2) the seasonal variation in how these concentrations are redistributed into the POA and SOA surrogate species, (3) changes in atmospheric conditions that affect the transport and chemistry of OA (e.g., wind patterns and temperature), and (4) spatial and temporal variability in anthropogenic and biogenic emissions inside the model domain.

The final paragraph of Sect. 3.3.2 addresses the second part of the comment. It clarifies that the redistribution scenarios treating total OA at the boundaries of the model domain as entirely POA or entirely SOA were not intended to represent realistic conditions, but were

designed as bounding cases to assess the sensitivity of the model to the unknown OA composition in the EAC4 dataset by exploring the maximum plausible range of impacts on the modeled mean daily OC concentrations. Although Sp100s0 and Vp100s0 produced the mean daily OC concentrations that most closely matched the observations in this sensitivity study, they assumed OA to be entirely composed of POA at the boundaries of the model domain, which is highly unrealistic. As already noted in the Introduction, Chen et al. (2022) found that SOA dominates the organic aerosol fraction of PM_{10} across Europe (ranging from 47.3 % to 100 %), indicating that a significant SOA component may be expected in real boundary conditions. While their results pertain to PM_{10} , they suggest that the improvements in the modeled mean daily OC concentrations obtained in Sp50s50 and Vp50s50, or in simulations falling between Sp50s50 and Sp0s100 and between Vp50s50 and Vp0s100, may more realistically reflect the influence of OA composition at the boundaries of the model domain.

4. This study includes sensitivity analysis on estimates of IVOC and SVOC emissions and OA boundary conditions, as well as comparisons using two different SOA schemes and chemistry schemes. Is it possible to conclude which of the analyzed factors/assumptions, or their uncertainties, are most important from the point of view of modelling OA in Central Europe with this model?

The revised conclusions now address the relative importance of the analyzed factors. They clarify that the setup used in the CVa experiment—featuring the 1.5-D VBS scheme with aging from all OA sources and the inclusion of both source-specific and non-source-specific IVOC and SVOC emissions—resulted in the best overall model performance, particularly during winter. They also explain that the inclusion and composition of OA in the boundary conditions had the greatest impact during summer, especially at rural sites. While the two sensitivity analyses targeted different aspects and cannot be directly compared, the conclusions now highlight how the dominant source of model sensitivity varies seasonally, providing clearer guidance on which assumptions are most influential for OA modeling in Central Europe.

Minor comments

Please mention in the abstract that the evaluation of the model simulations is focused on Czech Republic. Currently the reader finds out quite late in the text that the evaluation is not for wider Central Europe but only for one country.

We have added this information to the abstract.

L21: “have an undoubted environmental footprint” Please check the choice of word. In my understanding environmental footprint term is used for the impact of e.g. organization or products on environment, so for the source of aerosols one could talk about environmental footprint, but not for aerosols themselves. I did not find the term “environmental footprint” from the reference given for this statement, therefore it is not clear what the authors mean by this term.

We agree that “*environmental footprint*” is not the appropriate term in this context. To better reflect our intended meaning—that aerosols represent a burden to the environment—we have revised the wording to “*environmental burden*.”

L136-137: I would suggest moving this essential information from the Appendix to the main text.

In the revised manuscript, we have followed this suggestion and moved the content previously presented in Appendix A into the main text. This material now appears as a dedicated subsection (Sect. 2.2), ensuring that the description and comparison of the SOAP and 1.5-D VBS schemes are more prominently integrated into the Methods section. We believe this change improves both clarity and accessibility for the reader.

Some of the figures, e.g. Figure 2, are missing y-axis labels. I recommend adding y-axis labels.

Y-axis labels have now been added to all relevant figures (Figs. 2, 3, 6, 7, 10, 11 in the revised manuscript, and Figs. S1–S7 in the revised Supplement). Additionally, we have added descriptive titles to these figures to clarify the meaning of the symbols used in the y-axis labels. We hope these changes enhance the clarity and readability of the figures.

Tables S3, S4 and S5 contain acronyms for surrogate SOA/POA species in the model. Please add explanation of what these species are.

We have added footnotes to the relevant tables (Tables S4–S6 in the revised Supplement), which provide explanations of the surrogate SOA/POA species used in the model.

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