We thank the editor and reviewers for their effort and the reviewers for their feedback and insightful comments. A detailed answer to each individual comment can be found below.

We would like to start with a response to a key point brought up in multiple comments regarding the applied framework to estimate the importance of bimolecular reactions of RO_2 with NO, HO_2, and RO_2. The reviewers have pointed out the large uncertainties in the presented approach due to the utilization of box-modeled concentrations of the reaction partners and the generic rate coefficients. We fully agree that these are key concerns and a known limitation.

Regarding the uncertainty in the modeled reaction partner concentration, we included a reference in L. 296 to Baker et al. (2024), who showed that in the HO_2/RO_2 system the model correctly reproduces the HO_2 trend in comparison to measurements by bromide CIMS. Furthermore, as asked for by reviewers #1, #3 and #4, we performed a sensitivity analysis based on a variation of the pseudo-first order rate coefficients of RO_2, HO_2, and NO by ±30 %. This value is based on previous publications comparing box model results utilizing MCM chemistry to measurements in monoterpene photooxidation experiments (Rolletter et al., 2019; Kaminski et al., 2017). The results of the sensitivity analysis show that a change of the pseudo-first reaction order of this magnitude would, while it changes the specific reaction partner contributions and total bimolecular reaction rate, not impact the classification of the chemical regime or the ordering of the regimes. The results of the sensitivity analysis are described and discussed in Supplement **Sec. S9** and introduced in the main text in L. 301.

In general, we want to underline that we see the development of the framework and shown methodology as an important result to provide a tool for analysis. This is of more meaning than the derived values for rate coefficients in detail. When better inputs become available, they can be "plugged in" to get better constraint results. This is now better clarified in the main text (L.315):

"In general, the framework is intended as an analysis tool, so the reported bimolecular reaction rates and contributions by the reaction partners should not be seen as final quantitative results but rather as trend indicators in the process of better understanding the observed changes in HOM and SOA formation."

The framework uses bulk reaction rate coefficients as the goal is to plausibly describe the bulk chemistry and not the reactions of a singular RO_2. For the application of the framework to bulk trends, we utilized the generic information available and believe the applied rate coefficients are state-of-the-art. We have added an explanation in L.346 to clarify the purpose of the framework:

"The purpose of the chemical regime categorization is to plausibly describe the bulk chemistry to understand shifts in the HOM product distribution. In contrast, individual RO_2· may have very different reaction rate coefficients compared to the utilized generic values, so that the reaction pathways of a specific RO_2· may not be correctly captured."

In the absence of further constraints, it is difficult to provide a systematic sensitivity analysis of the applied reaction rates, as for a singular RO_2 basically any combination of slower or faster rate coefficients is imaginable (e.g. a much faster NO reaction and a much slower HO_2 reaction would of course significantly change the contributions to the bimolecular reaction rate). However, we want to highlight that Section 4.1 *"Termination product contributions as proof of utility for the chemical regime framework"* specifically shows that the trends described by the generic rate coefficients and modeled concentrations capture the trends observed in the measured HOM product distribution and

that using the generic/bulk reaction rate coefficients therefor seems to capture the bulk trends in the product distribution, making the approach a valuable analysis tool.

As mentioned above, depending on use case and availability the same method could be applied with more specific rate coefficients. Here, we see great prospects in combination with automated mechanism generation models like GECKO (Aumont et al., 2026).

Please find the detailed reviewer comments with our answers and applied changes below (reviewer comments in blue, answers in black).

Reviewer #2:

This manuscript presents a well-designed experimental study investigating how the bimolecular reactions of RO_2 radicals affect HOM formation and subsequent SOA formation under different reaction regimes. The authors systematically vary the overall bimolecular RO_2 reaction rate and the relative contributions of $RO_2 + HO_2$, $RO_2 + NO$, and $RO_2 + RO_2$ reactions while keeping the α-pinene OH turnover constant. The combination of HOM product characterization, targeted seeding experiments, and mechanistic interpretation provides an interesting framework for connecting laboratory observations of HOM chemistry with atmospheric SOA formation.

I am primarily an aerosol guy with a background in bulk aerosol chemistry rather than a specialist in detailed RO_2/HOM chemistry. Therefore, I may not be in the best position to critically assess all of the mechanistic interpretations and reaction pathways discussed in the manuscript. From an aerosol perspective, however, I find the experimental design and the attempt to link changes in HOM composition and volatility to SOA formation particularly interesting. The results appear to provide useful insights into how reaction conditions commonly used in laboratory studies may influence the inferred SOA formation potential.

I have only a few comments, mainly concerning the description and characterization of the experimental measurements and some details of presentation. Addressing these points would improve the reproducibility and clarity of the study.

In Sect. 3.2, more information on the calibration procedures and measurement characterization of the PTR-TOF-MS and CIMS would be helpful. In particular, the authors might describe the calibration methods, calibration standards or compounds used, and, where relevant, the sensitivity/response factors and detection limits.

- Thank you for the comment! A note on the PTR-MS calibration was added in L.251:
  "The PTR-MS was regularly calibrated with a gas-mixture containing α-pinene yielding linear calibration curves with good linear fit."
  Furthermore, information on the calibration and transmission of the CIMS was added starting from L. 265:
  "To get insight into the instrument's sensitivity, a calibration with sulfuric acid was performed, which can be found in the Supplement Sec. S1 of Baker et al. (2024). A stable transmission was assumed in the mass-to-charge region of interest based on previous measurements applying the technique described in Heinritzi et al. (2016)."

L54: "formation pathways a common concern" should be revised to "formation pathways, a common concern." Similar missing commas should be checked throughout the manuscript. For example, in

L280, “As explained in Sec. 3.1 two different…” should be revised to “As explained in Sec. 3.1, two different…”.

- Thank you for pointing this out, the mentioned instances were fixed and we rechecked the manuscript to add missing commas.

L242: Please provide the size range of the SMPS and the flow rates used for the relevant instruments. These details are important for assessing the aerosol characterization and would improve the reproducibility of the experiments.

- Thanks for asking for this additional information, we added a section in the Supplement (Sec. S4) containing the information you asked for and rephrased the description in the main manuscript starting from L. 258 to refer to this section.

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