Authors response to the referees

We thank all three reviewers for their thorough and constructive evaluations of our manuscript. The comments raised important issues regarding the clarity of the scientific framing, the robustness of methodological descriptions, the interpretation of modelling results, and the overall structure of the manuscript.

We note that the reviewers expressed somewhat different expectations regarding the scope and positioning of the study. In particular, while some comments requested a stronger emphasis on conceptual advancement, Referee 3 explicitly recommended publication as a Measurement Report.

After carefully considering all comments, we agree that the core strength of this work lies in the coordinated, multi-site observational dataset collected during a well-defined European heatwave. We have therefore revised the manuscript to better reflect this emphasis and resubmit the revised version as a Measurement Report. In this revised format, the modelling analyses are retained as interpretative support, while the primary focus is placed on the harmonised measurements, spatial variability, and observationally constrained assessment.

Below, we respond to each referees' comments (in blue, red, green) in detail and describe the corresponding revisions made in the manuscript.

Referee #1 (https://egusphere.copernicus.org/#RC1)

This manuscript presents a comprehensive multi-site analysis of VOCs, ozone, and carbonaceous aerosols during the July 2022 European heatwave, combining intensive measurements across a wide spatial network. The dataset is extensive and valuable, particularly given the coordinated speciation of VOCs and the simultaneous examination of ozone formation, SOA production, and particle number concentrations across diverse European environments. The study aims to assess the role of biogenic and anthropogenic precursors in shaping ozone and secondary aerosol formation under heatwave conditions. Such harmonized observations and analysis during an extreme heatwave event are of clear scientific interest and fall within the scope of ACP.

However, the manuscript does not yet clearly demonstrate a substantial and well-articulated scientific advance beyond the presentation of a large dataset. While numerous analyses are performed, the central conceptual contribution and the specific insights emerging from the VOC, ozone, and aerosol investigations remain insufficiently defined. To meet the level of conceptual advancement expected for ACP publication, the manuscript would need to more clearly demonstrate how the analyses lead to robust and quantitatively supported new insight, rather than primarily descriptive characterization.

We agree that the original manuscript, submitted as a Research Article, did not sufficiently support quantitatively new insight. After considering this and other reviewers feedback, we have revised the manuscript and resubmitted it as a Measurement Report. In this format, the primary contribution is the coordinated, multi-site documentation of VOC speciation, ozone, carbonaceous aerosol, and particle number distributions during a well-defined European heatwave, supported by interpretative modelling analyses. We changed some of the wordings to be less conclusive. i.e.:

In the abstract

- *Original: Although the roles of biogenic and anthropogenic precursors in ozone, secondary organic aerosol (SOA) formation and nanoparticle growth are recognized, their relative importance during extreme events remains poorly constrained. This work therefore aimed to quantify these interactions using coordinated measurements and model simulations. Measurements showed that oxygenated VOCs formed the largest fraction of total VOCs, followed by non-methane hydrocarbons (NMHCs) and aromatics, with substantial contributions from both anthropogenic and biogenic sources. Elevated....*

Changed to: Coordinated measurements and chemistry transport modelling were used to examine the spatial variability of ozone, VOC composition, and secondary organic aerosol (SOA) formation under extreme meteorological conditions. Oxygenated VOCs constituted the largest fraction of total measured VOC mixing ratios, followed by non-methane hydrocarbons (NMHCs) and aromatics, with substantial contributions from both anthropogenic and biogenic sources. Sensitivity simulations indicate that elevated..

- *Original: Overall, further reductions in NOx emissions, alongside targeted control of key anthropogenic VOCs, would benefit air quality, during extreme pollution episodes. These findings illustrate the complex interplay between meteorology, emissions, and atmospheric chemistry during heatwaves and emphasize the need for comprehensive precursor monitoring and improved modelling for effective air-quality management under future climate conditions.*

  Changed to: Overall, further reductions in NOx emissions, alongside targeted control of key anthropogenic VOCs, would benefit air quality under future climate extremes

In conclusion:

- *Original: During the campaign, the highest ozone peaks were driven by a combination of elevated BVOC and NOx emissions, and ozone formation was similarly sensitive to both NOx and BVOC at most sites.*

  Changed to: During the campaign, the highest ozone peaks were associated with a combination of elevated BVOC and NOx emissions, and sensitivity simulations indicate that ozone formation responded to both NOx and BVOC at most sites.

- *Original: Further reductions in NOx emissions would therefore effectively lower ozone levels during extreme pollution events.*

  Changed to: These results suggest that further reductions in NOx emissions remain effective in lowering ozone levels during extreme pollution events.

- *Original: Aromatic VOCs were the dominant SOA precursors, followed by monoterpenes.*

  Changed to: Aromatic VOCs and monoterpenes contributed substantially to SOA formation.

  We also integrated some text on the NPF on the conclusions as outlined in point 34 below

General comments:

1. The classification of VOC groups (O-VOCs, isoprene, monoterpenes, NMHCs, and aromatics) is potentially confusing and inconsistent throughout the manuscript. Aromatic hydrocarbons are formally a subset of NMHCs, yet they are treated separately, and in Figure 3 isoprene appears to be grouped together with C2–C6 NMHCs (also, Line 394 “isoprene contributing up to 75 % of NMHCs.”). It should be clearly specified whether “NMHCs” in this manuscript refer only to aliphatic hydrocarbons, and the categorization should be defined explicitly and applied consistently throughout the text and figures.

   Yes, we agree that the classification can be confusing. We have included better specifications in the figures and text and predefined the classification in chapter 2.1:

   > *For clarity, VOCs were grouped into the following categories, which are used consistently throughout this manuscript:*

- *Short-chain aliphatic hydrocarbons (C2–C6), excluding isoprene*
- *Longer-chain aliphatic hydrocarbons (C7–C12)*
- *Aromatics*
- *Isoprene: formally an aliphatic hydrocarbon but treated separately due to its dominant biogenic origin*
- *Oxygenated VOCs (O-VOCs) except methanol*
- *Terpenes. Primarily divided into monoterpenes and sesquiterpenes*

> *The specific compounds included in each category are listed in Table S2. Hereafter, the term "NMHCs" refers to aliphatic hydrocarbons excluding isoprene, unless otherwise specified.*

We agree that the original wording in line 394 was misleading, as it implicitly included isoprene within the total NMHC fraction despite being treated as a separate category in our classification scheme. We have therefore revised the paragraph (Lines 392–397) to ensure that C_2-C_6 aliphatic hydrocarbons and isoprene are clearly separated.

> *Original text: "Ethane was the most dominant C_2-C_6 NMHC on average, comprising 32 ± 12 % of the total NMHC concentration, followed by isoprene at 20 ± 19 %. Considering their respective lifetimes (2 months vs. 1 hour), this suggests large isoprene emission fluxes during the IMP. There were significant variations between sites, with isoprene contributing up to 75 % of NMHCs at Peyrusse Vieille (FR0013R) and 59 % at Ispra (IT0004R).*
>
> *Changed to: Ethane was the dominant C_2-C_6 NMHC on average, comprising 41 ± 13% of the summed C_2-C_6 NMHC mixing ratio, followed by ethene (15 ± 9%). On average, ethane concentrations were comparable to isoprene concentrations (1000 ± 260 pptv and 954 ± 1455 pptv, respectively), although site-to-site variability was substantial. Considering their respective atmospheric lifetimes (~2 months for ethane versus ~1 hour for isoprene), these comparable mixing ratios suggest large isoprene emission around some sites during the IMP. The highest isoprene concentrations were observed at Peyrusse Vieille (FR0013R) and Ispra (IT0004R).*

2. The units used for reporting species concentrations are not fully consistent. Expressions such as ppb, ppt, pptv, pmol/mol, ng/m^3, and µg/m^3 are used interchangeably. For clarity and readability, the author should adopt a more consistent unit where possible. In addition, the ACP submission guideline (https://www.atmospheric-chemistry-and-physics.net/submission.html) requests 'Units must be written exponentially (e.g. W m^{-2}).'

We have carefully reviewed the manuscript and standardized the unit notation throughout, ensuring consistency in accordance with ACP guidelines. Gas-phase species are reported as mixing ratios (pptv for VOCs and ppbv for ozone), while aerosol components are reported as mass concentrations (µg m^{-3} for PM and chemical composition, µg C m^{-3} for OC, and ng m^{-3} for organic tracers).

3. The manuscript includes a very large number of supplementary figures, many of which appear to contain information that is central to the interpretation of the results. However, these figures are often only briefly mentioned in the main text, with limited discussion. This makes it difficult for the reader to fully assess the robustness and internal consistency of the analysis. The authors may consider either strengthening the discussion of key supplementary results in the main manuscript or streamlining the scope of the study. In particular, given the breadth of topics covered, it may be worth considering separating the ozone-focused and aerosol-focused analyses into two more targeted manuscripts, which would allow a clearer narrative and a more in-depth treatment of each set of results.

We agree that the large number of supplementary figures may have made it difficult to fully follow some parts of the interpretation in the original version. Our intention was to keep the main manuscript focused on the central findings, while providing detailed supporting analyses in the Supplement for readers interested in deeper exploration of the dataset and model diagnostics. In response to this comment, we have revised the manuscript and moved former Figure S11 (ozone formation potential and SOA potential analysis) to the main manuscript, as it is directly referenced in the discussion of precursor contributions.

Regarding the suggestion to separate the ozone-focused and aerosol-focused analyses into two manuscripts, we consider that the strength of this work lies in the integrated assessment during a single, coordinated heatwave campaign. In the revised version, now submitted as a Measurement Report, we believe it is appropriate to present the full set of harmonised observations from this comprehensive campaign within a unified framework.

4. Throughout the manuscript, several mechanistic interpretations and source attributions are proposed to explain observed or modelled patterns (e.g., causes of model bias, effects of VOC and NOx on OH and H2SO4 production, drivers of SOA underestimation, and interactions between BVOCs and particle growth). While these explanations are chemically or geographically plausible, they are often presented without direct quantitative support from model diagnostics, sensitivity analyses or references. Given the complexity and regime dependence of atmospheric chemistry during heatwaves, it would strengthen the manuscript to either provide explicit supporting evidence or to moderate the language to clearly distinguish between findings and speculative interpretations.

In the revised manuscript, we have carefully reviewed the interpretative statements and moderated the language. The descriptive used is more in line with the scope of a Measurement Report manuscript as indicated in the revised Abstract and Conclusions mentioned above.

5. As currently structured, the manuscript attempts to cover both detailed ozone chemistry and aerosol formation processes within a single study. While the dataset is highly valuable, the scientific narrative may benefit from a clearer focus and prioritization of the core scientific questions. Alternatively, the authors may wish to reflect on whether the manuscript would benefit from a more explicitly measurement-oriented structure, similar in spirit to a measurement report. Ultimately, the most appropriate format and scope of the manuscript is of course a decision for the authors and the editor, but clarifying the primary focus would likely strengthen the overall coherence and impact of the study.

Yes, we agree and we have revised the manuscript to be a Measurement Report.

6. Sections 3.3 and 3.4 contain extensive analyses of ozone production, SOA formation, and source apportionment. However, the connection to the central heatwave focus of the manuscript is not always clearly described. Much of the discussion reads as a characterization of general spatial patterns across Europe, rather than explicitly highlighting what is specific to the 2022 heatwave conditions. It would strengthen the manuscript to more clearly distinguish between typical regional features and heatwave-driven anomalies, and to explicitly state how elevated temperatures and associated chemical conditions modified oxidation pathways, SOA formation, or source contributions. In addition, a more explicit comparison between observation-based apportionment (Section 3.4.2) and model-derived OA budgets (Section 3.4.3), along with a discussion of the uncertainties associated with the empirical assumptions used, would improve the overall coherence and robustness of interpretation.

We agree that the heatwave linkage could be clearer. The manuscript already includes July 2022 anomalies relative to climatological averages where available. Isolating temperature effects would require additional mechanistic modelling beyond the scope of this Measurement Report.

We have clarified the methodological differences between the SOAP approach, tracer-based apportionment, and model-derived OA budgets, and expanded the discussion of their respective assumptions and uncertainties. While no additional modelling has been introduced, the comparison has been made more explicit and balanced through targeted revisions to the text, as outlined below.

> *Original: The estimated SOAP results are broadly consistent with the ADCHEM model, which also identifies terpenes and aromatics as the dominant SOA precursors during the IMP campaign. However, the model substantially underestimates SOA.*
>
> *Changed to: The SOAP analysis (Fig 8) is broadly consistent with the ADCHEM model, which also identifies terpenes and aromatics as dominant SOA precursors during the IMP campaign. However, quantitatively the model substantially underestimates SOA*
>
> *Original: These discrepancies point to either missing SOA sources or underestimated SOA yields.*
>
> *Changed to: These discrepancies may reflect missing or simplified SOA precursor emissions, incomplete oxidation pathways, or uncertainties associated with empirical scaling factors used in the observation-based methods.*

Specific comments:

7. Line 68-69: The statement in the abstract that elevated ozone and SOA occurred under predominantly NOx-limited conditions appears inconsistent with the results. While Section 3.3 suggests NOx-limited ozone formation, Section 3.4 does not clearly demonstrate NOx-limited SOA formation, and the conclusion states that ozone was similarly sensitive to both NOx and BVOC at most sites. The regime characterization should be clarified for consistency.

We agree that the original wording was confusing. The revised abstract now clearly states that the NOx-limited regime refers to ozone formation, while SOA formation showed a modest decrease under elevated NOx as per the model simulations. The wording has been aligned with Sections 3.3 and 3.4.

> *Original in abstract: Elevated ozone and SOA levels were driven by the combined influence of biogenic VOCs and NOx emissions under predominantly NOx-limited conditions.*
>
> *Changed to: Sensitivity simulations indicate that ozone formation was predominantly NOx-limited across most regions during IMP2022. However, the highest ozone peaks occurred under conditions of elevated NOx in combination with enhanced BVOC emissions. In contrast, SOA formation was slightly enhanced under low-NOx conditions and reduced in elevated NOx.*
>
> *Original line 589: Delete "under NOx-limited conditions"*

8. Line 70-71: The statement that higher NOx concentrations reduce SOA formation by about 10 % appears somewhat abrupt in the abstract. It would be helpful to clarify under which conditions (e.g., sites, precursor types, or model scenarios) this reduction occurs, and to briefly indicate the underlying mechanism (e.g., altered RO2 chemistry or organic nitrate formation pathways). In addition, the sentence does not appear fully consistent with the quantitative results presented in Section 3.4 (which report changes of −5 ± 3 % for 2×NOx and +3 ± 1 % for 0.5×NOx). The origin of the "~10 %" value is unclear. Moreover, given that NOx sensitivity of SOA is only briefly discussed in the main text and not emphasized in the conclusions, it is unclear why this specific number is highlighted in the abstract, while other potentially more central findings (e.g., related to NPF) are not mentioned. This point should be reconsidered for consistency and balance.

We agree that the "~10%" value was not clearly supported by the quantitative results in Section 3.4. The sentence has been removed from the abstract, and the description of SOA–NOx sensitivity has been revised as shown above to ensure consistency with the main text.

9. Line 90-92: The two consecutive sentences on regional variability and uncertainties in vegetation-driven emissions appear somewhat repetitive (vegetation composition and vegetation types). The authors should consider clarifying the distinction between spatial variability in vegetation composition and process-level uncertainties in emission controls, including uncertainties in how

emission factors respond to environmental stressors such as heatwaves and drought, or merging the statements for conciseness.

We have revised these sentences to clearly distinguish between spatial variability in vegetation composition and uncertainties in BVOC emission responses:

> *Original: The relative contributions vary regionally, reflecting differences in vegetation composition. Furthermore, large uncertainties remain regarding the factors controlling emissions from different vegetation types (Bourtsoukidis et al., 2024, 2025; Guion et al., 2023; Seco et al., 2007).*
>
> *Changed to: The relative contributions vary regionally due to differences in vegetation composition and land cover. However, significant uncertainties remain in the parameterization of BVOC emissions, particularly regarding their sensitivity to temperature, radiation, and drought stress under extreme conditions (Bourtsoukidis et al., 2024, 2025; Guion et al., 2023; Seco et al., 2007).*

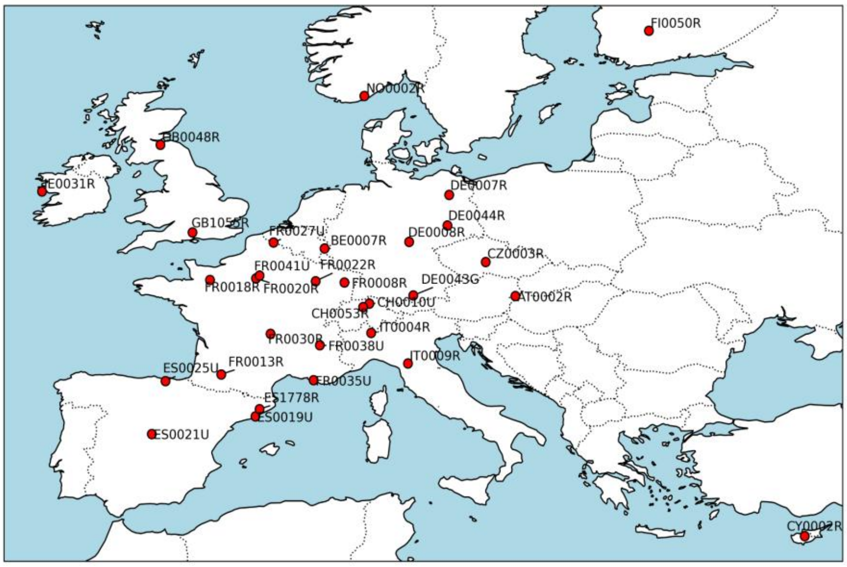
10. Line 118-122: The statement that high NO_x levels favor ozone production while low NO_x conditions favor SOA formation appears overly simplified. The effect of NO_x on both ozone and SOA is highly non-linear. In addition, following statement of NOx dependence to SOA formation is somewhat circular. The text states that SOA formation is non-linear with respect to NOx and then restates this by noting enhancement at low NOx and suppression at high NOx. However, the underlying chemical reasons for this non-linearity are not explained. A brief mechanistic clarification should be specified briefly.

We have revised this paragraph to remove the simplified formulation and to clarify the non-linear dependence of both ozone and SOA formation on NOx, while retaining appropriate references without adding extended mechanistic detail.

> *Original: The influence of NOx on the VOC pathways is not straightforward, as it depends on VOC structure and environmental conditions such as temperature, humidity and solar radiation. If the VOC concentration is sufficient, high NO_x levels favor ozone production, while low NO_x conditions favor SOA formation (Seinfeld and Pandis, 1998; Wang et al., 2024). However, NOx-dependent SOA formation can be highly non-linear. At low-NOx conditions, increasing the NOx concentration can increase SOA formation. The opposite occurs at high-NOx conditions, where increased NOx suppresses SOA yield (Nie et al., 2023; Sarrafzadeh et al., 2016; Yan et al., 2020).*
>
> *Changed to: The influence of NOx on VOC oxidation pathways is not straightforward, as it depends on VOC structure and environmental conditions such as temperature, humidity, and solar radiation. Ozone production generally increases with NOx under VOC-rich conditions, but the response is non-linear and depends on the VOC/NOx ratio (Seinfeld and Pandis, 1998). Similarly, SOA formation exhibits a non-linear dependence on NOx. At low-NOx conditions, moderate increases in NOx can enhance SOA formation, whereas at higher NOx levels, further increases tend to suppress SOA yields (Nie et al., 2023; Sarrafzadeh et al., 2016; Yan et al., 2020)*

11. Section 2.1: Although the spatial variability of species is quite important topic in this manuscript, the geographical distribution of the campaign sites is not immediately clear. While site names and coordinates are provided in Supplement Table S1, it is difficult to obtain an intuitive overview of the spatial coverage, particularly for readers less familiar with European geography and name of cities. The authors may consider explicitly referring to Table S1 in the opening of Section 2.1 and adding a map illustrating the locations of the participating sites with site numbers to improve clarity.

We added a Figure of the site locations in Section 2.2:



12. Section 2.2: The detailed description of PTR-QMS and PTR-ToF-MS m/z assignments in Section 2.2 can be moved to the Supplement (in a table or so), with only a brief summary retained in the main text to improve conciseness.

As this manuscript is now submitted as a Measurement Report, we consider that a transparent description of the PTR-MS methodology and its limitations is important for assessing data comparability across sites. We have therefore retained the core description of PTR-QMS and PTR-ToF-MS measurement principles and potential interferences in the main text.

13. Line 264-266: Please briefly clarify the reason for prioritizing canister data for benzene and toluene, while Tenax data were prioritized for other aromatic compounds.

Both benzene and toluene concentrations from the Tenax TA-Carbopack B tubes results were most uncertain due to the high background concentrations in benzene. Very high toluene values were detected at some French stations. This could be due to contamination during sampling, storage or transportation. For the rest of the aromatics, the sampling method was OK. We added a comment on this in the revised manuscript :

> “Benzene and toluene data from canisters were against that from Tenax tubes due to problems with high blank values”

14. Line 362-364: The statement that the large contribution of O-VOCs highlights the need to expand monitoring networks is not fully supported by the analysis presented here. While O-VOCs dominate in terms of mixing ratio, their relative importance for ozone or SOA formation is not explicitly demonstrated in this section. The authors may consider clarifying what is meant by their “known impact on atmospheric chemistry” or supporting this statement with reactivity-based arguments.

We agree that this statement is not clearly supported in the text here. We deleted this sentences and rather emphasised on the monitoring programme in the conclusion

15. Line 396-397: As noted in General comment #4, The statement that low nighttime isoprene concentrations at Ispra suggest a dominant biogenic source is not fully convincing. Given the short atmospheric lifetime of isoprene, low nighttime concentrations would be expected regardless of whether the source is local or transported. Moreover, no diel cycle figure is presented to support this interpretation. The reasoning should be clarified or supported with additional evidence.

We agree that low nighttime concentrations alone do not demonstrate a biogenic source. We have revised the text to remove this argument and instead refer more generally to site characteristics and known temperature- and light-dependent emission behaviour.

> *Original: These are likely from biogenic emissions, Peyrusse Vieille being located far from large anthropogenic sources, while a clear diel cycle with low nighttime concentrations of isoprene at Ispra also suggests a dominant biogenic source.*
>
> *Changed to: These elevated isoprene concentrations are consistent with dominant biogenic emissions. Peyrusse Vieille is located far from major anthropogenic sources, and the pronounced daytime maxima at Ispra are characteristic of temperature- and light-dependent biogenic emission patterns.*

16. Line 399-415: The discussion largely attributes monoterpene concentrations to biogenic sources. However, compounds such as α-pinene, β-pinene, and limonene may also have anthropogenic sources (e.g., volatile chemical products, cleaning agents). A brief acknowledgment of potential anthropogenic contributions, particularly at sites influenced by urban or regional emissions, would provide a more balanced interpretation.

We have revised the text to acknowledge that certain monoterpenes may also originate from anthropogenic sources, particularly at sites influenced by urban or regional emissions. We added the following text to the revised manuscript:

- *Although monoterpenes are primarily of biogenic origin, some compounds such as α-pinene, β-pinene, and limonene may also have anthropogenic sources (e.g., volatile chemical products and cleaning agents), particularly at sites influenced by urban or regional emissions.*
- *At sites with potential anthropogenic influence, however, contributions from non-biogenic sources cannot be excluded.*
- *..as well as possible local emission influences.*

17. Line 552-558: The description of the Ox and basic ozone photochemistry could be shortened or moved to the Introduction.

We moved most of this to the method chapter describing the setup for the ADCHEM model in chapter 2.3

18. Line 560-567: As noted in General comment #4, the explanations provided for model biases (e.g., marine air masses, misrepresented nighttime conditions, inadequate representation of free tropospheric influence, overestimated emissions, limited vertical mixing) appear largely interpretative and are not directly supported by quantitative evidence or references in the text. Please clarify whether these attributions are supported by trajectory analysis, sensitivity tests, or additional diagnostics. Otherwise, the statements should be framed more cautiously.

The explanations of model biases were intended as plausible interpretations rather than demonstrated causal attributions. In the revised manuscript, we have moderated the language by adding the wording “indicate” and “likely” where appropriate.

19. Line 569 and Figure 7: The purpose and interpretation of Figure7 are not entirely clear. While the figure presents a detailed ozone production and loss budget (expressed in DU over 0–2100 m and integrated over the past three days), the manuscript provides very limited explanation of how these values are calculated and how they should be interpreted. In particular, it is unclear whether the reported values represent time-integrated column production along trajectories, and why DU is used to express chemical production and loss. A clearer description of the budget calculation and a more detailed discussion of the dominant terms and sensitivity responses would be needed.

We agree with the reviewer that the description of how these values were calculated is unclear. The reported values represent height (atmospheric column) and time-integrated ozone production and losses along the trajectories. We have added an explanation of how the model accounts for the gas-phase chemistry production and losses of ozone in the revised manuscript (see text below). We chose to use DU since we investigated the time-integrated production and loss of ozone in an atmospheric column, which covers the atmospheric planetary boundary and the nighttime residual layer. An alternative would be to present the ozone production and losses e.g. in ppb_v at specific heights, but since the ozone surface observations are influenced by the production and losses in the entire planetary boundary layer upwind the station, as a result of atmospheric mixing, we chose to present the integrated production and losses in the atmospheric column. The 0-2100 m a.g.l. atmospheric column extends beyond the maximum height of the planetary boundary layer upwind the stations and hence represent the atmosphere which primarily are influenced by the surface emissions and deposition upwind the stations.

Text added to the revised manuscript:

> *ADCHEM keeps track of both the dry and wet deposition and gas-phase chemical production and losses of tropospheric ozone. The gas-phase chemistry includes in total 1461 other organic peroxy radicals and the total ozone production rate (P_{O_3}) from the reactions between HO_2, CH_3O_2 and the other RO_2 and NO in each model height layer was calculated using Eq. 1:*
>
> $$P_{O_3} = \sum_{i=1}^{1461}\left(k_i[RO_{2,i}][NO]\right) + k_{CH_3O_2+NO}[CH_3O_2][NO] + k_{HO_2+NO}[HO_2][NO] \quad \text{(Eq. 1)}$$
>
> *k_i denote the individual reaction rates for the reactions between the different RO_2 and NO. The ozone production was then integrated over all height layers from 0 to 2100 m a.g.l. and over the time spent along the air mass trajectories upwind the stations. Similarely the model calculates the O_x loses by reactions between O_3 and in total 201 VOCs (O_3+VOC), O_3 and OH (O_3+OH), O_3 and HO_2 (O_3+HO_2), O_x lost by reactions with 9 different chlorine, bromine and iodine radicals (O_3+halognes), $O(^1D)$ reacting with water vapour ($O(^1D)$ + H_2O) and NO_2 reacting with OH (NO_2+OH).*

20. Line 591-593: Please clarify how the potential NO_2 overestimation from molybdenum converters affects the subsequent analysis, particularly the NOx-based regime classification and modelling results.

The NO_2 concentrations are not used in the analysis, mainly used for model evaluation. Rewrote to include this:

> *Original: Accurate quantification of NO_2 is therefore essential for identifying ozone production regimes and guiding effective mitigation strategies. Stations that use a molybdenum converter for their measurements (Table 1) overestimate NO2, due other reactive nitrogen oxide compounds (Reiman et al., 2018).*
>
> *Changed to: On average, the model overestimated NO_2 concentrations by 21 ± 27 % at rural sites (Table S4). One possible explanation for this discrepancy could be uncertainties in the quantification of NO_2 at sites using molybdenum converters (Table 1), where concentrations may be overestimated due to the conversion of other reactive nitrogen oxide compounds (Reiman et al., 2018). However, no systematic differences in model–measurement agreement were found between sites using molybdenum and photolytic converters.*

21. Line 610-635: While Section 3.4 includes detailed analyses in the subsequent subsections, the introductory part of Section 3.4 does not clearly state the overall objective of this section. It would help briefly outline the main question being addressed and how the following subsections consisted of.

We included a short introduction:

> *To explore the spatial and temporal distribution of carbonaceous aerosols during the IMP2022, EC/OC was measured at all the sites with tracer analysis except at Mt Cimone (IT0009R) , 22 in total (Table 1)*

22. Line 610: Please add a short summary of previous findings from Yttri et al., 2007.

Yttri et al (2007) is used as a reference to show consistency with earlier observation of a south-to north gradient of OC and EC in Europe.

23. Line: 619-620: The author may consider add a short discussion why Montseny and Ispra showed higher OC levels while others doesn't.

This section primarily presents observational results, while the potential causes of elevated OC at specific sites (including Ispra) are discussed in more detail later in the manuscript in connection with model simulations and source apportionment.

24. Line 627: As noted in General comment #4, Please provide justification for the assumed OM/OC ratio of 1.8 and discuss its potential variability and impact on the reported carbonaceous fraction. Given the wide range of environments considered in this study, applying a uniform OM/OC ratio of 1.8 may introduce uncertainty in the estimated carbonaceous contribution.

We agree with the reviewer, that using a uniform OM/OC factor might cause uncertainties. We added a sentence to highlight this:

> *Note that using one factor for all sites is likely introducing additional uncertainties since the chemical composition may vary between sites but was adopted for transparency.*

25. Section 3.4.1 (refer also general comment #6): Given that this study focuses on heatwave conditions, it would be helpful to more explicitly discuss which aspects of the observed SOA tracers (e.g., 2-MT and 3-MBTCA) are characteristic of the 2022 heatwave, rather than reflecting typical spatial gradients across Europe. As currently written, the interpretation mainly describes general regional differences in biogenic sources, without clearly highlighting what is specific to the heatwave event.

It is difficult to discuss heatwave-driven anomalies due to the limited availability of long-term tracer measurements at most sites. We have clarified this limitation in Section 3.4.1:

> *Long-term tracer measurements are not available at most sites, which limits our ability to quantitatively distinguish heatwave-driven anomalies from typical July conditions.*

26. Line 647: The statement that the 3-MBTCA/2-MT ratio reflects “a shift in the relative importance of biogenic sources” is rather general. Since 2-MT and 3-MBTCA represent oxidation products of isoprene and α-pinene, respectively, it would be helpful to more explicitly discuss how the observed spatial gradient relates to differences in vegetation type (e.g., deciduous vs. coniferous forests) and associated emission patterns across Europe.

To avoid overgeneralization and redundancy, we have removed this sentence and retained the more detailed discussion later in the section, where the 3-MBTCA/2-MT ratio is explicitly linked to α-pinene-derived BSOA and regional variability.

27. Section 3.4.2: Section 3.4.2 relies on several literature-based scaling factors and fixed conversion ratios (e.g., tracer-to-OC relationships) to estimate source-specific OC contributions. While appropriate references are cited, the associated uncertainties are not discussed. Given that these empirical factors can vary substantially depending on atmospheric conditions and source composition, especially under heatwave conditions, a quantitative assessment of uncertainty or a sensitivity analysis would strengthen the robustness of the conclusions. Specifically, in Eq 7 and 9, using fixed scaling factors appears to be a strong assumption. Please provide clearer justification and discuss its applicability across different sites and heatwave conditions.

The tracer-to-OC scaling factors used in Section 3.4.2 are based on established literature values widely applied in European source-apportionment studies, and we agree that these are uncertain. We have added a paragraph clarifying this limitation:

> *These scaling factors may vary with source composition and environmental conditions. The resulting source contributions should therefore be interpreted as estimates rather than precise quantitative apportionment.*

28. Figure 10: It presents derived OC fractions, including uncertainty estimates (e.g., error bars or ranges based on sensitivity tests) which would improve transparency.

The OC fractions shown in Figure 10 are derived from several literature-based tracer-to-OC scaling factors and a simple error bar is not available.

29. Line 760 "despite lower OH and ozone levels": As noted in General comment #4, Is this statement can be supported by any figure? There seems no mention earlier about this. Figure 7 shows the increased net ozone formation in 2xNOx scenario.

Thank you for noticing this. It is a typo. It should be "*despite higher OH and ozone levels*" We have corrected this in the revised manuscript.

Line 763-766: The description may overemphasize autoxidation as the key SOA formation pathway for aromatics. Please clarify that multiple oxidation pathways contribute to aromatic SOA formation.

We rewrote this:

> *Original: Their oxidation is initiated by OH radicals and followed by autoxidation, leading to the formation of HOM.*
>
> *Changed to: Their oxidation is initiated by OH radicals and proceeds through multiple oxidation pathways, including autoxidation and formation of HOM (Iyer et al., 2023; Pichelstorfer et al., 2024).*

30. Line 867: The discussion of VOC and NOx effects on OH, H_2SO_4 production, and particle growth appears somewhat speculative. Please clarify whether these statements are directly supported by the model results or rephrase them more cautiously.

We rewrote this:

> *Original: Increasing NOx tends to increase the atmospheric oxidation capacity (OH and O3 concentrations) enhancing the oxidation of VOCs and SO2 and the production of O-VOCs and H2SO4,*
>
> *Changed to: The model results show that increasing NOx tends to increase the atmospheric oxidation capacity (OH and O3 concentrations), thereby enhancing the oxidation of VOCs and SO2 and the production of O-VOCs and H2SO4,*

.

31. Line 869-870: Same as #25 comment. "At the same time the formed O-VOCs tend to be more volatile under high-NOx conditions, which can in contrast reduce nanoparticle GR." This also needs to be supported with an evidence.

This is imbedded in the model simulations; thus we added two relevant references ((Roldin et al., 2019; Wollesen De Jonge et al., 2024))

32. Conclusion: Given the extensive quantitative analyses presented in the manuscript, it would improve the clarity and strength of the conclusions to explicitly reference key numerical findings, rather than relying solely on general statements.

We rewrote part of the conclusion as outlined in the reply to the general comment above

33. Conclusion: No conclusion for NPF?

We added this by integrating with SOA formation text:

> *Original: Aromatic VOCs were the dominant SOA precursors, followed by monoterpenes. Model results substantially underestimate SOA and UFP concentrations,*
>
> *Changed to: The underestimation of particle growth during observed NPF events further indicates that SOA formation and nanoparticle growth were closely coupled during the campaign*

34. Line 894-895: The authors may consider briefly reflecting on the likely causes of the model underestimation identified in this study. The statement ", highlighting the need…" is also too general. It should be specified with the outcome of this study.

We deleted this general statement

35. Figure S10: Given that ozone production isopleths are highly sensitive to precursor composition and environmental conditions, the use of a single background isopleth derived from Stuttgart (2020) for all IMP sites raises questions regarding representativeness. A discussion of the potential impact of heatwave conditions and site-specific chemistry on the isopleth structure would be required. Since the background contours show O3 and the marker colors indicate PO3, it would improve clarity to include two separate color bars or otherwise differentiate the color coding to avoid misinterpretation.

We agree that the Stuttgart-based isopleths are not fully representative, this is only used here as a reference for comparison. We added a note on this in the figure caption. We have also added a separate colour bar for the isopleths to improve clarity.

36. Figure S10 caption (Line 54): Is kOH for VOC same as R VOC? If so, please make it consistent. Also, CO should not add to the VOC reactivity.

The reactivity of VOC and NOx is expressed by using the parameter "OH-reactivity" instead of concentrations to illustrate the ozone production. We changed R to Reactivity to be consistent with the Figure. CO is part of the model for calculating oxidation capacity and one need data for estimating the reactivity of VOC

37. Figure S11: The right-hand panels attribute individual VOCs to single source categories (e.g., natural gas, combustion, biogenic), which represents a strong simplification. In reality, many compounds have multiple primary and secondary origins, and their atmospheric concentrations reflect mixed and regionally transported sources. Therefore, assigning each compound exclusively to one source category may overstate the apparent source-specific contributions to OFP and SOAP. If the intention is to discuss quantitative source contributions, a more detailed source apportionment approach (PMF analysis or similar to that applied for OA in Section 3.4.2) would be necessary. At minimum, the manuscript should clearly acknowledge the limitations of this simplified categorization.

We acknowledge that the source categorization is considerably uncertain. The purpose of this analysis is not precise quantitative apportionment, but to illustrate the dominant precursor groups across sites. We have added the VOC source classification in a Supplementary Table for transparency, and added the following text in the manuscript:

> It *should be noted that the source categorization is uncertain as many VOCs have multiple primary and secondary origins. The classification is therefore intended as an indicative grouping of dominant source types. The VOC source assignments are provided in Table S3.*

Technical corrections:

38. Line 66: Change to ‘Measurement showed that oxygenated VOCs (O-VOCs) contributed the largest…’. Here, O-VOCs can be defined.

OK done

39. Line 106: Add ‘s’ to be ‘species.

OK done

40. Line 136: Add a number to indicate the limit value

We have added a specific example of a regulatory limit value (EU target value of 120 µg m^{-3} for the maximum daily 8-hour mean) to clarify the statement and added references to EEA status report on air quality

41. Table 1: What does ‘x’ mean in O3?

It means that a monitor is deployed. Added explanations in the footnote

42. Line 320: Is ‘without BVOC’ includes Isoprene? Or does it indicates only monoterpenes?

Yes this includes all biogenic BVOCs, added isoprene to avoid confusion

43. Line 329: add numbers to ‘high ozone levels’.

Added above 80 ppbv

44. Line 382-384 “…by elevated NO2 values at…” & Line 386 “(e.g. AT0002R, FR0008R, FR0018R) without correspondingly high NO2 levels…”: Figure 4 does not show NO2 values. Is this referring Figure S2 or? Please refer a figure to this sentence.

Yes it is Figure S2. Added this reference

45. Line 399-400: Please clarify the reference to “the latter two sites,” as it does not appear to match with the order in the previous sentence.

Changed the order to be consistent

46. Line 561: Add site numbers for Birkenes, Mace Head, and Ispra.

Added

47. Figure 9: Unit for 3-MBTCA/2-MTs should be unitless.

Corrected

48. Line 820 “H_2SO4”: Subscript 4.

Corrected

49. Line 887: Change “combustion” to “anthropogenic”.

Changed

50. Line 894: Add “ADCHEM”.

Added

Referee #2: (https://egusphere.copernicus.org/#RC2):

The manuscript entitled “Spatial variability of VOCs, ozone, and carbonaceous aerosols during the 2022 European summer heatwave” collected and analyzed data on NMHCs, OVOCs, monoterpenes, sesquiterpenes, larger hydrocarbons, as well as OC/EC from 31 sites across Europe during July 12-19, 2022. A wide range of models and methods were employed for analysis, which plays an important role in understanding the spatial distribution characteristics of these pollutants during the European heatwave. However, this paper exhibited substantial deficiencies in: the comparability of multiple data types; the uncertainty, rationality, and applicability of the modeling approaches; and the coherence and logic among different sections of manuscript. In its current version, I cannot recommend its publication.

We thank the reviewer for the thorough evaluation and for raising important concerns. We have revised the manuscript to be submitted as a Measurement Report, placing primary emphasis on the coordinated multi-site observational dataset, while retaining the modelling analyses as interpretative support.

My specific concerns are as follows:

1. Abstract: It is recommended that the abstract be reorganized and rewritten to highlight the quantitative results and innovations of this study, rather than merely providing qualitative descriptions

   The abstract has been revised to improve clarity and to present the main observational results and supporting model findings more concisely. The wording has also been adjusted to avoid overly conclusive statements in accordance with being a Measurement Report manuscript. (see details in reply to Referee 1)

2. Introduction section should be reorganized and rewritten. In its current version, the author covered a large amount of content, including the macro-level background of air pollution and health, the key role of volatile organic compounds (VOCs) in atmospheric chemistry, the effectiveness and shortcomings of European emission reduction policies, and the limitations of the current monitoring system, etc. However, the logical connections between these topics were not very clear. Meanwhile, the key scientific question this study aims to address was the spatial variability of VOCs, O_3, and carbonaceous aerosols across Europe during the heatwave, and its implications for understanding O_3 and SOA formation. Therefore, it seems necessary for the authors to summarize and review whether similar studies have been conducted in Europe or globally. To the best of my knowledge, such studies do exist. The authors should clarify the differences between this study and those previous works, as well as its unique features, to highlight the innovation of this study.

   Yes, there are several studies of VOC spatial variability, coordinated multi-site observations combining VOC speciation, aerosol composition, and tracer analyses during a heatwave event remain limited, thereby highlighting the specific contribution of this study. We added the following sentence to the introduction:

   While previous studies have examined spatial variability of VOCs and their role in ozone and SOA formation, coordinated multi-site observations combining a broad suite of VOC species, aerosol chemical composition, and tracer analyses during a single, well-defined heatwave event remain limited.

3. Materials and methods: The authors need to provide adequate and reasonable quality control analysis and uncertainty analysis for the sampling and analysis components. Specifically, the authors collected and analyzed NMHCs, OVOCs, monoterpenes, sesquiterpenes, and larger hydrocarbons, as well as OC and EC at 31 sites. Were the sampling instruments for the same component consistent across all sites? Was any comparability analysis conducted on these instruments prior to sampling? Furthermore, different components were collected using

different sampling instruments and analyzed with different analytical platforms. For example, NMHCs were collected using Silcosteel canisters and analyzed by gas chromatography-mass spectrometry and flame ionization detector at Forschungszentrum Jülich; OVOCs were collected using solid adsorbent cartridges coated with 2,4-dinitrophenylhydrazine, with derivatives analyzed by high-performance liquid chromatography equipped with a UV detector at the ACTRIS Centre for Reactive Trace Gases in-situ measurements at IMT Nord Europe, France; monoterpenes, sesquiterpenes, and larger hydrocarbons were collected using Tenax TA-Carbopack B tubes and analyzed by thermal desorption-gas chromatography–mass spectrometers at the Finnish Meteorological Institute. Therefore, the comparability of these components was questionable, as the sampling and measurement processes for VOCs often involve substantial uncertainties. The authors did not adequately address this critical point in the manuscript, which poses substantial risks to all the analytical data and conclusions drawn therefrom.

We have clarified in the Methods section that measurements followed harmonised SOPs within EMEP and ACTRIS, and that centralised analyses were used to ensure comparability across sites. We also acknowledge that additional site-based measurements may introduce variability and have added a brief discussion of this:

All measurements followed established SOPs within the EMEP and ACTRIS frameworks, and centralised analyses were applied for key compound classes to ensure comparability across sites. While different analytical techniques were used for different VOC groups, these represent standard methods for the respective compounds. Additional site-based measurements (e.g., PTR-MS) may introduce some variability; where available, parallel measurements were used to assess consistency.

4. Section 2.3: The authors provided a detailed description of the model configuration. However, there was a clear lack of discussion on how the reasonableness of the model simulations was validated, how uncertainties in input data (such as emission inventories under extreme heatwave conditions) were assessed, and how the impacts of missing key processes (particularly wildfires and drought) were assessed. This deficiency leads readers to question the validity of the subsequent attribution analyses conducted with this model (e.g., “NO_x reduction would lower ozone” and “aromatics dominate SOA”). Therefore, it is recommended that the authors supplement this section by explaining: 1) how uncertainties in emission inventories under extreme heatwave episodes were addressed and quantified; 2) why wildfire emissions, an obviously influential factor, were omitted from the model; and 3) whether the model’s capability to simulate new particle formation and SOA formation has been subjected to more detailed validation.

   This section has now been updated with the following sentence and reference to the wildfire emission inventory that we used:

   The model also accounts for emissions of gases and particles from wildfire emissions using the satellite based GFED5 emission inventory (van der Werf et al., 2025)

   Further we added some text on previous studies on NPF and SOA using the ADCHEM model:

   In previous publications it has been demonstrated that ADCHEM generally is able to reproduce observed diurnal and seasonal trends in submicron particle number size distributions, organic aerosol mass concentrations and ozone concentrations at several rural continental stations in Europe and marine polar environments (Roldin et al., 2019; Olenius and Roldin, 2022; Xavier et al., 2024; Wollesen de Jonge et al., 2024; Zhang et al., 2024; Svenhag et al., 2025).

5. Section 3.1: This section should be revised and improved to provide a clearer understanding of what this heatwave episodes entailed, the severity of ozone pollution, and the evolution of

meteorological conditions. Additionally, this section suffered from poor correspondence between text and figures, completely overlooks the important influence of wildfires when explaining model biases, and presents a disconnect between the meteorological description and subsequent analytical sections

Section 3.1 describes the meteorological evolution and observed ozone levels during the IMP2022 period. To improve clarity, we have strengthened the linkage between the text and figures and added brief summary statements highlighting the progression of the heatwave and its relationship to ozone levels. We have also included a short statement acknowledging the influence of wildfire activity during the period:

This progression illustrates how the heatwave developed from southwestern Europe toward central and northern regions, leading to a gradual build-up of ozone across the continent (Fig. 2). The period was also affected by wildfire activity in parts of southern and western Europe, which likely contributed to elevated precursor levels and ozone formation.

The spatial and temporal evolution of ozone closely followed the movement of the high-pressure systems and associated temperature increases, highlighting the strong coupling between meteorological conditions and ozone formation during the event.

The detailed role of emissions and chemical processes is discussed in the subsequent sections, adding this text:

The role of emissions and chemical processes underlying these patterns is further explored in the following sections.

6. Section 3.2: the authors devote considerable space to analyzing the variability of different VOC species across multiple sites. However, such variability analysis presupposes that the data are comparable. Since the authors did not adequately and reasonably demonstrate in the Materials and Methods section that VOC data obtained under different measurement and analytical methods are indeed comparable, the multi-site comparative analysis presented here is therefore questionable.

   The comparability of VOC measurements across sites is ensured to a large extent by the use of centralised laboratories for the analysis of key compound groups, following harmonised SOPs within the EMEP and ACTRIS frameworks. This approach was specifically adopted to minimise inter-laboratory variability and ensure consistency across the network as described in point 3 above.

   We acknowledge that some additional measurements performed at individual sites (e.g., PTR-MS) may introduce variability. These aspects are now clarified in the Methods section, where we also describe the prioritisation of methods and the use of parallel measurements where available. We therefore consider the multi-site comparison to be sufficiently robust for the purposes of this study.

7. Section 3.3: the authors employ two analytical approaches: one is the calculation of ozone formation potential based on fixed POCP values, and the other is sensitivity experiments using the ADCHEM model. What is the rationale for using these models in this study? Meanwhile, the authors completely avoid discussing the potential impacts of model biases themselves (such as the overestimation of VOCs in Southern Europe and the overall underestimation of OC by up to 50%) on ozone simulations and the assessment of NOx sensitivity. They also fail to consider the measurement bias caused by the use of molybdenum converters at some sites, which leads to overestimation of NO_2 observations. Consequently, the core conclusion that “ozone formation is mainly NOx-limited” is built upon an inadequately validated evidence base.

The two approaches serve complementary purposes. The POCP-based ozone formation potential provides a literature-based, observation-driven under controlled conditions estimate while the ADCHEM sensitivity simulations allow assessment of the response of ozone and SOA to changes in precursor emissions. We acknowledge that model biases exist and these limitations are discussed in the manuscript We added some text on the NO2 bias and comparison with model (see point 22 in reply to Referee 1)

8. Section 3.4: the authors employed a tracer extrapolation method heavily reliant on fixed conversion factors for source apportionment. However, they did not conduct any sensitivity analysis or uncertainty assessment regarding the applicability of these factors during a wildfire-dominated heatwave. As a result, the final estimated SOA contribution (84±11%) appears precise but may merely represent an “exact mistake”.

   We agree that the SOA contribution is a crude estimate. We added in the intro to the source apportionment (3.4.2). See point 27 in the reply to Referee 1.

9. Section 3.5: the description of NPF events relies heavily on visual judgment and lacks quantitative and statistical analysis of key parameters such as formation rates and growth rates, rendering conclusions like "the model captures [the events] well" devoid of objective basis. Furthermore, this section simplistically attributes the model's systematic underestimation of particle number concentrations to underestimated SOA, creating a circular argument. It also fails to integrate the VOC observations from Section 3.2 and the SOA analysis from Section 3.4 to investigate the root causes of the underestimation, resulting in a lack of organic integration among the three threads of NPF, VOCs, and SOA.

   We agree that a fully quantitative analysis of NPF formation and growth rates, as well as a detailed process-based attribution linking VOCs, SOA, and NPF, would require additional dedicated analysis beyond the scope of this study.

   In the revised manuscript, we have therefore moderated the wording in Section 3.5 to avoid overinterpretation, i.e:

   Original: The onset of this major regional NPF and particle growth event was qualitatively well captured by the model at all stations.

   Changed to: The model qualitatively reproduced the timing of the onset of this regional NPF and particle growth event across the stations.

   We have also strengthened the discussion of limitations, particularly regarding the underestimation of SOA and its implications for particle growth, and improved the linkage between Sections 3.4 by adding this sentence:

   The underestimation of particle number concentrations is consistent with the underestimation of SOA (Section 3.4), suggesting a link between SOA formation and particle growth, although a quantitative attribution is beyond the scope of this study.

10. Additionally, there are some minor issues that need revision. For example, if the authors have not conducted statistical tests, they should avoid using the word “significantly”; alternatives such as “substantially” or “markedly” could be used instead. Line 106: The word "specie" contains a spelling error (it should be “species”).

    We have replaced significantly with *substantially, considerably, notably* or *markedly* where appropriate, and we’ve corrected the spelling mistake.

Referee #3 (https://doi.org/10.5194/egusphere-2025-6166-RC3)

General Overview:
The manuscript (egusphere-2025-6166) presents results from an Intensive Measurement Period (IMP) conducted during the European heatwave in July 2022, focusing on ozone, volatile organic compounds (VOCs), and carbonaceous aerosols at 31 sites across Europe. The topic of this study falls within the scope of the journal Atmospheric Chemistry and Physics (ACP). The authors present data from field measurements and analyze the temporal and spatial variability of the measured species. This manuscript is generally laid out well and shows its academic value. This manuscript is recommended to be published as a measurement report after addressing the concerns and comments below with minor revisions.

We thank the reviewer for constructive feedback., We have revised the manuscript accordingly to a Measurement Report

Major Concern:

- Section 2.2: The criteria and logics for prioritizing the sources of data do not seem to be clearly provided. Only the facts of data screening and prioritization were described. Please revise this section to ensure academic rigor.

We have clarified the criteria used for prioritising data when multiple measurements were available. The selection was based on methodological suitability, temporal resolution, and comparability across sites, with continuous measurements generally preferred where available, and centralised analyses ensuring consistency across the network.

Minor Concerns:

- Lines 290 - 291: Where are the simulated trajectories? Please provide a figure or figures of the simulated trajectories.

We added the trajectory figures to the Supplementary

- Lines 466 - 468: The statement is misleading the readers to think that the ratio would be MACR/MVK, while “their ratio” is actually MVK/MACR in Fig. 5. Please revise the statement for clarification.

Rewrote ratio to MVK/MACR

- Lines 472 - 473: The statement is misleading the readers to think that the slope would be (MVK+MACR)/HCHO while “the slope” is actually HCHO/(MVK+MACR) in Fig. 5. Please revise the statement for clarification.

Rewrote to HCHO/ (MVK+MACR) slope

- Lines 633 - 635: Are there studies in the literature showing that urban areas are more influenced by dust compared with regional sites? Please provide the supporting statements with citations.

We deleted this statement as we have no evidence that this is the case during IMP2022

Technical Comments:

- Line 106: The word “specie” seems to be a typographical error of “species”.

Corrected

- Line 206: The sentence seems to mean that the species were quantified by high-performance liquid chromatography (HPLC) but contains some typographical errors.

Corrected

- Line 721: Does “PMF” stand for “Positive Matrix Factorization”? Please use the full form before using the acronym by itself.

Yes. Added