Authors' responses to the reviewers

Dear Editor and Reviewers,

Thank you very much for your careful and constructive review of our manuscript (Manuscript No. EGUSPHERE-2025-3428), entitled "Evaluating the EPICC-Model for Regional Air Quality Simulation: A Comparative Study with CAMx and CMAQ."

We appreciate the decision of minor revision and the additional comments. These comments have helped us further improve the clarity and accuracy of the manuscript. We have carefully considered all comments and revised the manuscript accordingly.

A **Revision List** and **detailed Point-By-Point Responses** are provided in the following sections. All major revisions in the manuscript are marked in *blue*. The line numbers provided refer to the clean version of the revised manuscript without tracked changes.

Thank you once again for your time and effort in handling our manuscript.

Yours sincerely,
The Authors of EGUSPHERE-2025-3428

Responses to Reviewer 1

Comment 1:

Section 1, line 70: It is appropriate to use (Ramboll, 2020) as a reference for CAMx, but as a User's Guide it is considered grey literature. It would be better to include a published reference like (Emery et al., 2024). That reference is cited elsewhere in the revised manuscript but there is no listing for it in the Reference section.

Response to comment 1:

We thank the reviewer for this helpful suggestion. We agree that the CAMx User's Guide (Ramboll, 2020) is grey literature. Following the reviewer's recommendation, we have retained the Ramboll citation and additionally included the peer-reviewed reference Emery et al. (2024) in Section 1, line 70. This published paper describes the formulation and evaluation of CAMx and is therefore an appropriate complement to the User's Guide. Emery et al. (2024) was already cited elsewhere in the revised manuscript; we have also checked the Reference section and ensured that this entry is clearly listed. The revised text is as follows:

(Lines 69-71) *... the Comprehensive Air Quality Model with Extensions (CAMx) developed by Ramboll (Ramboll, 2020; Emery et al., 2024), ...*

Comment 2:

Section 2.1, lines 150-151: "whereas CMAQ and CAMx use 14σp layers." This specification of sigma-p is not correct for CAMx and is not entirely correct for CMAQ. CAMx uses an absolute terrain-following height coordinate (rather than normalized height sigma-z coordinate) and CMAQ uses the hybrid normalized pressure coordinate system directly from WRF. I suggest simply removing the sigma-p variable from the sentence and wherever it is used throughout the document when referencing the CMAQ and CAMx vertical coordinates. Since the EPICC-Model algorithms are largely based on CAMx, does it also use the same vertical coordinate as CAMx? If so, I suggest also removing the reference to sigma-z for EPICC-Model.

On a related note, the authors' response to my original comment about inconsistent number of layers was: "Regarding the different vertical layer structures used by the three models, the primary reason is that EPICC-Model adopts a terrain-following height coordinate, whereas CAMx and CMAQ employ terrain-following sigma-pressure coordinates. Due to differences in vertical coordinate definitions and internal numerical implementations, it is not feasible to maintain a completely identical vertical layer structure among the three models." This argument is based on an incorrect understanding of the CAMx vertical coordinate and how CAMx and CMAQ accommodate the WRF layer structure, as I point out above. Therefore, using this argument for 20 vs. 14 layers is invalid. I appreciate that the authors' response above was not included in the manuscript.

Response to comment 2:

We thank the reviewer for correcting our description of the vertical coordinates. We agree that referring to CAMx as using σ_p layers was incorrect, and that describing CMAQ simply as σ_p was also incomplete.

As clarified in the CAMx documentation and Emery et al. (2024), CAMx is formulated in a terrain-following absolute height (z) coordinate. The vertical grid is defined externally by layer-interface heights (in meters). In WRF-driven applications, these heights are generated by the WRF-CAMx interface from WRF geopotential fields and are then read by CAMx (e.g., as ZGRID_M). Consequently, CAMx does not integrate the continuity equation in WRF's sigma-pressure (or hybrid) space; rather, WRF's native pressure-based coordinate is converted offline to height interfaces for CAMx. Because the interface heights are supplied by time-varying meteorological inputs, the CAMx vertical grid may vary in space and time.

Although several EPICC physical and chemical algorithms were adapted from CAMx, its vertical grid was independently designed and does not use the CAMx coordinate. EPICC vertical levels were defined as:

$$\sigma_z = (z - h_s)/(H - h_s)$$

where h_s is terrain elevation and $H = 20$km is the fixed model top. The layer-center heights and thicknesses are prescribed from this normalized coordinate, and the WRF meteorological fields are vertically interpolated to these target heights. EPICC therefore does use a terrain-following normalized-height (σ_z) coordinate. We have retained the σ_z designation for EPICC but revised the manuscript to define it explicitly and to distinguish it from the CAMx coordinate.

We would, however, like to clarify one specific detail concerning CMAQ vertical layers. The CMAQ simulations in this study were prepared using MCIP v4.5, and the driving WRF v3.9.1 simulation did not use the hybrid-vertical coordinate option. Consequently, CMAQ did not use the WRF hybrid normalized pressure coordinate in this study. Instead, it used the original WRF-ARW terrain-following dry-hydrostatic-pressure eta coordinate. MCIP v4.5 selected and collapsed the WRF layers through its CTMLAYS specification to generate the 14-layer CMAQ grid. Thus, the CMAQ layers retained the non-hybrid WRF eta-coordinate structure but did not necessarily retain every original WRF layer.

We further agree that our earlier explanation was invalid: the 20-layer versus 14-layer difference is not necessitated by incompatible vertical coordinate definitions. CAMx and CMAQ can accommodate or aggregate the WRF layer structure; the 14-layer setups used here reflect operational forecasting configurations adopted for the year-long comparison (considering computational cost and forecast workflow), not a fundamental coordinate constraint. We have revised the related wording accordingly and no longer use the previous incorrect argument. The revised text is as follows:

(Lines 155-161) *In the operational configurations adopted in this study, the EPICC-Model employed 20 terrain-following normalized-height (σ_z) layers extending from the terrain surface to a fixed model top of 20 km, whereas CAMx and CMAQ were configured with 14 vertical layers. For CMAQ, MCIP v4.5 selected and collapsed layers from the original non-hybrid WRF-ARW terrain-following eta coordinate. For*

CAMx, WRFCAMx aggregated selected WRF layers and converted their full-level geopotential heights into time-varying physical layer-interface heights.

Comment 3:

Section 2.1, lines 151-155: Should there be an explicit mention of all 3-D transport treatments for EPICC-Model and CMAQ in the text like there is for CAMx?

Response to comment 3:

We appreciate this comment. To keep the level of detail consistent across models, we have expanded Section 2.1 to state the horizontal advection, vertical advection, and vertical diffusion schemes for the EPICC-Model and CMAQ, alongside the existing CAMx transport description. The revised text is as follows:

(Lines 167-171) *For three-dimensional transport, the EPICC-Model employed the Walcek scheme for both horizontal and vertical advection, and a first-order K-theory approach for vertical diffusion with diffusivities based on the YSU boundary-layer parameterization. CMAQ used the Piecewise Parabolic Method (PPM) for both horizontal and vertical advection and the Asymmetric Convective Model version 2 (ACM2) for vertical diffusion.*

Comment 4:

Section 2.1, lines 159-169: The flow of this paragraph could be improved by moving the rationale for the use of CB05 chemistry in CAMx after the chemistry descriptions of EPICC-Model, CMAQ, and CAMx (at line 182). Also, I suggest dropping the sigma-p reference on line 160 per my comment above.

Response to comment 4:

We appreciate the reviewer's suggestion. We have revised Section 2.1 by removing the reference to the σp vertical configuration and moving the rationale for using the CB05 chemical mechanism in CAMx to after the descriptions of the chemical mechanisms used in the EPICC-Model, CMAQ, and CAMx, thereby improving the paragraph's logical flow and readability.

Comment 5:

Section 2.1, lines 186-187: This statement remains imprecise. I suggest something like: "CMAQ and CAMx also support using global models like MOZART to provide initial and boundary conditions, but simpler sources of boundary conditions were used for CMAQ and CAMx." Then provide the rationale for those choices.

Response to comment 5:

We appreciate the reviewer's helpful suggestion. We have revised the statement in Section 2.1 to specify the initial and boundary conditions used by each model. The EPICC-Model used MOZART output. CMAQ and CAMx can also use global model output, such as MOZART, but profile-based boundary conditions were used in this study to retain their operational configurations. CMAQ boundary conditions were

generated from the CB6r3 profile file, and CAMx initial and boundary conditions were prepared with icbcprep. These configuration files are provided in the Zenodo repository (https://doi.org/10.5281/zenodo.17055755). The revised sentence is as follows:

(Lines 204-213) *Regarding boundary conditions, the EPICC-Model incorporated MOZART outputs as the initial and lateral background fields. Although CMAQ and CAMx also support the use of global model outputs, such as MOZART, to provide initial and lateral boundary conditions, simpler profile-based approaches were adopted for these two models to retain their operational configurations during the study period. Specifically, CMAQ boundary conditions were generated based on the CB6r3 profile file, and CAMx initial and boundary conditions were prepared using the icbcprep procedure with the corresponding profile configuration, which can be found in the Zenodo repository (https://doi.org/10.5281/zenodo.17055755).*

Comment 6:

Section 3.1, lines 390-391: "In spring, elevated PM2.5 concentrations occurred in several geographically separated regions." I think the authors are referring to observed conditions, that could be clarified by replacing "occurred" with "were measured" (or observed).

Response to comment 6:

We thank the reviewer for this careful reading and helpful suggestion. As the reviewer correctly noted, this sentence is intended to describe the observational features of springtime $PM_{2.5}$. We have therefore replaced "occurred" with "were observed" to make this meaning explicit. The revised sentence is as follows:

(Lines 419-421) *In spring, elevated $PM_{2.5}$ concentrations were observed in several geographically separated regions, including North China, the arid basins of Northwest China, and the Sichuan Basin.*

Comment 7:

Section 3.2, lines 554-555: "and the elevated fraction mainly results from underestimation of the total measured-component concentration." I do not understand this sentence, please revise to make the point more clearly.

Response to comment 7:

We thank the reviewer for pointing out the ambiguity in this sentence. We apologize for the unclear wording. Our intended meaning was that, in Tianjin, the absolute BC concentrations are simulated reasonably well, whereas other measured $PM_{2.5}$ components (particularly OC) are underestimated in absolute magnitude. As a result, the simulated total of the measured components becomes too low, and the relative mass fraction of BC appears elevated even though the absolute BC bias remains small. This interpretation is also consistent with the later discussion that simulated $PM_{2.5}$ in Tianjin is substantially lower than observations, primarily due to OC underestimation. The revised sentence is as follows:

(Lines 583-587) *In Tianjin, however, the absolute BC bias is small. The elevated BC mass fraction is mainly attributable to the underestimation of other measured $PM_{2.5}$ components in absolute terms (particularly OC), which reduces the simulated total of these components and thereby increases the relative contribution of BC.*

Technical Corrections:

1. Be sure to define all acronyms and abbreviations at the time of their first occurrence.

2. Use consistent past or present verb tense. Many sentences within single paragraphs shift between past and present throughout the manuscript.

3. Section 3.1, Line 383: Replace "As shown in Figure 3" with "As shown in Table 3".

4. Section 3.4, Line 709: The use of the European form of "centre" is inconsistent with the use of "center" throughout the rest of the paper.

Response to Technical Corrections:

1. We have carefully checked the manuscript and defined acronyms and abbreviations at their first occurrence. In particular, we have now provided full expansions for VOCs, OC, BC, EDGAR, MEGAN, and SOAP at first use, and we have also reviewed other abbreviations throughout the text to improve consistency and clarity for readers.

2. We have revised the manuscript to improve tense consistency within paragraphs, following a clear writing convention: (i) past tense is used for the methods applied in this study and for the results obtained from our simulations and evaluations; and (ii) present tense is reserved for general scientific facts, established physical/chemical mechanisms, and descriptions of model capabilities that are not specific to a single result sentence. On this basis, we have gone through the Introduction, Methods, Results, and Conclusions and revised sentences that previously mixed past and present tense within the same paragraph.

3. The sentence has been corrected to "As shown in Table 3", because the IOA values discussed here are presented in Table 3.

4. We have changed "centre" to "center" at this location and checked the manuscript for consistent American spelling of this word.

Responses to Reviewer 2

Comment 1:

L129-136: Though the authors highlight the EPICC system to be modular plug-in architecture, the evaluation itself does not assess alternate process representations or numerical schemes. If those assessments have been performed, it may be useful to include relevant citations for such. Also, if alternate approaches are available in the plug-in architecture, it would be useful to state the rationale for the choices in EPICC configuration in Table 1.

Response to comment 1:

We thank the reviewer for this constructive comment. Although the EPICC-Model features a modular plug-in architecture, the present manuscript does not evaluate alternate process representations or numerical schemes. The aim of this study is a year-long multi-model comparison of the EPICC-Model, CAMx, and CMAQ under consistent meteorology and emissions using operational configurations, rather than an intra-model sensitivity analysis of EPICC module options.

Assessments of alternate EPICC scientific options have nevertheless been reported in prior work. In particular, Wang et al. (2025) developed a multi-option scientific-module architecture in the EPICC-Model and evaluated alternative photolysis and gas-phase chemistry schemes (including CBM-Z and CB6r5) for tropospheric ozone simulation. A comprehensive description of the modular EPICC framework is provided by the EPICC-Model Working Group (2026). We have added a brief clarification in Section 2.1 and cited these studies accordingly.

Regarding Table 1, alternate approaches are available in the EPICC plug-in architecture. The EPICC configuration used here was selected to represent the operational forecasting setup for the study period and to enable comparison with CAMx and CMAQ under the same WRF meteorology and emission inputs.

For the reviewer's information, the rationale for the main EPICC choices in Table 1 is as follows:

(1) Walcek advection and first-order K-theory vertical diffusion with YSU-based diffusivities were adopted for consistency with the WRF/YSU meteorological fields used in this study.

(2) CB6r5 coupled with RADM aqueous-phase chemistry was selected as a relatively complete Carbon Bond mechanism suitable for regional O_3 and secondary-aerosol applications.

(3) ISORROPIA v2.2 and the two-product SOA treatment were used for inorganic and organic aerosol processes.

(4) ZHANG03 dry deposition and Henry's-law wet deposition were retained as the operational deposition options.

(5) full-science TUV (preprocessor) with streamlined in-model adjustment was used for photolysis, consistent with the CAMx-type workflow.

(6) specialized treatments of sulfate formation, heterogeneous HONO production, and N_2O_5 hydrolysis were retained as distinctive EPICC process representations for

complex pollution in China.
The revised sentence is as follows:
(Lines 138-142) *Although the EPICC-Model supports multiple interchangeable scientific options within its plug-in architecture, the present study evaluates a single operational configuration (Table 1) rather than performing intra-model comparisons of alternate process representations or numerical schemes. Such option-to-option assessments have been reported elsewhere (Wang et al., 2025; EPICC-Model Working Group, 2026).*

Comment 2:

Table 1: In the photolysis row and associated discussion, what does "Full science" imply?

Response to comment 2:

We thank the reviewer for pointing out this ambiguity. In the photolysis discussion, "full-science TUV" refers to the complete Tropospheric Ultraviolet and Visible (TUV) radiative-transfer model used offline as a preprocessor to generate clear-sky photolysis-rate lookup tables. This is distinct from the streamlined TUV module applied inside the model during the simulation, which adjusts the precomputed rates for local cloud and aerosol effects. We have revised the corresponding sentence in Section 2.1 to state this distinction more clearly. The revised sentence is as follows:
(Lines 201-204) *For photolysis calculations, both the EPICC-Model and CAMx used the full Tropospheric Ultraviolet and Visible (TUV) radiative-transfer model as a preprocessor to generate photolysis-rate input files. A streamlined TUV module was then applied internally to adjust these rates for cloud and aerosol effects.*

Comment 3:

L149-151: In their response to the choice of differing layer structures employed in the various models, the authors noted that these represented "operational configurations actually implemented in their respective routine air-quality forecasting systems". Are additional details or references available on these air quality forecasting configurations that interested readers can be pointed to? Even though the impacts of layer structure on the model comparison for surface level pollution are not discernible in the results presented, some readers are likely to be curious about why the models against which EPICC is compared employed a seemingly degraded layer structure/vertical resolution? Also, perhaps it should be noted that even though not employed in this study, harmonizing grid resolutions across models is desirable in any future intercomparison study.

Response to comment 3:

Thank the reviewer for this follow-up comment. The 14-layer configurations of CAMx and CMAQ should not be read as an inherent limitation or a "degraded" vertical capability of these models. Both CAMx and CMAQ can be run with substantially finer vertical structures, including setups that more closely follow the native WRF layering.

In this study, the 14-layer setups corresponded to the operational air-quality forecasting configurations used by the participating modeling teams during the study period. They were adopted for routine forecasting after considering computational cost, forecast timeliness, numerical stability, and compatibility with existing preprocessing and forecasting workflows.

Public documentation of the exact operational namelist settings is limited. Related guidance and published applications that reflect the same class of regional forecasting practice include the *Chinese technical guideline for numerical air-quality forecasting* (HJ 1130–2020) (MEE, 2020) and previous regional CAMx applications over China using computationally efficient operational-type configurations (Wen et al., 2021). The EPICC configuration used here is consistent with the EPICC v1.0 framework described by the EPICC-Model Working Group (2026). Section 2.1 has been revised to clarify that the difference in layer number is an operational choice for this intercomparison.

For future controlled intercomparison studies that aim to isolate process-level differences, harmonizing horizontal and vertical grid resolutions across models would be preferable. The revised sentence is as follows:

(Lines 161-166) *The 14-layer configurations of CAMx and CMAQ used here reflect operational forecasting setups adopted for the study period, rather than a model-required or intrinsically coarser vertical capability; both models can be configured with finer vertical structures. For future controlled intercomparisons aimed at isolating process-level differences, harmonizing vertical and horizontal grid resolutions across models would be desirable.*

Comment 4:

L185-196: To my knowledge the CMAQ boundary condition processor does include options to extract limited area LBCs from model outputs on a larger domain – say hemispheric CMAQ simulations, not just the profile file as suggested in this discussion. The impact of LBC specification on simulated surface concentrations is not only confined to areas in vicinity of the boundary but across large geographic domains, since specification of free tropospheric LBCs is influential due to rapid lateral transport at these altitudes and subsequent mixing to surface (see for example: https://doi.org/10.5194/acp-17-12449-2017; https://doi.org/10.5194/acp-18-3839-2018; https://doi.org/10.1021/acs.est.8b02496). Consequently, differences in LBC specification could be contributing to the noted differences between model O3 and PM2.5 at the surface and should be acknowledged in this discussion. Also, it would be useful to provide some comparison of O3 and PM2.5 values at altitudes of the top of the nominal daytime PBL (say ~2km) along the dominant inflow boundary for the 3 models. This may help better gauge the impacts of the different LBC specifications.

Response to comment 4:

We appreciate the reviewer's suggestion. CMAQ was run with profile-based lateral boundary conditions generated from the CB6r3 profile file. This setup matched the operational forecasting configuration used for the year-long comparison and does not

represent the full capability of CMAQ. BCON can also extract time-varying LBCs from concentration fields on a parent or hemispheric domain; that option was not used here. CAMx used icbcprep profiles, whereas the EPICC-Model used MOZART-derived initial and lateral background fields. The three models therefore differed in their chemical boundary specification.

Because free-tropospheric air can be advected rapidly along the domain perimeter and later mixed into the boundary layer, inconsistent LBCs may influence surface O_3 and $PM_{2.5}$ over large portions of the domain, not only near the lateral edges (Mathur et al., 2017; Hogrefe et al., 2018; Zhang and Arunachalam, 2018). This contribution cannot be separated from differences in chemistry, transport, and deposition in the present experiments. The surface evaluation remains valid as a comparison of operational configurations; inter-model differences should not be attributed solely to internal process formulations. Section 2.1 has been revised accordingly.

A comparison of O_3 and $PM_{2.5}$ near 2 km along the dominant inflow boundary was not added. The archived analysis was designed around surface observations. A robust inflow diagnostic would require a season-dependent definition of the inflow sector, interpolation of the three models onto a common geometric height despite their different vertical coordinates, and a separation of boundary air from in-domain production. Those steps were not part of the original post-processing protocol and cannot be completed robustly from the existing surface-oriented dataset. This limitation is stated in the manuscript and is recommended as a priority for future intercomparisons that archive three-dimensional fields for boundary-inflow analysis. The revised text is as follows:

(Lines 218-223) *Free-tropospheric LBCs may also influence surface O_3 and $PM_{2.5}$ away from the domain edges through rapid transport aloft and subsequent downward mixing (Mathur et al., 2017; Hogrefe et al., 2018; Zhang and Arunachalam, 2018). A comparison of O_3 and $PM_{2.5}$ near ~2 km along the dominant inflow boundary was not performed, which limits diagnosis of LBC contributions in this surface-oriented evaluation.*

Comment 5:

L359: "daytime particulate nitrate photolysis" is somewhat redundant. Could probably just say "particulate nitrate photolysis".

Response to comment 5:

We appreciate this comment. "Daytime" has been removed. Particulate nitrate photolysis is already a sunlight-driven process, so the additional qualifier was unnecessary. The revised text reads as follows:

(Lines 387-391) *Recent EPICC-specific sensitivity and source-diagnosis studies further demonstrate that these HONO pathways, including particulate nitrate photolysis, contribute to simulated HONO and O_3 formation and improve the representation of O_3 over China (Zhang et al., 2025; EPICC Model Working Group, 2026).*

Comment 6:

L642: Please clarify if manganese catalyzed SO2 oxidation is associated with aerosol water?

Response to comment 6:

Thank the reviewer for this valuable suggestion. the Mn-catalyzed SO_2 oxidation represented in EPICC is associated with aerosol liquid water. More specifically, following Wang et al. (2021), it is treated as an interfacial heterogeneous reaction occurring within the surface layer of deliquesced aerosol particles. Aerosol liquid water is required to maintain a liquid particle surface and dissolved Mn (II); gas-phase SO_2 is taken up at this surface, rapidly oxidized through Mn-mediated reactions, and the resulting sulfate is subsequently mixed into the particle liquid phase. Consistent with the original parameterization, this pathway is activated only when RH exceeds 35%, as a proxy for the presence of aerosol liquid water. However, aerosol water content is not included as a separate multiplicative term in the rate expression; its influence is represented through the RH criterion and aerosol-phase properties, including dissolved Mn(II) concentration, acidity, ionic strength, and particle surface area. The manuscript has been revised to state "manganese-catalyzed SO_2 oxidation at the air-liquid interface of deliquesced aerosol particles."

Comment 7:

L841-844: Perhaps it is a matter of framing, but in my view representation of natural dust, boundary layer mixing, secondary aerosol formation pathways, and emission input data are all areas of ongoing active research and model development. Thus, "priorities for future model enhancements" may be better wording than "priorities for future model development".

Response to comment 7:

The wording has been changed from "priorities for future model development" to "priorities for future model enhancements," to indicate continued improvement of existing treatments rather than the initiation of new model development. The revised text reads as follows:

(Lines 872-875) *By identifying model-specific limitations and challenges shared across the three models, the comparison highlights priorities for future model enhancements, including improved representations of natural dust, boundary-layer mixing, secondary aerosol formation, and emission inputs.*