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"Improving aerosol–radiation interaction feedback in AIRWISE operational system"

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Replies to Reviewers

We sincerely thank the Editor and the Reviewers for their careful evaluation of our manuscript and for providing constructive and insightful comments. We greatly appreciate the time and effort invested in reviewing the manuscript. The comments and suggestions have significantly helped us to improve the scientific clarity, presentation, and overall quality of the manuscript.

We have carefully addressed all comments raised by the reviewers and revised the manuscript accordingly. Detailed point-by-point responses are provided below. Reviewer comments are reproduced in bold text, and our responses are provided below each comment.

Comments from Reviewer #1

General comments

This manuscript studies aerosol–radiation interactions in the AIRWISE (WRF-Chem) operational system over Delhi. The authors update aerosol complex refractive indices (RI) and run a control simulation and sensitivity experiments for Oct 2023–Jan 2024. They show impacts on SWDOWN, near-surface meteorology, PBL height, and PM2.5, and they compare SWDOWN with WiFEX observations at IGI airport.

Overall, I do not see a major conceptual problem in the approach. The study is rather straightforward (control run + sensitivity runs with changed optical assumptions), and results are mostly plausible. However, I am not fully convinced about the novelty level for ACP, but this is editor's decision. Several key parts need clearer description and more careful wording of conclusions.

Main comments

1) "Region-specific" refractive indices – please justify or reword

The manuscript repeatedly calls the modified RI values "region-specific". However, I did not find a clear explanation why these values are truly specific to Delhi. Many of the changes look like more general literature updates (e.g., updated BC imaginary part, and discussion of absorbing OC/brown carbon at shorter wavelengths), rather than values specifically suitable for aerosol composition in Delhi or based on measurements (use of Mishra and Tripathi, 2008 for dust, is perhaps an exception). Please clarify your motivation to use "region-specific" here.

Response: We thank the reviewer for this important comment. We agree that the term "region-specific" may not accurately describe the modified refractive index (RI) values used in this study. The intention of this work was not to prescribe uniquely Delhi-derived RI values for all aerosol species, but rather to incorporate updated and more realistic RI values for aerosol chemical species based on recent literature and observational studies relevant to polluted environments such as Delhi and the Indo-Gangetic Plain (IGP).

The modified RI values represent literature-constrained updates to the optical properties of aerosol species such as black carbon, organic carbon/brown carbon, and dust, which are more representative of polluted conditions such as over Delhi compared to the default values used in WRF-Chem.

Accordingly, we have revised the terminology throughout the manuscript (e.g., line 15 in the revised manuscript) and replaced the phrase "region-specific refractive indices" with "updated and more realistic refractive indices of aerosol species". We have clarified this point in the revised manuscript (line 239-241) to avoid ambiguity regarding the applicability and origin of the RI values.

2) Hygroscopic growth / aerosol water uptake is not described in Methods, but it is critical information

I did not find a clear description in the model setup section about aerosol hygroscopic growth / aerosol water uptake and how it is treated in the optical calculations. This is important because aerosol optical effects depend not only on RI and size distribution, but also strongly on RH-driven water uptake (wet size, wet RI, and thus extinction/scattering).

You mention in the Results that the model has difficulty with fog because of RH bias, and you link this to underestimated hygroscopic growth and to PM2.5 underestimation. I think this statement is too vague and may be confusing. If your model PM2.5 includes aerosol water mass (wet PM2.5), then low RH can directly reduce PM2.5 through reduced water uptake. But I assume, and it should be clearly stated, that you compare dry PM2.5 mass. Then, if "underestimated hygroscopic growth" is used to explain lower dry mass, I assume you mainly mean that fog/high RH conditions increase secondary aerosol formation / partitioning and the model misses that process when RH is too low. This should be clarified and explained in more detail.

Response: We thank the reviewer for this important comment. We agree that aerosol hygroscopic growth and aerosol water uptake are important processes influencing aerosol optical properties and should be described more clearly in the manuscript. In the revised version, we have expanded Section 2.2 (line 225-230) to explain how aerosol water uptake is represented in WRF-Chem and how it influences the calculation of aerosol optical properties. Specifically, we clarify that aerosol optical properties are calculated for hygroscopically grown particles under ambient relative humidity conditions, and that aerosol water uptake affects particle size and, consequently, aerosol extinction, aerosol optical depth (AOD), single-scattering albedo (SSA), and aerosol-radiation interactions. We have also added text in line 253-258 in the revised manuscript describing how the revised refractive indices interact with hygroscopically grown particles to modify their scattering and absorption characteristics.

We also agree that the original discussion linking underestimated hygroscopic growth directly to lower $PM_{2.5}$ concentrations was not sufficiently clear. The $PM_{2.5}$ concentrations evaluated in this study represent aerosol dry mass and do not directly include aerosol water mass. Therefore, reduced hygroscopic growth under low-RH conditions primarily affects aerosol optical properties by limiting particle growth and optical enhancement rather than directly reducing the reported $PM_{2.5}$ mass. In addition, under foggy and high-humidity conditions, biases in relative humidity can suppress aqueous-phase processing, secondary aerosol formation, and gas-particle partitioning, thereby contributing indirectly to $PM_{2.5}$ underestimation. To address this concern, we have revised the discussion in the Results section (line 392-395 in the revised manuscript) to clarify the distinct roles of hygroscopic growth, aerosol optical enhancement, and secondary aerosol production in influencing $PM_{2.5}$ concentrations during foggy periods.

3) Radiation observations in Fig. 2 are too vaguely described ("observations", "net radiometer")

In Fig. 2 the observational curve is labelled only as "observations", and in the text you mention "net radiometer". This is ambiguous because a "net radiometer" can mean a single-output net radiation instrument (net all-wave), or a 4-component system that provides downward and upward SW and LW components separately.

Please add some relevant instrument metadata (type/model if possible) and specify the variable used in the comparison. Also, please state the spectral range of the SW sensor. Some pyranometers do not cover the full solar range, and this can create a small systematic offset when comparing to modeled broadband SWDOWN (even a couple percent).

Response: We thank the reviewer for this valuable suggestion. We agree that the description of the radiation observations in the original manuscript was not sufficiently detailed and could lead to ambiguity regarding the observational dataset used for comparison.

The radiation measurements were obtained from a 4-component Net Radiometer (Delta Ohm LPNET14) installed at the Winter Fog EXperiment (WiFEX) observational site at Indira Gandhi International (IGI) Airport, Delhi. The instrument consists of upward- and downward-facing pyranometers and pyrgeometers that separately measure incoming and outgoing shortwave and longwave radiative fluxes. In the present study, only the observed downward shortwave radiation (SWDOWN) component was used for comparison with the model-simulated SWDOWN.

The shortwave pyranometer sensors of the Delta Ohm LPNET14 operate over a spectral range of approximately 0.3–3.0 µm, which covers most of the broadband solar spectrum relevant for SWDOWN comparison with the model radiation fields. We acknowledge that small systematic differences may still arise due to differences between the instrumental spectral response and the modelled broadband solar radiation treatment.

The observational data were available at 1-minute temporal resolution and were averaged to hourly values to match the hourly WRF-Chem model output used in this study. We have also clarified that the comparison is appropriate because the lowest model level is located at approximately 2 m above ground level, which is reasonably consistent with the near-surface observational measurements.

Accordingly, additional details regarding the instrument type, measured variable, temporal averaging, and spectral range of the SW sensor have now been included in line 305-310 in the revised manuscript and figure description (Fig. 2).

4) Time axis and diurnal mismatch. Fig. 2 uses IST ("Local Time").

It is normal that the SWDOWN peak is not exactly at 12:00 IST because IST is a national time reference and local solar noon in Delhi is shifted. This is not a problem. However, more important is the model–observation mismatch pattern: it looks like the model is too high in the morning (until near noon), while agreement is better in the afternoon. Please discuss possible causes. From your paper, I assume that morning fog / cloudiness is perhaps the key reason, even now when you tried to have clear-sky cases only. But is it true or are there other reasons that would be useful information for the readers? What was the time window of ground-based measurement, hourly? Could the reason be then as simple as the time stamp for hourly mean being not fully representative for hourly mean, which would mean different impact before and after noon (when SZA is decreasing, and increasing, respectively).

Response: We thank the reviewer for this insightful comment. We agree that the peak in SWDOWN does not necessarily occur exactly at 12:00 IST because IST represents a national standard time, whereas the local solar noon over Delhi occurs somewhat earlier.

Regarding the model–observation mismatch pattern, the comparison shown in Fig. 2 represents the monthly mean diurnal cycle for December 2023. The radiation observations were available at 1-minute temporal resolution and were averaged to hourly values prior to comparison with the hourly WRF-Chem output. Therefore, uncertainties associated with temporal aggregation are expected to be relatively small and unlikely to fully explain the systematic morning overestimation.

We agree that morning fog and cloud contamination can influence SWDOWN comparisons. However, foggy periods were excluded during the clear-sky case selection procedure, and the positive SWDOWN bias persists even under these filtered conditions. Furthermore, we have noticed a similar overestimation in the model-simulated top-of-atmosphere shortwave radiation, where aerosol and cloud effects are absent. This suggests that the morning bias is unlikely to be solely attributable to aerosol loading, fog, or cloud representation.

At present, the exact origin of this bias remains unclear and may be related to inherent characteristics of the radiation parameterization, solar geometry treatment, or other aspects of the model formulation. A comprehensive investigation of these processes would require dedicated sensitivity experiments and detailed evaluation of the radiation schemes, which is beyond the scope of the present study.

5) Emissions year / representativeness

In Methods, anthropogenic emissions are from EDGAR-HTAP v2.2 (2010) and you mention “fine-gridded emissions over Delhi-NCR”. Please clarify what this refinement is (source/year/scaling). This is relevant for aerosol composition and PM2.5 interpretation in 2023–24

Response: We thank the reviewer for this important comment. We agree that additional clarification regarding the anthropogenic emission inventory and its representativeness for the 2023–24 period is necessary, particularly because aerosol composition and $PM_{2.5}$ concentrations over Delhi are strongly influenced by local emission characteristics.

In the present study, anthropogenic emissions over the outer model domain were obtained from the EDGAR-HTAP v2.2 inventory (Janssens-Maenhout et al., 2015), which provides sector-specific emissions at a horizontal resolution of 0.1° × 0.1°. However, because this coarse-resolution inventory cannot adequately represent the strong spatial heterogeneity of urban emissions over Delhi-NCR, the emissions within the Delhi region were refined using the High-resolution Delhi Emission Inventory (HrDEI) developed under the System of Air Quality and Weather Forecasting and Research (SAFAR) programme (Jena et al., 2021).

The HrDEI is a bottom-up inventory developed specifically for Delhi-NCR and is available at 400 m spatial resolution. It incorporates detailed sector-wise emissions from transportation, residential combustion, industries, power plants, construction activities, waste burning, and other urban sources. The original inventory was developed for the year 2018. Following Jena et al. (2021), the high-resolution emissions were aggregated to the model grid using a mass-conserving approach, thereby preserving total emissions while substantially improving the representation of urban emission hotspots and spatial variability across Delhi-NCR.

We acknowledge that the HrDEI inventory was developed for 2018 and therefore does not fully account for temporal changes in emissions up to 2023–24. However, its use provides a much more realistic representation of the spatial distribution and intensity of anthropogenic emissions over Delhi-NCR compared to the default EDGAR-HTAP inventory alone. We have revised the manuscript (line 189-196) to explicitly describe the source, year, spatial resolution, and implementation methodology of the refined emissions inventory, as well as its implications for the interpretation of aerosol composition and $PM_{2.5}$ simulations.

Reference:

Jena, C., Ghude, S. D., Kumar, R., Debnath, S., Govardhan, G., Soni, V. K., ... & Rajeevan, M. (2021). Performance of high resolution (400 m) $PM_{2.5}$ forecast over Delhi. *Scientific reports*, *11*(1), 4104.

Janssens-Maenhout, G., Crippa, M., Guizzardi, D., Dentener, F., Muntean, M., Pouliot, G., ... & Li, M. (2015). HTAP_v2. 2: a mosaic of regional and global emission grid maps for 2008 and 2010 to study hemispheric transport of air pollution. Atmospheric Chemistry and Physics, 15(19), 11411-11432.

Minor comments / technical points

The phrase "position vector of the particle" (line 69) is confusing in this context. What did you mean: "spatial location (grid cell)" rather than something about particle microphysics? Please reword to avoid misunderstanding.

Response: We thank the reviewer for pointing this out. The phrase "position vector of the particle" was used basically to describe the spatial location of the aerosol particle in three-dimensional space within the model framework.

The statement "hematite … with global average of 6.85% (Goudie, 1978)" (line 80), and other similar %-numbers there, is unclear. Please specify % of what (mass fraction of dust? fraction of iron oxides? something else). As written, it is ambiguous.

Response: We thank the reviewer for pointing out this ambiguity. We agree that the percentage values mentioned in the original text were not sufficiently defined.

In the cited discussion, the reported percentages refer to the mass fraction of hematite (and other iron oxide components) within mineral dust aerosols. We have revised the text (line 80-84) to explicitly clarify this point and avoid ambiguity.

Accordingly, the statement has been modified to read along the lines of: "Hematite constituting approximately 6.85 % of the total global mineral dust mass on average (Goudie, 1978)"

Similar clarifications have also been added for the other percentage values discussed in the same section.

"The AOD at specific wavelengths, such as 400 and 600 nm, is derived using the Ångström exponent formula" (line 72) I assume 400, 600 are not derived using AE, since these belong to those four shortwave radiation bands. Please be more specific here.

Response: We thank the reviewer for this important clarification. We agree that the original wording was imprecise and could incorrectly imply that the AOD values at 400 and 600 nm were themselves derived using the Ångström exponent (AE) relationship.

In our implementation, the aerosol optical properties are initially available at four shortwave bands. The AOD values at 400 and 600 nm are part of these available optical properties and are used to derive the Ångström exponent. The derived AE is subsequently used to interpolate AOD to the additional wavelengths required by the RRTMG shortwave radiation scheme (232–3462 nm).

To avoid ambiguity, we have revised the manuscript (line 71-74) to explicitly state: "The Ångström exponent is derived from AOD values at specific wavelengths, such as 400 and 600 nm, and is subsequently used to interpolate AOD to other wavelengths within the shortwave spectrum (232–3462 nm) of the WRF-Chem."

"… in which we have removed the cloud cover (set > 0.1) in the model along with the foggy hours …" (line 301) Please clarify what "cloud cover >0.1" means (units/definition) and how cloud cover is estimated (ceilometer method?). Please give the needed.

Response: We thank the reviewer for pointing out the need for additional clarification regarding the cloud-screening methodology used to identify clear-sky conditions.

In the model simulations, cloudy and foggy conditions were excluded using the WRF-Chem-simulated cloud fraction (CLDFRA) field. Specifically, only those hours were retained for which the cloud fraction remained below 0.1 (dimensionless fractional cloud cover) throughout the entire atmospheric column over Delhi. Consequently, any hour with cloud fraction greater than 0.1 at any model level was excluded from the analysis. It should be noted that WRF-Chem does not provide a separate clear-sky SWDOWN diagnostic; therefore, clear-sky conditions were identified through cloud screening rather than by using a model-generated clear-sky radiation output.

For the observational datasets, cloudy and fog-affected periods were identified using ceilometer backscatter measurements and the associated cloud detection products. Hours corresponding to the presence of cloud or fog layers were removed from the observed SWDOWN dataset. Thus, only cloud-free and fog-free periods were retained.

The SWDOWN evaluation was therefore restricted to periods classified as clear-sky in both the model and observational datasets. In other words, hours for which the model indicated cloud cover, but the observations were cloud-free, or vice versa, were excluded from the comparison. We have revised the manuscript (line 348-355) to explicitly define the cloud fraction threshold, describe the cloud-screening methodology, and clarify the role of ceilometer-based cloud and fog detection in the observational filtering procedure.

Comments from Reviewer #2

The manuscript by Kumar et al. involves implementation of improved optical properties in regional model simulations with WRF-Chem, and quantification of the subsequent influence on biases in surface shortwave radiation and other variables. By using region-specific refractive indices instead of default values, they find reduced shortwave radiation at the surface and reduced biases compared to observations. The study is fairly well-written, and highlights the importance of using region-specific optical properties, but I have several comments as outlined below.

Main comments

Generalizability: The study is done for a few months over a small region in North-India, mainly using observations from only one station in Delhi. In addition, the WRF-Chem simulations use GOCART, which is a fairly simple bulk aerosol scheme (except that dust and sea salt are size resolved). While its numerical efficiency is an advantage over more sophisticated aerosol schemes, GOCART is mostly useful when the focus is on complex gas phase chemistry and not aerosols. In this case the focus is on aerosols, and I therefore wonder about the generalizability of the findings. Can the findings be put in a broader context? Except for the conclusion that it seems to have some importance for near-surface meteorology to use region-specific refractive indices, I am not sure about the usefulness of the results for others. For instance, can you discuss how the use of GOCART vs. a more detailed scheme like MOSAIC could have influenced the results? Are the default refractive indices in the model specific for each aerosol scheme or universal across all WRF-Chem aerosol schemes? I.e., is the necessity for updating the refractive indices specific to GOCART or is it for WRF-Chem in general? Do the authors think the findings would have changed if a more detailed aerosol scheme would have been used, and if so how? I think a discussion of these issues may make the findings more useful for WRF-Chem users that employ more detailed aerosol schemes.

Response: We thank the reviewer for this important comment. We agree that a discussion of the broader applicability of our findings and the implications of using different aerosol schemes strengthens the manuscript.

Use of GOCART and Implications for Generalizability:

The objective of this study was to assess the sensitivity of aerosol–radiation interactions and associated meteorological feedback to aerosol refractive indices (RI) within WRF-Chem during highly polluted conditions over Delhi. We used the GOCART aerosol scheme because of its computational efficiency and its widespread application in operational and research air-quality simulations over India.

We acknowledge that GOCART is a relatively simple bulk aerosol scheme compared to more detailed sectional schemes such as MOSAIC, which explicitly represent aerosol size distributions and composition. Consequently, simulations using MOSAIC may produce different quantitative responses in aerosol optical properties, radiative forcing, and meteorological feedback due to its more detailed aerosol representation.

Relevance of Refractive Indices Across Aerosol Schemes:

The central focus of this study is the role of aerosol refractive indices in aerosol optical calculations. In WRF-Chem, aerosol optical properties are ultimately calculated using aerosol size, composition, and complex refractive indices, which are then passed to the same Mie-scattering framework to derive extinction coefficients, aerosol optical depth, single-scattering albedo, and asymmetry parameters. Therefore, uncertainties associated with aerosol refractive indices are not specific to GOCART but are relevant to aerosol–radiation interaction calculations across WRF-Chem aerosol modules, including MOSAIC.

For this reason, we expect the main conclusion of this study is that more realistic and updated refractive indices can significantly influence surface shortwave radiation, boundary-layer evolution, and near-surface $PM_{2.5}$ concentrations, to remain applicable across different aerosol schemes. The magnitude of the response may vary depending on how aerosol size distribution and composition are represented, but the importance of accurate refractive indices is expected to remain.

Observational Representativeness:

Regarding observational representativeness, the detailed evaluation of the simulated experiments was conducted using high-quality radiation measurements from the Winter Fog EXperiment (WiFEX) campaign at Indira Gandhi International (IGI) Airport, Delhi during two representative winter pollution periods. In addition, model simulations were performed continuously for October 2023–January 2024 to evaluate the seasonal impact of the RI modifications during the post-monsoon and winter periods.

Broader Applicability Beyond Delhi:

The modified RI values have been independently evaluated using surface radiation observations from multiple India Meteorological Department (IMD) stations across diverse regions of India, including the Indo-Gangetic Plain, desert, coastal, and central Indian regions. These analyses showed consistent improvements in simulated surface shortwave radiation and aerosol–radiation interactions. The results of this broader assessment are currently being prepared for a separate publication.

Changes Made in the Revised Manuscript:

To address the reviewer's concern, we have expanded the discussion section (line 663-678) to include:

(i) the rationale for using GOCART,

(ii) limitations of bulk aerosol representations,

(iii) potential implications of using more detailed schemes such as MOSAIC, and

(iv) the broader applicability of refractive-index updates in WRF-Chem aerosol–radiation interaction studies.

Reference:

Kumar, Sumit and Jadhav, Ashwin Vijay and Govardhan, Gaurav and Koundal, Kartik and Ghude, Sachin D., Impacts of realistic aerosol optical properties on simulated downwelling shortwave radiation across the Indian region. Available at SSRN: https://ssrn.com/abstract=6730902 or http://dx.doi.org/10.2139/ssrn.6730902

Novelty: Linked to the comment above, I am left wondering how important the updating of refractive indices really is compared to other factors. The discussion section mentions uncertainties due to urban morphology and soil moisture, and touches upon weaknesses of the GOCART scheme and emission inventories. In particular, I think the use of GOCART together with an old emission inventory is a substantial weakness of the present study. The GOCART scheme does not include SOA or nitrate aerosols, which are likely very important contributors to PM2.5 in the region, especially due to large ammonia emissions from agriculture causing formation of nitrate particles. I am of course in favor of updating to more realistic RIs, but the improvement in modelled SWDOWN and meteorological variables seems minor compared to the overall model vs. observation differences. Given also my comment above about the findings being very specific to a certain location and model setup, I am therefore wondering if the study is novel enough for ACP, but I will leave that decision to the Editor.

Response: We thank the reviewer for the careful evaluation and constructive comments regarding the novelty of the study and the relative importance of refractive-index (RI) uncertainties compared to other sources of uncertainty in WRF-Chem simulations. We agree that aerosol simulations can be influenced by multiple factors, including aerosol chemistry, emissions, urban morphology, land-surface processes, and aerosol optical properties.

Novelty and Scientific Motivation:

The primary objective of this study is to isolate and quantify the sensitivity of aerosol–radiation interactions to aerosol refractive indices within the WRF-Chem framework. While numerous studies over Delhi and the Indo-Gangetic Plain have examined uncertainties associated with emissions, aerosol loading, aerosol chemistry, urban processes, and boundary-layer dynamics, comparatively limited attention has been given to the refractive indices used internally in aerosol optical calculations.

Recent work by Feng et al. (2025) demonstrated that modifications to aerosol optical property treatments and refractive-index assumptions can significantly influence radiative fluxes and aerosol–radiation interactions in WRF-Chem. Building on this work, the present study incorporates updated and observationally constrained refractive indices representative of Indian aerosol conditions. In particular, the dust refractive indices were adopted from measurements over Rajasthan, India, a major dust source region influencing Delhi and the Indo-Gangetic Plain. Thus, the novelty of the present work lies in evaluating how updated and regionally representative aerosol optical properties influence coupled aerosol–radiation–meteorology interactions.

Limitations of the GOCART Aerosol Scheme:

We acknowledge the reviewer's concern that GOCART does not explicitly represent secondary organic aerosol (SOA) and nitrate aerosol, both of which can contribute substantially to wintertime $PM_{2.5}$ over northern India. This limitation has been discussed in the manuscript (line 665-667).

Nevertheless, the objective of this study is not to evaluate aerosol chemistry mechanisms but to investigate the sensitivity of aerosol optical calculations to refractive indices. GOCART was selected because it remains computationally efficient and is widely used in operational and research applications (Ghude et al., 2024). Furthermore, the role of refractive indices in determining aerosol scattering and absorption is not unique to GOCART and remains relevant regardless of the aerosol module employed.

Emission Inventory Considerations

We would also like to clarify that the simulations do not rely solely on the global EDGAR-HTAP inventory. Following the methodology of Jena et al. (2021), emissions over Delhi and National Capital Regions (NCR) were refined using the high-resolution emission inventory developed under the project of System of Air Quality and Weather Forecasting and Research (SAFAR) specifically for Delhi and NCR using bottom-up sectoral activity data. This substantially improves the representation of urban emissions compared to the use of coarse global inventories alone.

We nevertheless acknowledge that uncertainties remain in all available Indian emission inventories due to rapidly changing anthropogenic activities, residential combustion, road dust, agricultural emissions, and biomass burning. This limitation has been further discussed in the revised manuscript (line 654-658).

Importance of RI Uncertainty Relative to Other Uncertainties

We fully agree that uncertainties associated with urban morphology (Bhautmage et al., 2024; Jayakumar et al., 2025), soil moisture, land-surface processes (Parde et al., 2022), aerosol chemistry, and emissions (Sengupta et al., 2022; Ghude et al., 2024) can have a substantial impact on simulated meteorology and air quality. Previous studies have demonstrated significant improvements in model performance through better representation of these processes.

However, these uncertainties act through different physical pathways than those investigated here. The objective of this study is not to suggest that aerosol refractive indices represent the dominant source of uncertainty in WRF-Chem simulations. Rather, it is to quantify an uncertainty that directly controls aerosol optical properties and aerosol–radiation interactions but has received comparatively limited attention.

Our results demonstrate that, even when all other model components remain unchanged, the use of physically realistic refractive indices produces systematic and physically consistent changes in SWDOWN, near-surface meteorology, boundary-layer evolution, and $PM_{2.5}$ concentrations. Therefore, refractive-index uncertainty should be viewed as a complementary source of uncertainty that warrants consideration alongside emissions, aerosol chemistry, urban processes, and land-surface processes.

Significance of the Results

We acknowledge that the improvements associated with RI modifications are smaller than the total model–observation differences. However, this is expected because refractive indices represent only one component of a complex modelling system. The purpose of the study is not to eliminate all model biases, but rather to quantify the contribution of aerosol optical property assumptions to aerosol–radiation interactions.

The results demonstrate that realistic RI values can systematically improve simulated radiative fluxes and influence coupled meteorological and air-quality responses without modifying emissions, chemistry, or physical parameterizations. We believe this finding is scientifically valuable because refractive indices are commonly prescribed parameters in aerosol models, yet their uncertainties are rarely examined in regional air-quality studies.

Accordingly, we believe that the present study provides a novel contribution by identifying and quantifying the impact of regionally representative aerosol refractive indices on aerosol–radiation interactions and coupled aerosol–meteorology feedbacks within WRF-Chem.

References:

Bhautmage, U. P., Ghude, S. D., Parde, A. N., Kamath, H. G., Gokul Dhangar, N., Pleim, J., ... & Rajeevan, M. (2024). Implementation of WRF-urban asymmetric convective model (UACM) for simulating urban fog over Delhi, India. *Journal of Geophysical Research: Atmospheres*, *129*(20), e2024JD040760.

Feng, J., Zhao, C., Du, Q., Yang, Z., & Jin, C. (2025). Amending the algorithm of aerosol–radiation interactions in WRF-Chem (v4. 4). *Geoscientific Model Development*, *18*(3), 585-603.

Ghude, S. D., Govardhan, G., Kumar, R., Yadav, P. P., Jat, R., Debnath, S., ... & Rajeevan, M. (2024). Air quality warning and integrated decision support system for emissions (AIRWISE): enhancing air quality management in megacities. *Bulletin of the American Meteorological Society*, *105*(12), E2525-E2550.

Jayakumar, A., Anurose, T. J., Bhati, S., Hendry, M. A., Hayman, G., Gordon, H., ... & Ravichandran, M. (2025). Development of an integrated modeling framework for visibility and air quality forecasting in Delhi. *Bulletin of the American Meteorological Society*, *106*(2), E261-E274.

Jena, C., Ghude, S. D., Kumar, R., Debnath, S., Govardhan, G., Soni, V. K., ... & Rajeevan, M. (2021). Performance of high resolution (400 m) PM2. 5 forecast over Delhi. *Scientific reports*, *11*(1), 4104.

Parde, A. N., Ghude, S. D., Sharma, A., Dhangar, N. G., Govardhan, G., Wagh, S., ... & Niyogi, D. (2022). Improving simulation of the fog life cycle with high-resolution land data assimilation: A case study from WiFEX. *Atmospheric Research*, *278*, 106331.

Sengupta, A., Govardhan, G., Debnath, S., Yadav, P., Kulkarni, S. H., Parde, A. N., ... & Ghude, S. D. (2022). Probing into the wintertime meteorology and particulate matter (PM2. 5 and PM10) forecast over Delhi. Atmospheric Pollution Research, 13(6), 101426.

Length: The results are very specific for WRF-Chem with the GOCART scheme over Delhi. It does not have wider implications, as far as I can tell, other than that those using WRF-Chem should check that the RIs are representative of their region. I would therefore have preferred a shorter paper. One suggestion for shortening is to remove section 3.5 on the monthly-mean picture since those results are largely in line with the prior analysis on the events. Instead, the associated Figure 9 and Table 3 could be moved to the supplementary, and then been discussed, very briefly, in the other subsections. Another thing would be to remove results for domain 1 entirely and only show results for domain 2, unless there is a good reason for keeping the domain 1 results. Results are anyway shown over Delhi, which is included in domain 2.

Response: We thank the reviewer for the constructive suggestions regarding the manuscript structure and the presentation of multi-domain results. We respectfully retain both the monthly-scale analysis and the results from Domain-1 and Domain-2 because they provide important scientific and operational context for interpreting the aerosol-radiation sensitivity experiments.

Regarding the use of two domains, the principal conclusions of this study are not strongly dependent on horizontal model resolution. Domain-1 (10 km) and Domain-2 (2 km) simulations exhibited very similar responses in SWDOWN following the implementation of the updated aerosol refractive indices. To demonstrate this consistency, we have added supplementary tables comparing the SWDOWN changes from both domains. The analyses presented in the manuscript are based on spatial averages over the Delhi region rather than isolated grid-point comparisons, with $PM_{2.5}$ evaluated using approximately 40 monitoring stations distributed across Delhi. The similarity of the responses across both domains indicates that the identified RI-induced aerosol-radiation sensitivity is robust and not an artifact of a particular model resolution.

We also believe that retaining Domain-1 has operational relevance. One of the motivations of this work is to improve aerosol optical property representation within computationally efficient WRF-Chem forecasting frameworks. While convection-permitting simulations at 2 km resolution provide additional spatial detail, they are computationally expensive for long-duration forecasting applications. Demonstrating comparable radiative responses in a 10 km configuration therefore highlights the applicability of the proposed RI updates to operational forecasting systems such as AIRWISE.

Regarding the monthly-scale analysis (Section 3.5), we respectfully believe that it complements rather than duplicates the event-based analyses. The November 2023 and January 2024 case studies illustrate the physical mechanisms associated with aerosol–radiation interactions during representative pollution episodes, whereas the monthly simulations demonstrate that the effects of the revised refractive indices persist over longer timescales relevant to continuous forecasting applications. Following the reviewer's suggestion, the original Fig. 9 (Fig. S7 in the revised manuscript) and Table 3 (Table S10 in the revised manuscript) have been moved to the Supplementary Material, where they are now presented together with a comparison of the results from Domains 1 and 2. The salient findings from this analysis are briefly discussed and appropriately referenced in the revised manuscript.

Accordingly, we believe that retaining the multi-domain and monthly-scale analyses strengthens the manuscript by demonstrating the robustness of the results across model resolutions and by showing that the radiative and meteorological responses are not limited to isolated short-term events.

Specific comments

Title: Do you need both "interaction" and "feedback" in the title, and if so, I think "and" should be added in between?

Response: We thank the reviewer for this helpful suggestion. We agree that the simultaneous use of the terms "interaction" and "feedback" in the title could be redundant and may reduce clarity. Accordingly, we have revised the title by removing the word "feedback". The revised title is *"Improving aerosol-radiation interaction in the operational forecasting system - AIRWISE"* which more concisely reflects the scope of the study while avoiding potential ambiguity.

L15: Should specify whether SWDOWN is incoming/downward or net surface shortwave radiation

Response: We thank the reviewer for this comment. The word "downwelling" has been added in the revised manuscript to clearly indicate that SWDOWN refers to downward (incoming) shortwave radiation rather than net surface shortwave radiation.

L19-20: The ~0.2C decrease in surface temperature only occurs during very few hours in the morning, according to Figure 5, most of the day there is an increase.

Response: We thank the reviewer for this observation and agree that the maximum reduction in near-surface temperature (~0.2 °C) occurs only during a few daytime hours, particularly during the late-morning period, while slight temperature increases are observed during other parts of the day. This diurnal behaviour and its underlying mechanisms have been discussed in detail in Sect. 3.3 of the revised manuscript (line 465-474). However, the statement in the Abstract is intended to briefly summarize the magnitude of the meteorological response associated with the modified aerosol optical properties, together with the corresponding changes in near-surface wind speed, boundary layer height, and relative humidity.

L23 "total aerosol-induced reduction": Do you mean just ARI or is ACI also now included?

Response: We thank the reviewer for this comment. We confirm that the statement refers exclusively to aerosol–radiation interactions (ARI) and does not include aerosol–cloud interactions (ACI). The reported reduction in SWDOWN was evaluated under strictly clear-sky conditions, where all cloud-contaminated periods were removed from both the model simulations and the observations. To avoid ambiguity, we have revised the text to explicitly state that the reported aerosol-induced reduction corresponds to clear-sky ARI effects only.

L23-25: RMSE is a better measure, and this does not show as good improvement as the mean bias

Response: We thank the reviewer for this valuable suggestion. We agree that RMSE provides a more robust measure of model performance because it accounts for both the magnitude and variability of the errors and is not influenced by compensating periods of overestimation and underestimation. Accordingly, in addition to the mean bias, we have included a discussion of the RMSE in the revised manuscript in the result section (line 594-596). The results show that the RMSE is also reduced in the RI-ALL (previously written as EXP5) configuration compared to CTRL in both December and in January. While the improvement in RMSE is smaller than that in the mean bias (Table 1), both metrics consistently indicate improved simulation of surface shortwave radiation following the implementation of the revised aerosol optical properties.

Table 1. Statistical evaluation of SWDOWN (Wm^{-2}) from d01 simulations in December 2023 and January 2024 under clear-sky (non-foggy) conditions over Delhi.

S. No.	Month	MB (Wm^{-2})		RMSE (Wm^{-2})		NMSE		FB	
		CTRL	**RI-ALL**	**CTRL**	**RI-ALL**	**CTRL**	**RI-ALL**	**CTRL**	**RI-ALL**
1.	**December**	10.53	3.96	37.54	34.01	0.06	0.05	0.07	0.02
2.	**January**	23.63	15.26	44.94	39.35	0.08	0.06	0.15	0.10

L28: earth -> Earth

Response: We thank the reviewer for pointing this out. The suggested correction has been incorporated in the revised manuscript.

L29: Could also add a reference to a more recent estimate, such as IPCC AR6 Ch. 7:

Forster, P. M., T. Storelvmo, et al. (2021), The Earth's Energy Budget, Climate Feedbacks, and Climate Sensitivity, in Climate Change 2021: The Physical Science Basis. Contribution of Working Group I to the Sixth Assessment Report of the Intergovernmental Panel on Climate Change, edited by V. Masson-Delmotte, et al., pp. 923–1054, Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA.

Response: We thank the reviewer for this helpful suggestion. We agree that a more recent assessment is appropriate in this context. Accordingly, we have included the recommended reference from the IPCC Sixth Assessment Report (Forster et al., 2021) in the revised manuscript to complement the existing references and provide an updated perspective on the Earth's energy budget and radiative forcing.

L30: I would have added "and climate" after "weather"

Response: We thank the reviewer for this suggestion. The suggested modification has been incorporated in the revised manuscript (line 30).

L36: Should add a reference after this statement (after "air quality"). For instance one of these:

Sharma, A., C. Venkataraman, K. Muduchuru, V. Singh, A. Kesarkar, S. Ghosh, and S. Dey (2023), Aerosol radiative feedback enhances particulate pollution over India: A process understanding, Atmospheric Environment, 298, 119609, doi: https://doi.org/10.1016/j.atmosenv.2023.119609.

Hodnebrog, Ø., K. Aunan, S. Chowdhury, L. Marelle, G. Myhre, C. W. Stjern, and S. Wang (2025), Strong reduction in near-surface turbulence due to aerosols in South and East Asia, npj Clean Air, 1(1), 9, doi: 10.1038/s44407-025-00009-6.

Response: We thank the reviewer for this helpful suggestion. We agree that this statement would benefit from support from recent literature demonstrating the influence of aerosol radiative effects on boundary-layer dynamics and air quality. Accordingly, we have added the suggested references of Sharma et al. (2023) and Hodnebrog et al. (2025) in the revised manuscript.

L53: "mitigating" may be a better word than "regulating"

Response: We thank the reviewer for this suggestion. We have modified the sentence in the revised manuscript.

L62: WRF-Chem has not been introduced/defined yet

Response: We thank the reviewer for pointing this out. We have revised the manuscript to introduce WRF-Chem as the Weather Research and Forecasting model coupled with Chemistry (WRF-Chem) in line 57-58 in the revised manuscript before its subsequent use in the text.

L65: I would say also ACI in addition to ARI? Twomey effect, which is mentioned, is ACI.

Response: We thank the reviewer for this observation. We agree that the preceding discussion includes aerosol–cloud interaction (ACI) processes, such as the Twomey effect. Accordingly, the text has been revised to explicitly refer to both aerosol–radiation interactions (ARI) and aerosol–cloud interactions (ACI) in the context of improving their representation in NWP models.

L78: Should define PM2.5

Response: We thank the reviewer for pointing this out. The manuscript has been revised to define $PM_{2.5}$ as particulate matter with an aerodynamic diameter ≤ 2.5 µm in line 79-80 in the revised manuscript.

L78-85: Could mention that dust emissions/concentrations/forcing are very uncertain and differ a lot between different models, e.g.,

Wu, C., Lin, Z., and Liu, X.: The global dust cycle and uncertainty in CMIP5 (Coupled Model Intercomparison Project phase 5) models, Atmos. Chem. Phys., 20, 10401–10425, https://doi.org/10.5194/acp-20-10401-2020, 2020

Haugvaldstad, O. W., Olivié, D., Storelvmo, T., and Schulz, M.: Dust radiative forcing in CMIP6 Earth System models: insights from the AerChemMIP piClim-2xdust experiment, Atmos. Chem. Phys., 25, 13199–13219, https://doi.org/10.5194/acp-25-13199-2025, 2025

Response: We thank the reviewer for this valuable suggestion. We agree that uncertainties in dust emissions, concentrations, and radiative forcing constitute an important source of variability in aerosol-climate and aerosol-radiation interaction studies. Accordingly, we have expanded the discussion in line 84-88 in the revised manuscript to acknowledge that dust-related processes remain highly uncertain and can differ substantially among models due to differences in emission parameterizations, size distributions, mineralogical composition, transport, and deposition processes. We have also incorporated the suggested references of Wu et al. (2020) and Haugvaldstad et al. (2025), which highlight the large inter-model spread in simulated dust burdens and dust radiative forcing in CMIP5 and CMIP6 Earth System Models, respectively.

L125-126: I find "air quality" a bit superfluous when the following part of the sentence states "regional and urban-scale pollution"

Response: We thank the reviewer for this observation. We agree that the term "air quality" is somewhat redundant in this context, as the following phrase already refers to regional and urban-scale pollution. Accordingly, we have revised the sentence to improve clarity and conciseness by removing the phrase "air quality".

L126-128: Should also mention sulfate and primary organic aerosols

Response: We thank the reviewer for this suggestion. As in this study, we have updated the refractive indices of aerosol species for which region-specific or observation-based optical properties are available and differ substantially from the default values implemented in WRF-Chem. Accordingly, the refractive indices of mineral dust, black carbon, and primary organic carbon aerosols were revised based on observational and literature-derived values. In contrast, the refractive indices of sulfate aerosols used in WRF-Chem are already broadly consistent with values reported in the literature, and therefore no modification was applied.

L136 / Figure S1: The caption mentions a star indicating the location of Delhi, but I cannot find a star

Response: We thank the reviewer for noting this oversight. A star symbol has now been added to Fig. S1 to clearly indicate the location of Delhi, and the figure caption has been updated accordingly.

L140 "within the first kilometres": How many kilometres?

Response: We thank the reviewer for pointing this out. We agree that the phrase "within the first kilometres from the surface" was ambiguous. The manuscript has been revised to explicitly state that 8 model levels are located within the lowest one km above ground level to better resolve boundary-layer processes.

L151-153: I assume the convective parameterization is not applied in domain 2? Convection is typically thought to be fairly resolved at grid spacings <4 km (Prein et al.,

2015), and it has even proved beneficial to turn off the convection parameterization at grid spacings lower than around 25 km (Vergara-Temprado et al., 2020).

Prein, A. F., W. Langhans, G. Fosser, A. Ferrone, N. Ban, K. Goergen, M. Keller, M. Toelle, O. Gutjahr, F. Feser, E. Brisson, S. Kollet, J. Schmidli, N. P. M. van Lipzig, and R. Leung (2015), A review on regional convection-permitting climate modeling: Demonstrations, prospects, and challenges, Rev. Geophys., 53(2), 323-361, doi: 10.1002/2014rg000475.

Vergara-Temprado, J., N. Ban, D. Panosetti, L. Schlemmer, and C. Schar (2020), Climate Models Permit Convection at Much Coarser Resolutions Than Previously Considered, J. Clim., 33(5), 1915-1933, doi: 10.1175/jcli-d-19-0286.1.

Response: We thank the reviewer for this important comment. The Grell–Freitas Ensemble scheme was applied in both domains, including the 2 km inner domain. We acknowledge that convection can be partially or explicitly resolved at convection-permitting resolutions (< 4 km), as discussed by Prein et al. (2015) and Vergara-Temprado et al. (2020). However, the Grell–Freitas scheme was specifically developed to provide a scale-aware representation of convection by progressively reducing the contribution of parameterized convection as the model grid spacing decreases (Grell and Freitas, 2014; Kwon and Hong, 2017). Therefore, unlike conventional convective parameterizations, the scheme allows a seamless transition between parameterized and resolved convection across nested domains of different resolutions. We have clarified this aspect in the revised manuscript (line 162-166) and added the relevant references.

References:

Grell, G. A., and Freitas, S. R. (2014). A scale and aerosol aware stochastic convective parameterization for weather and air quality modeling. Atmospheric Chemistry and Physics, 14(10), 5233-5250.

Kwon, Y. C., & Hong, S. Y. (2017). A mass-flux cumulus parameterization scheme across gray-zone resolutions. *Monthly Weather Review*, *145*(2), 583-598.

L162-164: Did the simulations use nudging (grid or spectral) towards the ERA5 data?

Response: We thank the reviewer for this question. No grid nudging or spectral nudging towards the ERA5 reanalysis data was applied in the simulations. ERA5 was used solely to provide the initial and lateral boundary conditions for the outer domain (d01), after which the model evolved freely. To clarify this point, we have revised the manuscript in Sect. 2.1.1 (line 176-180) and explicitly stated that the simulations were performed without any nudging towards ERA5 during model integration.

L166-169: I think this part can be skipped, I believe it is described in the WRF documentation.

Response: We thank the reviewer for this suggestion. We agree that this information is already well documented in the WRF-Chem documentation and is not directly relevant to the objectives of the present study. Accordingly, this part has been removed from the revised manuscript to improve conciseness and readability.

L175-176: How was this refining of emissions done? What source of fine-gridded emissions data for Delhi was used?

Response: We thank the reviewer for this important comment. To clarify the methodology, we have expanded the description of anthropogenic emissions in the revised manuscript (line 189-196). The base anthropogenic emissions were obtained from the EDGAR-HTAPv2.2 inventory (2010) at 0.1° × 0.1° resolution. To better represent the strong spatial heterogeneity of emissions over the Delhi-NCR region, these emissions were refined using the High-resolution Delhi Emission Inventory (HrDEI) developed under the System of Air Quality and Weather Forecasting and Research (SAFAR) programme (Jena et al., 2021). The HrDEI is a bottom-up inventory available at 400 m spatial resolution and includes detailed sector-wise emissions from transportation, residential, industrial, construction, waste-burning, and power sectors. The high-resolution emissions were aggregated to the model grid using a mass-conserving approach, thereby preserving the total emitted mass while providing a more realistic representation of urban emission hotspots and spatial variability across Delhi-NCR. The manuscript has been revised accordingly to explicitly describe both the source of the fine-gridded emissions data and the emission refinement procedure.

Reference:

Jena, C., Ghude, S. D., Kumar, R., Debnath, S., Govardhan, G., Soni, V. K., ... & Rajeevan, M. (2021). Performance of high resolution (400 m) PM2.5 forecast over Delhi. *Scientific reports*, *11*(1), 4104.

L182-186: For this study focusing on aerosols, what was the reason for choosing the simple GOCART scheme and not a more detailed aerosol scheme? For instance the sectional aerosol scheme MOSAIC works well with the MOZART chemistry scheme.

Response: We thank the reviewer for this important comment. We agree that sectional aerosol schemes such as MOSAIC provide a more detailed representation of aerosol size distributions, mixing states, and aerosol chemistry than the bulk GOCART aerosol module.

The GOCART aerosol module was selected because it forms the operational aerosol framework of the AIRWISE forecasting system (Ghude et al., 2024) and provides a computationally efficient configuration for long-duration and real-time forecasting applications over India. In addition, the present work builds upon an existing operational WRF-Chem setup that has been extensively evaluated for air-quality forecasting over the region.

We acknowledge that the use of MOSAIC could lead to differences in simulated aerosol concentrations, size distributions, and aerosol optical properties owing to its more detailed treatment of aerosol composition and microphysical processes. Consequently, the quantitative magnitude of the simulated aerosol-radiation interactions may differ when using MOSAIC.

However, aerosol optical properties in WRF-Chem are ultimately calculated using aerosol size, composition, and complex refractive indices, which are then passed through the same Mie-scattering framework to derive extinction coefficients, aerosol optical depth, single-scattering albedo, and asymmetry parameters. Therefore, uncertainties associated with aerosol refractive indices are not specific to GOCART but are relevant across WRF-Chem aerosol modules, including MOSAIC.

To address this point, we have expanded the discussion in the revised manuscript (line 666-679) to acknowledge the limitations of the GOCART framework and to highlight the need for future investigations using more detailed aerosol schemes such as MOSAIC.

Reference:

Ghude, S. D., Govardhan, G., Kumar, R., Yadav, P. P., Jat, R., Debnath, S., ... & Rajeevan, M. (2024). Air quality warning and integrated decision support system for emissions (AIRWISE): enhancing air quality management in megacities. *Bulletin of the American Meteorological Society*, *105*(12), E2525-E2550.

L187: I would have split this subsection by renaming "experiment details" to e.g. "refractive indices" or "optical properties", and added a new subheading "experiments" to line 223.

Response: We thank the reviewer for this helpful suggestion. We agree that separating the discussion of aerosol optical properties from the experimental design improves the organization and readability of the manuscript. Accordingly, Sect. 2.2 has been renamed as Aerosol optical properties, which describes the revised refractive indices and associated optical parameters. In addition, Sect. 2.3 named as Experiment details, which outlines the design of the numerical experiments.

L191: What kind of aerosols are included in the "other GOCART primary PM2.5" and how are they treated?

Response: We thank the reviewer for this question. We agree that the description of the P25 aerosol species in the original manuscript was not sufficiently clear. In the GOCART aerosol module, P25 represents an un-speciated primary $PM_{2.5}$ category that accounts for other inorganic fine particulate matter not explicitly represented by the individual aerosol species (e.g., sulfate, BC, OC, dust, and sea salt). In the aerosol optical calculations of WRF-Chem, P25 is assigned the same refractive indices and optical properties as mineral dust and is therefore treated as a dust-like aerosol species when estimating aerosol radiative effects. To clarify this treatment, we have revised the manuscript accordingly (line 215-218).

L225-228: It would be useful with more informative names of experiments EXP1-EXP5. An alternative could be EXP2 -> RI_dust and similarly for other experiments.

Response: Thank you for this helpful suggestion. We agree that descriptive experiment names improve readability and make it easier for readers to follow the discussion. Accordingly, we have replaced the generic experiment labels (EXP1–EXP5) throughout the manuscript with more informative names that directly reflect the corresponding model configurations:

- No-ARF – No aerosol–radiation feedback
- RI-DUST – Modified refractive index of dust only
- RI-BC – Modified refractive index of black carbon (BC) only
- RI-OC – Modified refractive index of organic carbon (OC) only
- RI-ALL – Modified refractive indices of dust, BC, OC, and P25

These revised experiment names have been incorporated throughout the manuscript, including the methodology, results, figures, and tables.

L248-249: Are 2 days enough for the chemistry to spin up, given the coarse resolution of MOZART initial conditions?

Response: We thank the reviewer for this important comment. We acknowledge that the required chemistry spin-up period depends on the model configuration, domain size, chemical mechanism, and scientific objective. In the present study, Event-I and Event-II are short-duration, high-pollution case studies spanning approximately 10 days, and the primary objective is to evaluate the sensitivity of aerosol–radiation interactions to modified aerosol refractive indices rather than to investigate long-term chemical evolution. A spin-up period of two days was therefore applied to allow the meteorological fields and MOZART-derived chemical initial conditions to adjust to the model dynamics, transport processes, and emissions before the analysis period.

Previous WRF-Chem studies over India and South Asia have similarly employed relatively short spin-up periods for episodic air-pollution simulations and process-oriented analyses (e.g., Jena et al., 2021; Agarwal et al., 2024), while substantially longer spin-up periods are generally adopted for seasonal and long-term chemistry simulations. Consistent with this practice, the two-day spin-up in our study was applied only to the short event-based simulations. For the extended four-month simulations (September 2023–January 2024), a significantly longer spin-up period of 10 days was used to ensure a more complete adjustment of meteorological and chemical fields.

References:

Agarwal, P., Stevenson, D. S., & Heal, M. R. (2024). Evaluation of WRF-Chem-simulated meteorology and aerosols over northern India during the severe pollution episode of 2016. *Atmospheric Chemistry and Physics*, *24*(4), 2239-2266.

Jena, C., Ghude, S. D., Kumar, R., Debnath, S., Govardhan, G., Soni, V. K., ... & Rajeevan, M. (2021). Performance of high resolution (400 m) PM2.5 forecast over Delhi. *Scientific reports*, *11*(1), 4104.

L249-250: I did not quite understand the reasoning for only comparing against the outer (10 km) domain and not the inner (2 km) domain. I would think high resolution is important to get a more realistic representation of aerosol concentrations (PM2.5), especially since there may be many local emission sources around Delhi, which is presumably why the authors have chosen to refine the anthropogenic emissions in this region. Is the choice related to lack of observations in the Delhi region? I would think not, because later on it is described that 40 monitoring stations across Delhi are used. On a related note, do the simulations use one-way or two-way nesting? If two-way nesting is used, the domain 1 results would at least benefit from the domain 2 results in regions where they overlap.

Response: We thank the reviewer for this thoughtful comment. We agree that higher spatial resolution can improve the representation of local emission gradients and urban-scale variability in aerosol concentrations. However, the primary objective of this study is to quantify the sensitivity of aerosol–radiation interactions to revised aerosol refractive indices rather than to investigate the impact of model resolution.

To address this concern, we have included a comparison of the four-month simulations (September 2023–January 2024) from both the outer domain (d01; 10 km) and the inner domain (d02; 2 km) in the Supplementary Material (Table S7 and S8). The results show that the changes in SWDOWN associated with the modified aerosol optical properties are very similar in both domains, indicating that the key conclusions of this study are largely independent of the horizontal resolution. This is expected because the analysis focuses on the domain-averaged response over Delhi rather than on individual grid cells or localized emission hotspots.

Furthermore, the evaluation was not constrained by the availability of observations. For $PM_{2.5}$, observations from 40 monitoring stations distributed across Delhi were averaged and compared with model values averaged over the Delhi region. However, SWDOWN and meteorological variables were evaluated using observations from the Winter Fog EXperiment (WiFEX) site at Indira Gandhi International (IGI) Airport, Delhi. Therefore, the analysis was designed to assess the city-scale response to modified aerosol optical properties rather than local-scale variability.

The event-based analyses (Event-I and Event-II) were presented using d01 because the results from d01 and d02 are found to be very similar, while d01 is computationally more efficient and directly relevant for operational forecasting applications. Since one of the motivations of this work is to improve aerosol–radiation interactions within the AIRWISE forecasting framework, demonstrating the impacts at the operational model resolution is particularly important. The added d01–d02 comparison in the Supplementary Material provides evidence that the conclusions regarding the sensitivity of aerosol–radiation interactions to refractive indices remain robust across both model resolutions.

L283-285: It would be useful to know what the height of the observations versus the height of the lowermost model layer is, which I presume has been used for the comparison.

Response: We thank the reviewer for this useful suggestion. The $PM_{2.5}$ observations used in this study were obtained from the surface monitoring networks operated by the Central Pollution Control Board (CPCB) and the Delhi Pollution Control Committee (DPCC). These measurements represent near-surface ambient $PM_{2.5}$ concentrations recorded at monitoring stations located a few metres above ground level. For the model evaluation, we used the $PM_{2.5}$ concentrations from the lowest model layer, which is located approximately 2 m above the surface. Thus, the comparison was performed between near-surface observations and the corresponding near-surface model concentrations. We have clarified this information in the revised manuscript (line 331-332).

L300-302: I do not understand what is meant by "removed the cloud cover (set > 0.1)". So you did not use the clear-sky fluxes but rather ignored the hours where the model has a cloud fraction >0.1, is that what is meant? I guess there would be times where the model shows cloudy conditions and the observations cloud-free, and vice versa, how is that treated in the comparison?

Response: We thank the reviewer for this important comment and apologize for the lack of clarity in the original text. We did not use model-generated clear-sky radiation fluxes, as WRF-Chem does not provide a separate clear-sky SWDOWN diagnostic output. Instead, cloudy and foggy periods were identified and excluded from both the model simulations and the observations prior to the comparison.

For the model simulations, only those hours were retained for which the cloud fraction (CLDFRA) remained below 0.1 throughout the entire atmospheric column. Thus, any hour containing significant cloud cover (CLDFRA > 0.1) at any model level was excluded from the analysis. For the observations, cloudy and fog-affected periods were identified using a ceilometer (installed at IGI Airport, Delhi.) backscatter measurements and associated cloud detection products and were subsequently removed. The SWDOWN evaluation was then restricted to periods classified as cloud-free in both the model and observational datasets. Consequently, hours for which the model indicated cloud cover but the observations were cloud-free, or vice versa, were excluded from the comparison. We have revised the manuscript (line 348-355) to explicitly describe this filtering procedure and clarify the definition of clear-sky conditions used in this study.

Figure 2: I cannot see the line for the result with the Goddard scheme, as it is hidden underneath the CAM line. Perhaps reducing the line thickness would help. The striking similarity between the two schemes makes me wonder how different these two parameterizations really are. Is the similarity just by chance or do the two schemes share common features?

Response: We thank the reviewer for this insightful observation. We agree that the Goddard scheme curve was difficult to distinguish from the CAM scheme curve in the original figure because the simulated SWDOWN values from the two schemes are nearly identical. The corresponding statistical metrics for all three schemes are provided in Table S3 of the Supplementary Material which shows that there are still some differences in simulated SWDOWN by Goddard and CAM schemes; they are not exactly identical. To improve clarity, we have revised Fig. 2 by reducing the line thickness and modifying the line styles so that the results from all three schemes are readily distinguishable.

We further note that the close agreement between the CAM and Goddard schemes over Delhi during the study period does not necessarily imply that the two parameterizations are identical. A comprehensive assessment of the similarities and differences between these schemes would require a dedicated investigation of their radiative transfer formulations, spectral discretizations, and the treatment of gaseous absorption, aerosols, and clouds.

L312-313: Was this the case also for the other modelled months?

Response: We thank the reviewer for this question. The comparison between the RRTMG, Goddard, and CAM radiation schemes was conducted only for December 2023 and was intended to identify the most suitable shortwave radiation parameterization for the subsequent sensitivity experiments. The results showed that all three schemes overestimated SWDOWN relative to the observations, but the overestimation was substantially smaller for RRTMG, particularly during the afternoon hours. In contrast, both the Goddard and CAM schemes exhibited larger positive biases throughout most of the day, with biases approximately twice those of RRTMG. This behaviour was consistently observed throughout the simulation period of December 2023 and is reflected in the monthly mean diurnal cycle presented in Fig. 2. Based on this evaluation, RRTMG was selected for all subsequent experiments. Since the objective was to identify an appropriate radiation scheme rather than perform a seasonal intercomparison of radiation parameterizations, similar tests were not conducted for the other modelled months.

L320-323: Could the underestimation be related to missing components in GOCART, notably SOA and nitrate aerosols? Also, if I understand correctly, the comparison is done using the 10 km resolution results – have you checked if the results were better in 2 km?

Response: We thank the reviewer for this insightful comment. We agree that the underestimation of $PM_{2.5}$ may partly arise from limitations in the GOCART aerosol module. In particular, GOCART does not explicitly represent secondary organic aerosols (SOA) and nitrate aerosols, both of which can contribute significantly to wintertime $PM_{2.5}$ concentrations over Delhi. Consequently, some fraction of the observed $PM_{2.5}$ mass may not be fully captured in the simulations. In addition, previous studies have shown that the AIRWISE forecasting system tends to over-ventilate pollutants during daytime hours, leading to lower simulated $PM_{2.5}$ concentrations, especially during severe winter pollution episodes (Sengupta et al., 2022; Ghude et al., 2024).

Regarding the model resolution, we agree that the 2 km domain can better represent local emission gradients and urban-scale variability than the 10 km domain. We examined the corresponding simulations in the inner domain (d02) and found some quantitative differences relative to the outer domain (d01), reflecting the influence of higher spatial resolution. However, the overall response of $PM_{2.5}$, SWDOWN, and the sensitivity to the modified aerosol optical properties remained broadly consistent between the two domains. Therefore, although the magnitude of the changes differs somewhat, the main conclusions regarding the impacts of revised aerosol refractive indices are not altered by the choice of model resolution.

References:

Ghude, S. D., Govardhan, G., Kumar, R., Yadav, P. P., Jat, R., Debnath, S., ... & Rajeevan, M. (2024). Air quality warning and integrated decision support system for emissions (AIRWISE): enhancing air quality management in megacities. *Bulletin of the American Meteorological Society*, *105*(12), E2525-E2550.

Sengupta, A., Govardhan, G., Debnath, S., Yadav, P., Kulkarni, S. H., Parde, A. N., ... & Ghude, S. D. (2022). Probing into the wintertime meteorology and particulate matter (PM2. 5 and PM10) forecast over Delhi. Atmospheric Pollution Research, 13(6), 101426.

Figure S2: Does the standard deviation represent the spread between different station locations?

Response: We thank the reviewer for this question. Yes, the standard deviation shown in Fig. S2 represents the spatial variability within the Delhi region. For the observations, it corresponds to the standard deviation across the 40 CPCB and DPCC monitoring stations used in the analysis. For the model, it corresponds to the standard deviation across all model grid cells located within Delhi. To avoid ambiguity, we have clarified this in the figure caption (Fig. S2 in supplementary).

L329: Remove "down"

Response: We thank the reviewer for pointing this out. The word 'down' has been removed from the revised manuscript.

L335 "This is partly related to": I do not understand what it is related to, I think the sentence needs to be rewritten.

Response: We thank the reviewer for pointing this out. We agree that the phrase "This is partly related to" was ambiguous and did not clearly convey the intended meaning. Accordingly, we have rewritten the sentence *"This is partly related to the inability of the model to capture foggy conditions due to a bias in RH (Sengupta et al., 2022), and consequently the underestimated hygroscopic growth of aerosols in the model, which limits the magnitude of simulated* $PM_{2.5}$*."* as *"The underestimation is likely associated with the model's difficulty in reproducing the observed fog and near-saturated conditions because of a negative RH bias (Sengupta et al., 2022). Lower simulated RH can limit aerosol hygroscopic growth and suppress aqueous-phase processing, secondary aerosol formation, and gas-particle partitioning, thereby contributing to lower* $PM_{2.5}$ *concentrations during foggy periods."* in the revised manuscript (line 392-395).

L353-354: The very small bias in EXP5 is caused by the underestimation in the latter part of the period compensating for the overestimation in the first part of the period. The RMSE is a better measure, and this changes only very little, so I would at least remove "substantially" from the sentence.

Response: We thank the reviewer for this insightful comment. We have revised the text by removing the word *"substantially"* and clarifying that the improvement in the agreement between the simulated and observed SWDOWN is reflected primarily in the reduced mean bias, whereas the improvement in RMSE is relatively modest (line 411-413 in the revised manuscript).

Figure 3: It is difficult to see the difference between the lines, especially in Fig. 3a. To improve readability, I suggest removing the nighttime hours (when SWDOWN is zero) and the hours that have been screened out.

Response: We thank the reviewer for this helpful suggestion. We agree that the inclusion of nighttime hours, when SWDOWN is zero, reduces the visibility of the differences among the simulations. Accordingly, Fig. 3 has been revised to improve readability by displaying only the daytime hours relevant to SWDOWN. In addition, hours excluded by the cloud-screening procedure are no longer shown. The revised figure more clearly highlights the differences between the simulations and facilitates comparison of the impacts of the modified aerosol optical properties.

L358-361: I think 4b should be 4c, and 4c should be 4b

Response: Thank you for noting this error. The references to Fig. 4b and Fig. 4c have been corrected in the revised manuscript.

L365 and L378: I do not think "rd" and "th" should be added after the fractions

Response: We thank the reviewer for pointing this out. We have made the corresponding corrections.

L368-369: It would have been useful with some information on the composition of PM2.5 in the CTRL simulations, e.g. in the supplementary. This would help understand if the small impact on SWDOWN from dust is indeed due to the modifications of optical properties or if it is due to low dust concentrations.

Response: We thank the reviewer for this valuable suggestion. We agree that information on the simulated $PM_{2.5}$ composition helps to interpret the relative contributions of different aerosol species to the radiative effects. Accordingly, we have added the $PM_{2.5}$ composition from the CTRL and RI-DUST simulation in the Supplementary Material (Fig. S4). The analysis shows that mineral dust contributes only a relatively small fraction of the total $PM_{2.5}$ mass during the post-monsoon and winter periods considered in this study. This is consistent with the seasonal meteorological conditions over Delhi, which are characterized by weaker winds, increased soil moisture in the upwind regions, and higher humidity compared to the pre-monsoon season, thereby limiting the long-range transport of mineral dust from the Thar Desert and Arabian regions. Therefore, the relatively small impact of the modified dust optical properties on SWDOWN is attributable not only to the optical property changes themselves but also to the comparatively low dust concentrations during the study period. We have clarified this point in the revised manuscript (line 433-437) and included the $PM_{2.5}$ composition in the Supplementary Material for reference.

L371-372: It is a bit confusing to understand which numbers belong to BC/OC and Event-I/II.

Response: We thank the reviewer for pointing this out. We agree that the original sentence was ambiguous and made it difficult to identify which values corresponded to BC and OC, and to Event-I and Event-II. Accordingly, we have revised the text to explicitly state the impacts for each aerosol species and event separately, thereby improving clarity and readability (line 431-433 in the revised manuscript).

L396: shows -> show

Response: We thank the reviewer for pointing out this typographical error. The suggested correction has been incorporated in the revised manuscript.

L400-402: The temperature reduction occurs just a few hours in the morning, while there is an increase in the rest of the day. Is there an explanation for this temperature increase?

Response: We thank the reviewer for this insightful comment. We agree that the reduction in T2 is primarily confined to the morning hours, while slight temperature increases are observed during other parts of the day. To clarify this behaviour, we have expanded the discussion in the revised manuscript. The reduction in SWDOWN associated with the more absorbing aerosol mixture tends to cool the surface. However, the modified aerosol optical properties also reduce the planetary boundary layer height (PBLH), thereby suppressing turbulent mixing and confining heat within a shallower near-surface layer. This dynamical response partially offsets the radiative cooling and can lead to small increases in near-surface temperature during certain periods of the day. We have revised the text accordingly to explain the competing radiative and boundary-layer processes governing the simulated temperature response (line 465-474 in the revised manuscript).

Figure 5: I cannot see the black line (observations) for PBLH

Response: We thank the reviewer for pointing this out. There were no observational measurements of PBLH available for the study period at the WiFEX site. Therefore, the PBLH panel presents only the model-simulated values from the CTRL and RI-ALL experiment, and no observational data are shown. We have revised the corresponding text (line 492 in the revised manuscript) to explicitly state that observational PBLH data were not available, thereby avoiding any confusion regarding the absence of a black observational line in the figure.

L416-417: It is good that the wind speeds are improving compared to observations, but I think it is also worth mentioning, in the previous discussion on temperature, that T2 gets slightly worse with the updated optical properties (RMSE increases from 1.43C to 1.59C).

Response: We thank the reviewer for this valuable observation. We agree that the reduction in T2 is primarily confined to the early morning hours, while slight temperature increases occur during other parts of the day. To address this point, we have expanded the discussion in the revised manuscript to explain the competing processes controlling the temperature response. While the reduction in SWDOWN associated with the more optically thick aerosol mixture tends to cool the surface, the simultaneous decrease in PBLH may reduce turbulent mixing and modify the redistribution of heat within the near-surface layer, partially offsetting the radiative cooling. Consequently, the net impact on T2 remains modest, with differences generally within ±0.3 °C throughout the day. We have also included the corresponding RMSE values (1.43 °C for CTRL and 1.59 °C for EXP5) to provide a quantitative assessment of model performance. Although the modified aerosol optical properties do not improve the T2 statistics, the analysis helps to elucidate the physical pathways through which aerosol–radiation interactions influence near-surface meteorology.

L417-418: But the reduction of temperature only occurs for a few hours, it is mostly an increase, so this probably does not explain the reduction of boundary layer turbulence?

Response: We thank the reviewer for this insightful comment and agree that the relatively smaller reduction in near-surface temperature alone cannot account for the sustained decrease in boundary-layer turbulence. We have therefore revised the discussion and added new analyses of the vertical structure of temperature, turbulent kinetic energy (TKE), and the gradient Richardson number (Figs. 6 and S5). These diagnostics show that, despite the modest surface cooling, enhanced aerosol extinction reduces surface heating while producing a warmer layer aloft, thereby increasing atmospheric stability (higher Richardson number) and suppressing buoyancy-driven turbulence (lower TKE). The resulting reduction in turbulent mixing leads to a shallower planetary boundary layer and weaker ventilation, providing a physically consistent explanation for the enhanced $PM_{2.5}$ concentrations. The revised text now reflects this mechanism explicitly (line 497-519 in the revised manuscript).

L420-421: Most of the afternoon hours show 100-200 m difference, so I think it should be specified that the 200-300 m reductions are for hours 17-18 IST only.

Response: We thank the reviewer for this observation. We agree that the largest reductions in PBLH, approaching 200–300 m, occur during the late afternoon hours, particularly around 17–18 IST. However, our intention was to indicate the maximum magnitude of the reduction during the afternoon period through the phrase "as high as 200–300 m", rather than imply that reductions of this magnitude persist throughout the entire afternoon. Since the wording may be interpreted differently, we have revised the text to explicitly state that the largest reductions occur during the late afternoon hours, while smaller reductions are present during the remainder of the afternoon period (line 492-494 in the revised manuscript).

L424-426: I would add "in the morning hours" after "Event-I"

Response: We thank the reviewer for this suggestion. The text has been revised to indicate that the reduction in near-surface temperature during Event-I occurs primarily in the morning hours (line 526-527 in the revised manuscript).

L431-432: I would add "broadly" before "similar" as there are quite some differences

Response: We thank the reviewer for this suggestion. We have revised the text by replacing "similar" with "broadly similar" to more accurately reflect the comparison (line 534 in the revised manuscript).

Figure 6: There do not seem to be observations in this plot, so I am wondering at what location/region do the values represent? Same as in Fig. 5, whole domain 2, or something else? If same as in Fig. 5, I would suggest including the Fig. 6a-b plots in Fig. 5 and Fig. 6c-d plots in Fig. S4, instead of showing this as a separate figure.

Response: We thank the reviewer for this helpful suggestion. We agree that Fig. 6 could be better integrated with the related analyses. Accordingly, Fig. 6a–b have been incorporated into Fig. 5, while Fig. 6c–d have been moved to the Supplementary Material in Fig. S6. We have also revised the figure captions to clarify that the quantities shown are averaged over the Delhi region under clear-sky conditions. However, unlike the meteorological variables presented in Fig. 5, the ventilation coefficient is a derived diagnostic calculated from the model-simulated PBLH and wind speed fields, and corresponding observations for PBLH are not available for evaluation.

L449: add "of" before "aerosols"

Response: We thank the reviewer for pointing this out. The word "of" has been added before "aerosols" in the revised manuscript (line 552).

L462-466: It is good to include a schematic of the processes, but I feel that some of the literature on the topic should be acknowledged. For instance Ding et al. (2016) describes the enhanced air pollution through aerosol-PBL interactions due to absorbing aerosols and shows a similar schematic. And the Sharma et al. (2023) paper mentioned previously is highly relevant for this study.

Ding, A. J., X. Huang, W. Nie, J. N. Sun, V.-M. Kerminen, T. Petäjä, H. Su, Y. F. Cheng, X.-Q. Yang, M. H. Wang, X. G. Chi, J. P. Wang, A. Virkkula, W. D. Guo, J. Yuan, S. Y. Wang, R. J. Zhang, Y. F. Wu, Y. Song, T. Zhu, S. Zilitinkevich, M. Kulmala, and C. B. Fu (2016), Enhanced haze pollution by black carbon in megacities in China, Geophys. Res. Lett., 43(6), 2873-2879, doi: https://doi.org/10.1002/2016GL067745.

Response: We thank the reviewer for this valuable suggestion. We agree that the processes illustrated in Fig. 8 are closely related to the aerosol–boundary layer feedback mechanisms discussed in previous studies. Accordingly, we have expanded the discussion and added references to Ding et al. (2016) and Sharma et al. (2023). These studies demonstrate how enhanced aerosol absorption can suppress boundary-layer development, reduce atmospheric ventilation, and promote the accumulation of pollutants near the surface, thereby strengthening aerosol–meteorology feedback. While the schematic in the present study is developed based on the processes identified in our simulations, we now acknowledge its consistency with the conceptual framework proposed in these earlier studies and have cited the relevant literature in the revised manuscript (line 617-620).

L471-474: Are these simulations different from what was used in sections 3.1-3.4?

Response: We thank the reviewer for this question. Yes, the simulations discussed in this section are different from those used in Sections 3.1–3.4. While Sections 3.1–3.4 focus on the two high-pollution case studies (Event-I and Event-II), this section presents results from the long-term simulations conducted from 20 September 2023 to 31 January 2024 with a 10-day spin-up period. These simulations were specifically designed to assess the impacts of the revised aerosol optical properties over an extended period encompassing a broader range of meteorological and air-quality conditions. We have already clarified this distinction in Sect. 2.3 in the revised manuscript.

L474-476: What is the reason for showing domain 1 results? Unless the point is to discuss differences in results between resolutions, which I sense it is not, then I would replace Figure 9 with Figure S5 showing the domain 2 results. They look anyway very similar.

Response: We thank the reviewer for this helpful suggestion. We agree that the primary objective of this section is not to investigate the sensitivity of the results to model resolution and that the simulations over Domains 1 and 2 exhibit very similar behavior. Accordingly, and consistent with the reviewer's suggestion above, the original Fig. 9 (Fig. S7 in the supplementary) and the associated Table 3 (Table S10 in the supplementary) showing the Domain 1 results have been moved to the Supplementary Material, where they are presented together with the corresponding Domain 2 results for completeness. The key findings from this comparison are now briefly discussed and appropriately referenced in the revised main text. The overall conclusions of the study remain unchanged.

L480-482: I cannot see that Table 2 shows any of this

Response: We thank the reviewer for pointing this out. We agree that the reference to Table 2 was incorrect. The information discussed in this section is presented in Table 1, not Table 2. Accordingly, we have corrected the table citation in the revised manuscript (line 590).

L488-489: It does not look like the changes due to RI of BC/OC are twice as high in November. Maybe close to twice as high as in October, but not for the other months.

Response: We thank the reviewer for this careful observation. We agree that the statement was too strong and could be interpreted as implying that the impacts of the revised BC and OC refractive indices are approximately twice as large in November compared to all other months. This is not the case. The largest impacts are observed during November, particularly when compared with October, but the differences relative to the other winter months are smaller. Accordingly, we have revised the text to more accurately describe the seasonal variation in the sensitivity of SWDOWN to the modified BC and OC optical properties and to avoid overstating the magnitude of the November response.

L496-497: Again, it would be useful to know if one-way or two-way nesting was used. If two-way nesting, then the results in domain 1 and 2 should be the same.

Response: We thank the reviewer for this comment. We apologize for not explicitly stating the nesting configuration in the original manuscript. In this study, a one-way nesting approach was used, whereby Domain-2 (2 km resolution) was dynamically downscaled from Domain-1 (10 km resolution) without feedback from the child domain to the parent domain. We have now clarified this in the revised manuscript.

L500-506: I suggest discussing RMSE instead of the mean bias. If it is as in section 3.2, different periods of overestimation and underestimation compensate, which leads to a very small mean bias. The RMSE accounts for this, and in Table S9 it shows that the RMSE reduction is not nearly as large as the reduction in mean bias.

Response: We thank the reviewer for this valuable suggestion. We agree that RMSE provides a more robust measure of overall model performance because it is not affected by compensating periods of overestimation and underestimation. Accordingly, we have revised the discussion to include both the changes in mean bias and RMSE. While the mean SWDOWN bias decreased from 10.53 to 3.96 W m^{-2} in December and from 23.63 to 15.26 W m^{-2} in January, the RMSE also showed improvements, decreasing by approximately 9.4 % in December and 12.4 % in January. Thus, although the reduction in RMSE is more modest than that in the mean bias, both metrics consistently indicate an improvement in the simulation of surface shortwave radiation following the revision of aerosol optical properties. The manuscript has been revised accordingly (line 594-596).

L522: Remove "thus"

Response: We thank the reviewer for pointing this out. We have removed the word "Thus" in the revised manuscript (line 627).

L524-525: I do not think the surface temperature change is a main driver here. Since the modifications to the refractive indices make the aerosols more absorbing, I would expect the vertical temperature profile to change and make the atmosphere more stable. This would involve weaker winds and shallower boundary layer. See for instance the Ding et al. (2016) paper mentioned previously or Stjern et al.:

Stjern, C. W., Ø. Hodnebrog, G. Myhre, and I. Pisso (2023), The turbulent future brings a breath of fresh air, Nature Communications, 14(1), 3735, doi: 10.1038/s41467-023-39298-4.

Response: We thank the reviewer for this insightful comment and agree that changes in surface temperature alone cannot explain the reductions in wind speed and planetary boundary layer height. In response, we have revised the manuscript to explicitly examine the vertical dynamical response of the boundary layer.

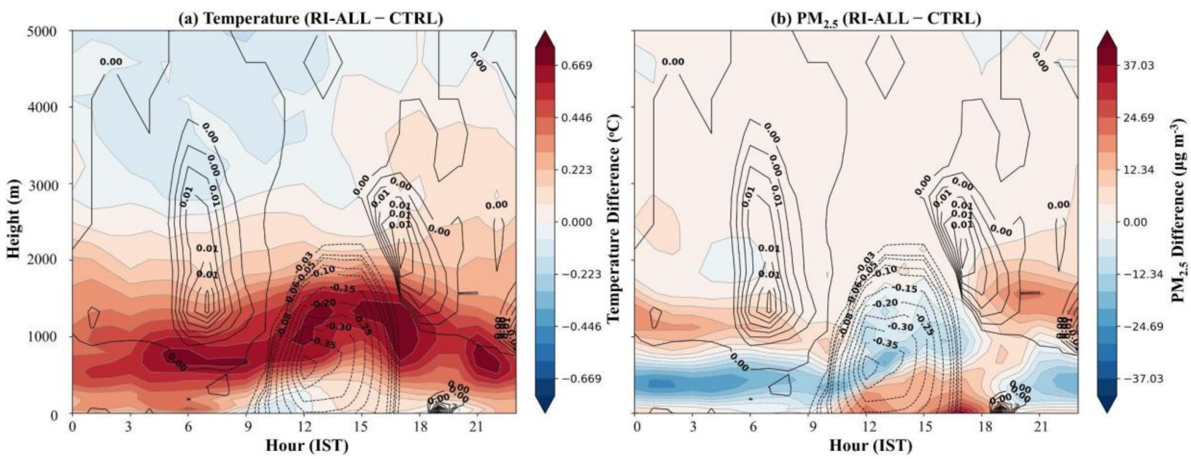

Fig. 1. Mean diurnal time–height cross-sections over Delhi showing the differences in (a) temperature (°C) and (b) $PM_{2.5}$ concentration (μg m^{-3}) between the RI-ALL and CTRL simulations during Event-I. Filled contours represent the differences (RI-ALL − CTRL) in temperature and $PM_{2.5}$ concentration. Black contours denote differences in turbulent kinetic energy (TKE; m^2 s^{-2}), with solid contours indicating positive differences and dashed contours indicating negative differences

We have added time–height cross-sections of temperature, $PM_{2.5}$ and turbulent kinetic energy (TKE) differences (new Fig. 6), together with the corresponding gradient Richardson number diagnostics (Fig. S5). The new analyses show that the enhanced aerosol absorption in the RI-ALL simulation modifies the vertical heating profile, producing reduced heating near the surface and warming aloft (approximately 700–1500 m AGL). This increases atmospheric stability, as evidenced by the larger Richardson numbers in RI-ALL relative to CTRL and the concomitant daytime reduction in TKE (−0.20 to −0.35 m^2 s^{-2}) within the lower 1.5 km (Fig. 2). The suppressed turbulence inhibits the growth of the daytime mixed layer, leading to a shallower PBL, weaker near-surface winds, and reduced ventilation, thereby favoring the accumulation of $PM_{2.5}$ near the surface. We have revised the discussion accordingly and included references to Stjern et al. (2023) and Ding et al. (2016), which describe analogous stability-driven aerosol–boundary layer feedback mechanisms associated with absorbing aerosols.

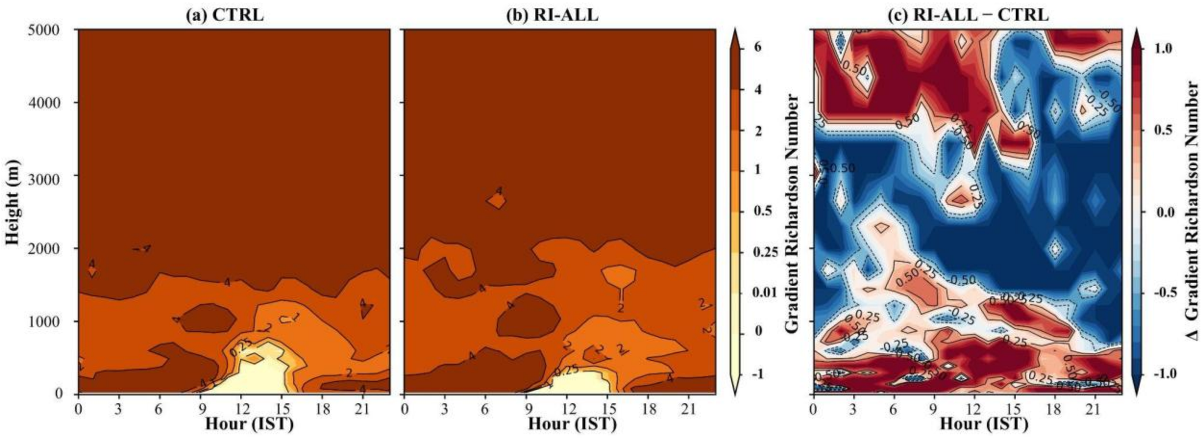

Fig. 2. Mean diurnal time–height cross-sections over Delhi showing the gradient Richardson number (R) for (a) CTRL and (b) RI-ALL simulations, and (c) their difference (RI-ALL − CTRL) during Event-I. Filled contours represent R (or R differences in panel c), while black contours denote selected R values. Positive (negative) differences indicate enhanced (reduced) atmospheric stability in the RI-ALL simulation relative to CTRL.

L582-584: Should remove "significantly" – it is not shown that EXP5 performs significantly better than CTRL. And where do the 25-37 W m-2 bias reduction numbers come from? Table S9 shows only a small reduction. Same with the ~80 W m-2 bias reduction, where is that from?

Response: We thank the reviewer for this careful observation. We agree that the term "significantly" may imply a formal statistical assessment, which was not performed in this study. Accordingly, we have removed this term from the revised manuscript.

We also agree that the values of 25–37 W m^{-2} and ~80 W m^{-2} do not represent reductions in model bias. Rather, these values quantify the changes in simulated SWDOWN resulting from the revised aerosol optical properties. Specifically, the 25–37 W m^{-2} values refer to the monthly mean reductions in SWDOWN relative to the CTRL simulation during the long-term simulations, while the ~80 W m^{-2} value represents the maximum hourly change in SWDOWN during the highly polluted conditions of Event-I. At the same time, we note that the revised aerosol optical properties do improve agreement with observations, as evidenced by the reductions in mean bias and RMSE discussed in Sections 3.2 and 3.5. The revised text now clearly distinguishes between (i) the physical impacts of modified aerosol optical properties on SWDOWN and meteorology, and (ii) the resulting improvements in model performance relative to observations.

L584-589: It looks like these are the maximum changes during the extreme event no. 1, so the improvements appear a bit overstated, in my opinion.

Response: We thank the reviewer for this observation. We agree that the meteorological and air-quality changes quoted in the conclusion correspond primarily to the impacts observed during the severe pollution episodes, particularly Event-I, and may not be representative of the entire simulation period. Our intention was to summarize the maximum response of the system under highly polluted conditions, where aerosol–radiation interactions are strongest.