Review of ACP Manuscript: egusphere-2026-472

General Overview

This study presents a computationally efficient framework using geostationary GEMS NO_2 retrievals to update hourly NOx emissions over the Bangkok Metropolitan Region (BMR). The authors utilize a Cross-Sectional Flux (CSF) inversion to derive top-down constraints, which are used to update the hourly prior NO_x emissions and then integrated into a WRF-Chem simulation and evaluated against independent observations from the ASIA-AQ campaign. Overall, the top-down emissions estimates improve the WRF-Chem simulation of NO_x over the BMR, although both the model results and the emissions may be biased low. This is likely due to potential systematic errors in the GEMS data and the impact of uncertainties in model meteorology. The research fits well within the scope of Atmospheric Chemistry and Physics and the proposed algorithm is particularly timely given the recent operational status of the global geostationary constellation (TEMPO, GEMS, and Sentinel-4). However, I have some concerns regarding the methodology and certain discrepancies in the data presentation that should be addressed. I recommend minor revision before the manuscript is considered for publication.

General Comments

Section 3: The study relies heavily on the CSF method for deriving top-down NO_x emissions. While this approach is computationally inexpensive, it has known limitations. For instance, how does the algorithm determine the optimal length for line density integration? Additionally, what is the sensitivity of the inferred emissions to transport wind speed errors and the fitting process for line density and chemical lifetime? I suggest the authors include a brief discussion on the structural uncertainties inherent to the CSF method.

The authors develop a novel approach for updating diurnal emission profiles; however, the inversion is essentially simplified to a 1-D problem (magnitude over time for a single urban box). Inaccuracy in the spatial distribution of NO_x sources also contributes significantly to model error. Do the authors have thoughts on how to enhance this method to quantify emissions on a finer spatial scale?

Biomass burning in Southeast Asia significantly influences regional air quality. Was the study period affected by any specific biomass burning events? I would appreciate a brief discussion on how such events might impact the NO_x and O_3 simulations, as well as the impact on the NO_x emission inversion.

Section 3&4: The current terminology used to describe emissions is inconsistent and may be

confusing to the reader. Terms such as 'prior NO_x emissions,' 'CSF-derived emissions,' and 'emissions after applying the forward operator' are used interchangeably or in ways that overlap. I suggest the author review these sections to consolidate the terminology and ensure that uniform terms are used consistently throughout the manuscript to distinguish between the different emissions datasets.

The citations for figures in the Supplement appear disorganized. Please verify the numbering and ensure they are cited sequentially.

Specific comments:

Line 49-51: The statement that GEMS NO_2 product bias "cannot be diagnosed using satellite data alone" seems somewhat redundant, as independent observations are inherently required to assess retrieval quality.

Line 125: The reanalysis dataset should be formally cited as NCEP FNL.

Figure 1: Since NO_x is primarily emitted as NO, it would be more appropriate to plot either NO or total NO_x emissions rather than just NO_2.

Table 1 and Section 6.1.2: Since model NO_x is sensitive to the accuracy of local wind field simulations, were any measures like grid or observational nudging applied to the model meteorology? Furthermore, why was 1-degree NCEP FNL data utilized instead of higher-resolution 0.25-degree data?

Line 227: Correct the citation for Kuhlmann et al. to (2024)

Line 298: Please provide more justification for the fixed conversion factor f_q = 1.32, as the NO_2-to-NO_x ratio is highly dependent on environmental conditions and emissions sources. Could WRF-Chem provide a localized factor for Bangkok?

Line 343-347: Do the author know why does the CSF method perform better than the GP or IME approaches in recovering the model's prior emissions?

Figure 3b: Is the black dashed line (WRF Prior) representing the three-hour backward average of EDGAR v5 emissions? It appears visually inconsistent with the prior curve shown in Figure 4c.

Line 370-378: I have concerns regarding the GEMS NO_2 column scaling factor in Equation 11. Early morning model errors in boundary layer height and NO_2 profiles can introduce artifacts into this scaling factor. Would it be more robust to correct GEMS NO_2 columns using independent observations (e.g., Pandora, MAX-DOAS) prior to the inversion?

Section 4.1: Since the authors did not directly assimilate NO_2 columns, the term "satellite observations" in this context is imprecise (e.g., Line 422-424, Line 434-435). I suggest using "GEMS-informed emissions" or a similar term.

Figure 4: The caption for Figure 4a and 4b requires more detail to clearly distinguish between the optimized profiles and those viewed through the observational operator.

Line 527-528: Beyond meteorology, how do inaccuracies in the spatial pattern of emissions affect the inversion framework? Do the authors have any thoughts on addressing this issue under this study's emissions inversion framework?

Line 643-644: Note that the highest observed O_3 peak actually occurred on March 22, rather than March 20–21.

Line 655-661: The shift in O_3 peak time to the early morning in the $WRF_{updated}$ rum (Figure S12a) does not align with observations. Does this discrepancy suggest the emission inversion timing might be incorrect?

Line 677-679: Please clarify which Pandora data (e.g., direct sun or sky scan), data screening, and temporal collocation methods were used for the evaluation

Equation 17: Please define SW (Scattering Weights).

Figures 8–9: I suggest converting the time from UTC to Local Standard Time (LST) for better interpretability.

S19 is provided in supplement materials but not used in the manuscript.

Line 804: This reference should likely be Figure S17.

Lines 925–929: Any model output (e.g., WRF_{base}) can be used as a transfer standard for accessing GEMS vs. GCAS, though high-resolution models are preferred.