This study uses NO_2 observations from the GEMS satellite to re-estimate NO_x emissions over Thailand. It updates emissions in two ways: where they are higher or lower across regions (spatial pattern), and how they change through the day (diurnal variation). Using a geostationary satellite like GEMS to capture the daily cycle of emissions is a worthwhile contribution, since this is something the low orbit satellites cannot do.

The method itself (IFDMB) is taken from earlier work, but applying it to Thailand and using GEMS to refine the diurnal cycle is a genuinely useful effort. The main issues to address are (1) how trustworthy the resulting emissions are, and (2) whether the diurnal results are physically reasonable. I recommend major revisions.

Major comments

1. The North Thailand emissions are likely contaminated by a spurious satellite signal.

: The paper concludes that emissions increase in the North, but this comes from an abnormally high GEMS signal over Lampang. As the authors themselves show (Section 3.5), this signal does not appear in TROPOMI or in the alternative GEMS retrieval (DVCF). It originates from an error in the prior inventory (ASIA-AQv3) used inside the GEMS retrieval, which then propagates through the inversion and shows up as a false "emission increase". Presenting this as a real finding is misleading. Please state clearly in the abstract and conclusions that the North increase is most likely a retrieval artifact, and report the Thailand-total posterior emission both with and without the North so readers can judge its influence.

2. The regularization parameters (C_1, C_2) controlling the strength of the emission adjustment are insufficiently justified.

: This study uses two values, C_1 and C_2, which essentially decide how much to trust the satellite and how aggressively to change the emissions. These values drive the over-adjustment in the North raised in comment 1. Yet the chosen values are justified only briefly, and the sensitivity test (Table S1) shows the results change substantially with them (e.g., NMB improvement ranging ~30–66%). Please move a short sensitivity analysis from the Supplement into the main text, and discuss letting C_1 vary by region. Also, the signs in the denominators of

Eqs. (2) and (4) appear to contradict the text, which says C_1 is added to "inflate" the denominator. Please double-check.

3. The diurnal emission update is a good direction, but the results need to be made more physically realistic.

: Using GEMS to capture the daily cycle of emissions is a real strength of this paper. However, the current results need a few improvements before they are usable. Because the satellite only sees during daytime, the updated emission curve drops back to the prior at night, creating an unnatural, discontinuous shape. The authors mention applying a smoothing step — please actually apply it and show the result.

4. GEMS misses the rush-hour periods (early morning and evening) when emissions are highest.

: Please quantify how much this gap affects the updated diurnal profiles. The conclusion ends somewhat vaguely ("no single scheme is consistently best"). A clear, practical recommendation — e.g., which scheme to use when spatial accuracy matters versus when capturing the daily cycle matters — would be much more useful to future users.

5. Averaging the data to 81 km removes the ability to constrain local sources.

: The analysis groups the data into 81 × 81 km boxes (9 times coarser than the model). Please justify quantitatively why 9× and not, say, 3× or 5×. If possible, test one or two intermediate resolutions to show the trade-off between reducing the spread of signal into neighboring cells and losing the ability to constrain local sources such as traffic. This directly relates to why the roadside surface comparison gets worse.

Minor corrections

1. Some numbers are inconsistent between sections. For example, the NRMSE reduction for the temporal-average scheme is 57% in the text (line 581) but 61% in the conclusions. The

daylight scheme is 36% (line 579) versus 39% (line 820).

2. Line 836 says "October 2023," but the study period is September 2023 ?

3. Figure references: line 410 "(Fig. 5a)" should be "(Fig. 6a)"; line 522 "10f" does not exist and is likely "Fig. 9f". Please check.

6. The surface stations (PCD) measure NO_x while the model is compared as NOy (lines 195–198). Please quantify or at least bound how much this difference affects the results.

7. The reference list appears to contain a duplicate (Kim et al. 2023a and 2023b look like the same paper). Please also disambiguate the several "Li et al., 2023" citations in the text.

8. Typos: line 807 "access" → "assess"; line 478 "Thongsame et al. (2023)" → "Thongsame et al. (2024)."