Response to Reviewer 3

Reviewer's comments are in italics, followed by our responses in non-italics.

This paper presents an evaluation/comparison of modeled tropospheric nitrogen dioxide in UKCA with OMI satellite observation. The model results are presented and compared with satellite data. The following are my comments;

Major comments:

1. *The manuscript keeps mentioning evaluation/comparison of two datasets. For example, the title describes it as 'evaluation' but abstract section mentioned it as 'comparison'. From my understanding, comparison involves analyzing the similarities or differences while evaluation is a quantitative assessment of how well a model replicates reality (OMI satellite retrievals in this case). The authors need to clearly describe whether they are evaluating or comparing the two datasets.*

We apologize for any lack of clarity. As described in an earlier response to Reviewer 2, we consider our study a quantitative evaluation of tropospheric NO_2 columns (and their trends) over S/E Asia in the UKCA model.

2. *The need for the comparison is not well established in the introduction section. Is this the first-time comparison between UKCA model and OMI or the comparisons reported before for two datasets? Further, why did the authors choose OMI for evaluating/comparing the UKCA results? Are the OMI NO_2 observations evaluated before in the study region? and are good enough to make comparisons. If so, authors can mention some studies conducted for evaluation of OMI NO_2 columns in different regions.*

This study represents a comprehensive evaluation of the UKCA model using OMI NO_2 data over South and East Asia, a region with diverse and rapidly changing emission sources. OMI and UKCA NO_2 columns have previously been compared in Archibald et al. (2020). However, our current study goes into more detail about the comparison, considers other seasons, diurnal variations, and also temporal trends.
OMI was chosen for its long-term (since 2004), daily, high-resolution measurements of tropospheric NO_2, which provide robust datasets for evaluating model performance. The suitability of OMI observations for model comparison has been demonstrated in numerous studies. For example, Lamsal et al. (2014) evaluated OMI NO_2 data against in situ and surface-based observations over the continental U.S., confirming its reliability for capturing regional NO_2 distributions. Additionally, comparisons between OMI and ground-based MAX-DOAS observations in East Asia have demonstrated consistency (Irie et al. 2009), further supporting OMI's suitability for model evaluation in this region.
We have revised the introduction to include these references and clarify the motivation.

3. *The paper is describing the diurnal simulations from UKCA model, but the OMI only provides one measurement per day (as described by the authors), then what about evaluation/comparison of diurnal variation? or the authors only evaluating for satellite overpass time? If this is the case, then figure 02 may lead to a confusion for the readers as if diurnal variations are evaluated.*

We acknowledge that the OMI satellite provides only one measurement per day at its overpass time (~13:45 local time), which limits its ability to capture the full diurnal variation of NO_2. In our study, the comparison between the UKCA model and OMI observations is restricted to the satellite overpass time, where the model is sampled to match the temporal resolution of the satellite.

The diurnal variations shown in Figure 2 are derived solely from the UKCA model simulations and are not directly evaluated against OMI data. The purpose of presenting these diurnal profiles is to provide context for understanding how NO_2 concentrations vary throughout the day in the model, influenced by meteorological processes such as boundary layer dynamics and photochemistry. This helps illustrate the broader behaviour of NO_2 beyond the satellite overpass time.

It is important to appreciate the large diurnal cycle of NO_2 and the seasonal variation in this diurnal cycle, as this affects uncertainties in the model-satellite comparison. For example, as we state in the manuscript, during summer over China the satellite retrieval time is during a broad minimum, whereas during winter the retrieval time is during a time of day when NO_2 column is changing. This means that the wintertime comparisons carry larger uncertainties. Without illustrating the diurnal cycle this would not be obvious.

To address potential confusion, we have revised the caption for Figure 2 to clarify that the diurnal variations are simulated by the model and not directly compared with OMI observations. Additionally, we ensure that the text explicitly states that the model-observation comparisons are limited to the satellite overpass time.

Minor Comments

1. *Line 99-100: "The model's horizontal resolution (N96: 1.875° longitude × 1.25° latitude) is much coarser than the satellite data products used." I think there is a need to explain how the data is matched on a spatial scale for fair evaluation/comparison.*

The model-satellite comparison requires careful spatial matching to ensure fair evaluation. Although the UKCA model has a coarser horizontal resolution (N96: 1.875° longitude × 1.25° latitude) compared to the higher resolution of OMI satellite products, we accounted for this difference by interpolating the satellite data to the model grid. Specifically, the satellite-retrieved NO_2 data were averaged over spatial areas corresponding to the UKCA grid cells, ensuring consistency between the two datasets. This approach allows for a meaningful comparison while minimizing biases introduced by the resolution mismatch.

2. *Line 109: What's the resolution of ECMWF ERA-interim data used here?*

The ECMWF ERA-Interim data used here is obtained at T255 (78 km) resolution on ECMWF hybrid-p levels, provided at six-hourly intervals. These variables are then interpolated to the N96 resolution. The resolution information has been added in the revised manuscript.

3. *Line 144: The study area is mentioned at the end of methodological section. I would suggest moving the description of study area at the start of methodological section.*

We agree that moving the study area description to the start of the methodological section would improve the flow and clarity. We have adjusted the structure accordingly to introduce the study area at the beginning of Section 2.

4. *For Figure 1, why the percentage change is much higher over the oceans, what could be the possible sources/reasons? also it would be helpful to show the emissions for 2005.*

The higher percentage changes observed over the oceans are primarily due to large changes in shipping emissions. As global shipping activities have increased, they have contributed significantly to emissions over ocean areas. Additionally, we have modified Figure 1 to include the emissions data for 2005, as suggested.

5. *Line 174-175: Other than the pollution, what could be the role of meteorological parameters in modulating the vertical distribution/height of NO_2 in different regions of study area?*

Thanks for this question. Meteorological factors, particularly temperature, play a significant role in modulating the vertical distribution and lifetime of NO_2 (Atkinson, 2000; Liu et al., 2016). Lower winter temperatures slow chemical reactions, extending the atmospheric lifetime of NO_2. This effect, combined with shallow boundary layers and stable atmospheric conditions, contributes to higher NO_2 concentrations near the surface and alters its vertical distribution.

References

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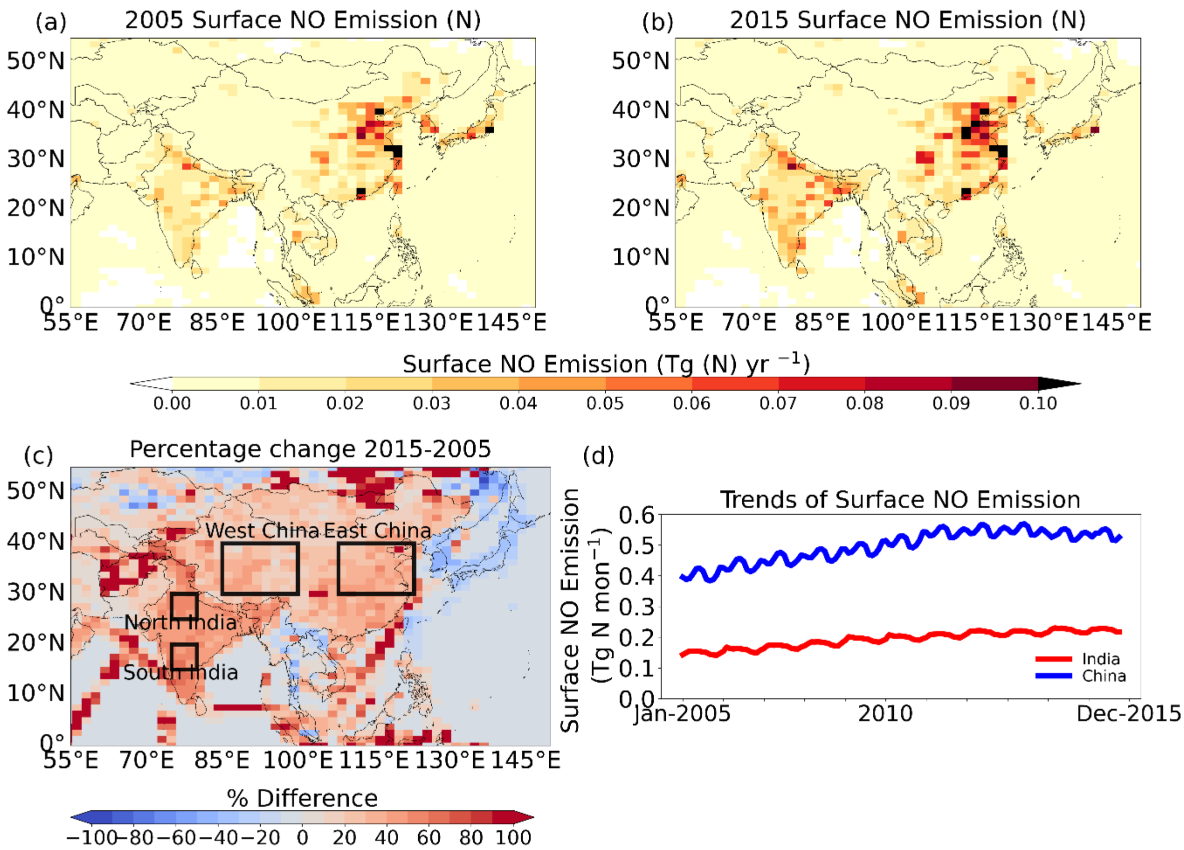

Revised Figure 1 Surface nitrogen oxide (NO) emissions over S/E Asia (Tg N yr^{-1}) in (a) 2005 and (b) 2015; (c) Percentage change in the NO surface emissions from 2005 to 2015; (d) trends of NO surface emissions (Tg N month^{-1}) from 2005 to 2015 over India and China. Boxes shown in (b) indicate regions referred to in the text.