

We thank the two anonymous reviewers for their helpful comments. The reviewer comments are in black and our responses are in blue and include page and line numbers where changes were made to the accompanying manuscript.

Response to Reviewer #1

This manuscript is very well written and makes an important contribution to the understanding of NO₂ trends over the United States. The authors have succeeded in reconciling the different trends seen in OMI NO₂ tropospheric column data and surface NO₂ mixing ratio observations. The analyses in the manuscript indicate that a better understanding of tropospheric background NO₂ is needed to interpret satellite NO₂ observations in terms of trends. The authors show that the GEOS-Chem model well simulates the surface NO₂ trends compared with AQS and SEARCH observations, but fails to simulate the flattening of the tropospheric NO₂ column seen in the OMI observations. Improvement of the model column trend in the winter is achieved in a sensitivity simulation by adding additional free tropospheric NO₂. However, the authors do not recommend any particular model modifications to remedy this situation. The manuscript could be strengthened by adding some recommendations in this regard. I recommend publication after minor revisions.

Specific comments: p. 6, line 2: I think the word "steady" should be removed here, as you indicate later on this page that the downward trend does become smaller after 2009.

We rephrase our discussion of the AQS trends on page 6 lines 1-2 and 10-13.

p. 7, line 31: for nitrate wet deposition and tropospheric column NO₂ is similarly weaker.....

We make the addition to the sentence as suggested on page 7 lines 32-33.

p. 9, line 12: at the end of this paragraph it would be appropriate to mention your earlier findings (Silvern et al., 2018) that an alternative hypothesis for why the model NO/NO₂ ratio is too large compared with observations is that there may be errors in the model cycling of NO, NO₂, and O₃. You showed that adjusting the rate constant for the NO + O₃ reaction and the NO₂ photolysis rate can lead to improved NO₂ results.

We add discussion of the finding of possible kinetic errors in Silvern et al. (2018) on page 9 lines 16-18.

p. 10, lines 8-9: higher lightning flash rates are observed in tropopause penetrating storms and these type of storms may have increased in frequency. However, the Lightning Imaging Sensor (LIS) data do not seem to show any long-term trend in lightning over the US. This should be mentioned here.

We now include a statement about no long-term trend in total lightning from LIS on page 10 lines 15-17 (Koshak et al., 2015).

p. 10, lines 11-12: It would be good to explain that even at 0.5 x 0.625 degree resolution the model is too coarse to resolve convective overshoots.

We add a comment about model resolution not resolving convective overshoots on page 10 lines 20-21.

p. 10, line 31: remove "steady"

Removed, page 11 line 5.

p. 11, lines 13-14: Can you recommend modifications to GEOS-Chem to better represent NO₂ background?

We identify possible errors in GEOS-Chem that could account for the underestimate of background NO₂ on page 11 line 24-page 12 line 2.

References

Koshak, W. J., Cummins, K. L., Buechler, D. E., Vant-Hull, B., Blakeslee, R. J., Williams, E. R., and Peterson, H. S.: Variability of CONUS Lightning in 2003-12 and Associated Impacts, *J. Appl. Meteorol. Climatol.*, 54, 15-41, 10.1175/jamc-d-14-0072.1, 2015.

Response to Reviewer #2

This paper performed an intercomparison of trends between the emission inventory, satellite observations and in-situ measurements. It shows that the flattening of the OMI NO₂ trend is in fact not inconsistent with the sustained decrease of NO_x emissions reported by the NEI, and that the NEI emission trend is consistent with other atmospheric observations of NO_x and ozone trends, highlighting the importance of accounting for the free tropospheric NO₂ background. The paper is well written and the analysis is solid. I suggest publication after very minor revision.

General comments: 1. Page 6, line 10. “But they used all AQS sites in that analysis including those with incomplete records, which could bias the trend.” Does this indicate that the sites without complete records have flattening changes? Where are those sites located? Any reasons why their trends are different from others?

Here we simply point out that our more restrictive analysis gives different results, as stated in the text, page 6 lines 8-11.

2. Page 8, line 4. “the post-2009 flattening of the OMI trend is due to background influence rather than to leveling of US NO_x emissions.” An additional analysis about the possible driving forces for the changes of background emissions are appreciated.

We clarify on page 8 line 5 that we do not suggest a trend in background emissions, but rather a change in the relative contribution from background sources to the observed tropospheric NO₂ column.

Specific comments: 1. Figure 1. Please change the color for soil or fertilizer. It is not easy to distinguish them.

We have changed the colors in the right panel of Figure 1 (page 23).