

[Interactive
Comment](#)

***Interactive comment on* “Emissions of nitrogen oxides from US urban areas: estimation from Ozone Monitoring Instrument retrievals for 2005–2014” by Z. Lu et al.**

Anonymous Referee #1

Received and published: 27 June 2015

This paper presents an interesting and valuable assessment NO_x emissions based on OMI observations. The strategy for using wind observations building on Valin et al.'s initial insight is nicely explained and results in emissions estimate that are significantly different from prior estimates. The paper should be published.

The one larger concern I have is with the absence of a discussion of possible remaining systematic errors. The authors have identified a substantial systematic error in prior emissions estimates (and trends) and explained the error arises by using OMI observations under all wind conditions. I wonder whether they think other significant systematic errors remain. Examples: Weekend vs. weekday emissions, a priori correlated with

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)



winds, chemical feedbacks, ...

Other minor issues: >On p. 14976, lines 10-15, the paper describes the procedure for filtering by wind speed and ensuring sufficient OMI observations. de Foy et al. (2014) does show that the EMG method generally gives the same answer for emission rate once the wind speed is greater than 3 m/s. Given this, why not take all winds >3 m/s rather than vary the cut off? Was this to be more consistent with Valin et al. (2013) where the number of OMI observations made it possible? Would always using 3 m/s change the results very much?

>Is the “fitting interval” (p. 14978, line 21) x_0 , σ , or something else? If it is x_0 , then what is the difference between the effective and dispersion lifetimes? Is it that the effective lifetime is obtained at high wind speeds and the dispersion lifetime at low wind speeds?

>The discussion of NO_x lifetimes on pp. 14977-14978 is secondary to the main discussion of the correlation between the various emissions and burden trends, so it seems out of place in the middle of a section on emissions. Maybe it should be moved to the end of that section, or given its own (albeit short) section?

>p. 14975 – lines 12-15. when discussing trends from weak wind data, is it meant to isolate the trends in the NO₂ column/burdens specifically? This sentence could be clarified as “we utilize OMI data under weak wind conditions to calculate the satellite-observed NO₂ columns, burdens, and trends in these quantities in this work.” This would just make clear that weak wind data is not being used to calculate trends in OMI-derived emissions.

>p. 14977 – lines 9-10. Consider rewording as “and trends in NO₂ columns obtained at slow winds may better reflect the real bottom-up NO_x emissions trends.” This helps make clear that that column measurements at weak winds are being used as an indication of trends in surface emissions, rather than the strong-wind method of deriving emissions directly.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



>p. 14977 – lines 20-21. What exactly is meant by statistically significant in this context? Is it just that the columns are observable over background given the precision of the OMI measurements?

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 14961, 2015.

ACPD

15, C4104–C4106, 2015

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



C4106