

***Interactive comment on* “Reconciling the differences between OMI-based and EPA AQS in situ NO₂ trends” by Ruixiong Zhang et al.**

Anonymous Referee #2

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The manuscript *Reconciling the differences between OMI-based and EPA AQS in situ NO₂ trends* by Zhang et al. is an investigation of the differences between trends in tropospheric NO₂ columns derived from the OMI satellite instrument and those derived from the EPA AQS network. This is an important and interesting research question, as in remote areas one often has to rely on remote sensing data in order to get reliable measurements of air quality. The manuscript falls well within the scope of AMT.

That being said, the manuscript fails to convince the reader regarding the comparability of the two datasets to begin with. Also, the manuscript is often too imprecise.

Most of the following points are minor and can be fixed by providing more precise information about what the authors did exactly, but they should be addressed before publication in AMT:

1 Comparing VCDs and surface concentrations

The authors fail to convince the reader why OMI VCDs, which are the integrated NO₂ content of the troposphere at a given location, should be comparable to the in-situ surface concentrations of the AQS dataset. There have been numerous studies trying to relate the two measures to each other, and it should be clear to the authors that in order to compare the two, one has to take special caution. This becomes most problematic in the discussion of the effect of the lightning filter, where the authors leave the impression that lightning leads to "wrong" OMI VCDs.

In a revised manuscript, the authors should include a summary of the problems arising from comparing the integrated satellite to the in-situ point measurements, should reference relevant literature, and should make sure that they consider these differences in the comparisons of the relative trends. Also, they should explicitly discuss the problems arising from comparing *relative trends* of these two different measures.

2 Definition of the relative trends

- 07/05: It is not entirely clear how exactly the authors calculate the relative trends. Is it a linear trend, calculated by linear regression? By default, the Mann-Kendall test is non-parametric. If the authors use the *Sen slope estimator* as *relative trend*, they should explicitly say so. Otherwise, the authors should explicitly say what the reference value is for the *relative trends* (i.e., 2005, or average of the whole period, or ...).
- In some places, the authors do give an uncertainty of relative trends. However, they do not give enough detail about how these trend uncertainties are being calculated. If they indeed use the *Sen slope estimator* from the Mann-Kendall test as relative trend, it is unclear how they define the uncertainty of this estimate.

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This is however crucial in order to evaluate if the improvements in the agreement of OMI and AQS relative trends are statistically significant at all. Furthermore, in some Figure captions the authors indicate *95% confidence intervals*; please briefly describe in the text how these are derived.

- Another point regarding the trend calculations is the uncertainties of the relative trends. The notion of *difference between OMI and AQS trends* only makes sense if there is some way of assessing if these differences are statistically significant at all.

3 Importance of yearly varying NO_x emissions

05/04: The authors claim that the *yearly variations of [...] anthropogenic emission changes have little impact on trend analysis results*, and they cite a paper by Lamsal et al. (2015). However, in the cited paper, Lamsal et al. state (Sect. 2.2.1):

In this work, we further improve the operational OMI NO₂ retrieval [...] by using new a priori NO₂ profiles [...] with year-specific emissions. The year-specific emissions not only improve the representation of the NO₂ vertical distribution, but also capture the yearly changes in NO₂ profile shapes. The latter is critical due to rapid decline in the U.S. NO_x emissions in recent years [...].

Since the present study deals with the time period 2005–2014, I do not see how the authors' choice to use fixed 2010 NO_x emissions is backed by the cited work by Lamsal et al. Given the fact that the study period does include the years of economic crisis, the authors' choice to use fixed emissions is questionable. I strongly suggest some quantitative assessment of the influence of using fixed emissions.

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4 Reconciling chemiluminescent and photolytic in-situ measurements

The authors claim that calculating a correction factor for the chemiluminescent in-situ data by taking the average ratio of chemiluminescent to photolytic measurements. This would only work properly if the reasons for the high bias of the former instruments were identical at all measurement stations. While it is true that this correction does not influence the relative trends, the authors should at least mention this.

5 Importance of individual sources of AMF uncertainty

04/28: The authors claim that *the first two factors* are most important for the NO₂ trend analysis, but fail to back up their claim.

6 Time span of lightning filter

06/20: The authors' choice of lightning filter (72hrs / 90km) seems arbitrary and needs to be justified. As the authors correctly state, the lifetime of NO_x in the free troposphere can reach up to one week. By making their filter only 90km wide, a back-of-the-envelope calculation quickly shows that the NO_x produced by a single lightning occurrence can easily be transported considerably further within 72hrs than only 90km. The authors seem to be aware of this inconsistency, because they introduce an additional filter for the Northeast which depends on lightning occurrence in the South, implying a transport distance of many hundreds of kilometers.

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7 Minor comments

- 04/18: NO₂ *partial* VCDs
- 05/06: Which trends? Those with the *default* albedo, or those with the *update*? ...)
- 05/09: I personally find the name *ocean trend* misleading, as it has nothing to do with the ocean (except for the geographical location of the clean background region). Maybe the authors can come up with a name that somehow indicates the origin of the trend (e.g., *instrument drift*).
- 07/10: It seem that there are *four* different OMI-based NO₂ trends
- 07/15: To avoid confusion, please explicitly mention that these are *absolute* differences of the *relative* trends.
- 09/05: *trends of OMI data are less* — than what?
- 09/07: OMI VCDs are *not* overestimated when not filtering for lightning NO_x - the lightning NO_x is part of the VCD. It leads to worse agreement between OMI and AQS trends, but then again, these are two fundamentally different measures anyways.
- 09/11: What is a *reduction of decreasing surface trends*? Misleading phrase, since the trends are decreasing trends to begin with. Maybe it'd be better to say *stronger decreasing trends* or something similar.
- 09/12: Again, OMI VCDs are not *biased* due to lightning, see above.
- 09/13: *reduction of decreasing trends* — see above

- 09/15: OMI VCDs are not wrong when they include lightning NO_x – the authors should therefore not make the qualitative statement *corrected here*. *Filtered* would be better.
- 09/29: I would assume that the driving factor in stronger decreasing trends close to anthropogenic source regions is the decreasing emissions in those, resulting in less transported NO_x in those areas.
- 10/03: Since comparing VCDs to surface concentrations is a difficult issue to begin with, I would not blame the OMI retrievals for the differences – when comparing apples and oranges, why should one blame one and not the other for the differences? Saying that the OMI data are *not designed for trend analysis* doesn't make sense. If one has to *design* a dataset in order to be able to do trend analysis, maybe there just are no significant trends in the underlying data to begin with?
- In Fig. 1a-d, it is not clear if positive numbers mean that the OMI trend or the AQS trend is higher. Please update the Figure caption with a mathematically precise description (e.g., "OMI relative trend minus AQS relative trend").
- Fig. 3: Please update the Figure caption with a *precise* indication of the units, e.g., "number of days [...] per REAM grid cell". Also, please spell out *cloud-to-ground* instead of *CG* in the caption.
- Fig. 6: Please indicate NO₂ somewhere in the Figure caption. Also, the legend for the OMI data should be something like *OMI (lightning filter)*; after all, the data show trends of OMI NO₂ columns and not of *the lightning filter*.
- Fig. 7: I don't understand what *the figure legends are the same as in Figs. 6 and 8* is supposed to mean. Please clarify.

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- Fig. 7: Please explicitly indicate in the Figure caption if statistically insignificant trends are shown or not.
- Fig. 9a: There is something wrong with the Figure caption, it does not contain a complete sentence (maybe there's just a *of* missing?). Please indicate what the barbs on the individual data points mean.
- Fig. 9b: Please be specific about which OMI NO₂ data you show in this Figure, using the nomenclature from earlier. As explained above, the notion of *corrected* is misleading.

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