Interactive comment on “Reconciling the differences between OMI-based and EPA AQS in situ NO$_2$ 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 NO$_2$ trends by Zhang et al. is an investigation of the differences between trends in tropospheric NO$_2$ 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 NO$_2$ 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-parameteric. 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.
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 NOx 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. NOx 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 NOx 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 NOx 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 NOx 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: NO$_2$ 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 NO$_2$ trends
• 07/15: To avoid confusion, please explicitly mention that these are absolute dif- 
  ferences 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 NOx - the 
  lightning NOx 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

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• 09/15: OMI VCDs are not wrong when they include lightning NOx – the au- 
  thor 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 NOx 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 com- 
  paring apples and oranges, why should one blames 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 analy- 
  sis, 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 NO$_2$ 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 NO$_2$ 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.
• 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 NO$_2$ data you show in this Figure, using the nomenclature from earlier. As explained above, the notion of corrected is misleading.