

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

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Hi Ruixiong,

Interesting paper, I have a couple of comments. Checking for trends in OMI data can be tricky, especially due to the row anomaly.

One point I would like to raise is whether or not the observed trends you find are (1) actually 'real' - or in other words not caused by trends in the cloud fraction, cloud-top pressure, the AMFs or number of samples, and (2) statistically significant. Both aspects are important when comparing to real data. You might be getting better agreement to the trends in the in-situ data - but is that because the OMI NO₂ is improved with your corrections or because a trend, in say the AMFs, has been introduced? I honestly don't

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know but you should at least consider it.

Please see the discussion in my paper:

Barkley, M. P., González Abad, G., Kurosu, T. P., Spurr, R., Torbatian, S., and Lerot, C.: OMI air-quality monitoring over the Middle East, *Atmos. Chem. Phys.*, 17, 4687-4709, <https://doi.org/10.5194/acp-17-4687-2017>, 2017.

Section 2.3.1 Ocean trend. Is the trend statistically significant? In my paper we looked at OMI NO₂ over the Pacific (60 N–60 S, 90–170 W) - we couldn't find a statistically significant trend - but hey we maybe gridded the data in a different way!

Section 2.3.3 The lightning filter. I like this approach but aren't you introducing a sampling trend by losing these observations? I assume this is done per OMI observation - in which case you changing the footprint resolution too.

Anyways, good luck with the paper.

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