

Interactive
Comment

Interactive comment on “Impact of NO₂ horizontal heterogeneity on tropospheric NO₂ vertical columns retrieved from satellite, multi-axis differential optical absorption spectroscopy, and in situ measurements” by D. Mendolia et al.

Anonymous Referee #2

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This study presents comparison of NO₂ columns from three independent measurement methods: satellite-borne OMI instrument, ground-based multi-axis DOAS instrument, and in situ chemiluminescence monitors. A careful intercomparison can provide valuable insights and help develop improved validation strategies for satellite retrievals. Major strengths of the manuscript include the use of independent measurement techniques and thorough review of previous publications. But, I have several concerns as listed below that need to be addressed before it can be published in AMT.

General comments

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1. The title is not clear to me. What is the purpose of this study? The approach followed and results obtained are not revealing any major findings.

2. The conclusions drawn here heavily depend upon the method of deriving NO₂ vertical columns from in situ measurements. The method has been applied without testing the validity of the method. The authors could evaluate their method using NO₂ vertical profile from aircraft measurements or a CTM model simulation.

3. The authors are using outdated version 1.02 OMI NO₂ retrievals while new retrievals have been made publicly available for more than a year. Some comments on differences in results with the v2.0 data have been included, but such subjective statements are making the manuscript weaker. Although 20% difference between v2.0 and 1.02 have been reported in the mean sense, there must be a large spatial variation in the difference given the nature of improvements made from v1.02 to v2.0 in the DOMINO product. The analyses should be revised using current improved retrievals.

4. MAX-DOAS observations are too few (9) to be helpful in drawing any conclusions. Moreover, these observations were taken at different location, elevation, and azimuth angles. I clearly see a danger in interpretation made with poorly characterized data.

5. Presentation of the manuscript should be substantially improved. In few places, there are some lengthy discussions that are not helpful for the intent of this study, but are just distracting. For example, some parts of the first paragraph and 2nd paragraph (Page 827, lines 5-28+) are not adding any value to motivate this study. I do not understand what the authors are trying to say by discussing the results of Petritoli et al and Ordonez et al (in Page 829). Rather than discussing others works, state clearly and concisely how your work is different, superior, and valuable. The paper will benefit by reducing the introduction section and focusing on objectives of the paper. Same comment applies for discussions of MAX-DOAS instrument, retrieval method, and uncertainties. Please, provide the information in consistent manner as in Section 2.4.

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Specific comments

1. Abstract: I think, the first sentence is not quite right. The authors did not retrieve trop NO₂ from OMI for the first time. Satellite data did exist for Toronto for a long time.
2. Page 828, lines 4-8: This statement is unclear. NO₂ is retrieved from spectral fit in 405-465 nm window. There is no need to include the wavelength range here as the information is also in the OMI section.
3. Page 828, line 28: Remove negative sign from longitude coordinate.
4. Page 829, lines 6-29: What message the authors want to provide from these paragraphs? Are the authors implying that the methods they intend to use have previously been used?
5. Page 830-831: I do not understand what the authors are trying to say here. The discussions sound like MAX-DOAS measurements have previously been used for evaluating satellite retrievals, but there are several difficulties associated with the approach. Then, why authors are still considering the same approach?
6. Page 831, lines: 26-28: "The goal of this study was to evaluate...". I do not think this study is evaluating the response of NO₂ column to NO₂ emissions.
7. Page 832, lines 8-12: This information is behind in MAX-DOAS information. I suggest to delete the statement.
8. Page 832, line 24: Remove "pollution".
9. Page 833, lines 11-13: "Both MAX-DOAS and in situ measurements ...". I wonder how that would be possible. Please, elaborate.
10. Page 833, lines 22-23: What is the need of averaging data for 2 hours to compare with OMI and MAX-DOAS observations? Wouldn't it make more sense to use in situ observation that is close to OMI measurement time, which is available in OMI data files?

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11. Page 834, Eqn 3: This equation is misleading because not all NO_z are converted to NO₂.

12. Page 835, line 3: Previous studies suggest that NO_z interference show strong seasonal variation. How did that interference change in winter and summer months at both altitudes?

13. MAX-DOAS: As stated earlier, shorten this. I do not see much value by giving some terminologies such as “jscrip”, “DOASIS”, and “McArtim”. If needed, they should be cited. Please, provide the fitting window for MAX-DOAS retrieval.

14. Page 842, lines 14-18: “The larger NO_z interference correction at the . . .” Isn’t this sentence telling the same thing as in preceding sentence?

15. Page 845, lines 5-8: This is a subjective statement. The observed difference is not necessarily due to differences in the geographic footprint surveyed. The demonstration is not sufficient enough to claim this.

16. Page 847, line 20: Replace “retrieved” by “downloaded”.

17. Table 4 is unnecessary when the information on fitting window and fitting parameters are included in discussions.

18. Table 6: NO₂ DT decreased by 8% and NO₂ CN decreased by 12% after applying NO_z interference correction. Why the change in VCD is less than the change in NO₂? Shouldn’t it be between 8% and 12%?

19. I think, Figure 3a and b can be combined by using different colors and/or symbols.

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 825, 2013.

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