

Interactive comment on “Hemispheric asymmetry in stratospheric NO₂ trends” by Margarita Yela et al.

Anonymous Referee #1

Received and published: 21 July 2017

Review of “Hemispheric asymmetry in stratospheric NO₂ trends” by M. Yela et al.

In this paper, the authors report on long-term ground based zenith-sky observations of stratospheric NO₂ columns at four stations and how they changed over the last 20 years. Interestingly, trends are of the order of 10% per decade at all stations but have opposite signs for the NH sub-tropical station and for the SH high latitude stations. The stability of the trends with respect to time interval and parameters included in the trend model is evaluated, the difference between AM and PM trends discussed and the change in diurnal build-up of NO₂ evaluated. For Izana, DOAS NO₂-trends are also compared to other data sources from FTIR and satellite observations and qualitative but not quantitative agreement is found.

In my opinion, this is an interesting and thorough analysis of stratospheric NO₂ trends

C1

which provides interesting data and results. While interpretation is mostly limited to qualitative discussion, and detailed comparison to dedicated model runs for the locations of the instruments is needed for a more quantitative evaluation, the study is relevant enough as it is to warrant publication. The manuscript is clearly structured, well written and the figures are clear and useful. As I have only few comments and suggestions, I recommend publication of this paper after minor revisions.

Major Comments

In the discussion of the difference in NO₂ trends between Marambio and Ushuaia, it is stated that the months with large differences are linked to the presence of the polar vortex. While this sounds plausible, it is not really supported by Fig. 8 which shows large differences already in April. To me this appears more like a latitudinal dependence which is more pronounced in winter than in summer.

It is also stated that the trend in NO₂ columns could be linked to changes in vortex position and the resulting change in statistics. This makes sense, and at least for Ushuaia, it would be relatively simple to check this assumption by repeating the analysis but excluding all measurements where the station was influenced by polar vortex air. I'd suggest adding this analysis to the paper.

In the comparison to satellite data, it would be good to add some information on collocation criteria used and the respective overpass times of the satellites. I think it would also be interesting to compare the satellite data to the PM DOAS measurements. Although the time difference between satellite observations and AM data is usually shorter, the AM measurements are strongly impacted by night-time chemistry whereas the satellite data at least over sub-tropical regions are more representative for daytime chemistry. In my experience, correlation of ground-based and satellite NO₂ data is better when using PM observations at least at low latitudes.

Minor comments

C2

Line 38: I think the reactions listed do not lead to catalytic ozone destruction; for this reaction of NO₂ with O needs to be included

Line 56: major source of NO₂ => major source of NO₂ in the stratosphere

Line 93: was installed Antarctica => was installed in Antarctica

Line 116: were derived from 6 typical individual measurements => were typically derived from 6 individual measurements (?)

Line 134: effect on the cross-sections => effect on the effective cross-sections

Line 141: For monthly data – do you mean the fraction of months for which you have no data at all?

Line 160: alpha > 0.1 – alpha not defined

Line 160: significant values of less than 90% => significance values of less than 90%

Line 266: Both halogens should result => The observed changes in both halogens should result

Line 192: thus there was less N₂O₅ – while this is a reasonable explanation, I think other explanations cannot be completely excluded

Line 364: N₂O oxidation is not the cause of the observed trend, nor of other global parameter changes. – this sentence sounds odd, please rephrase

Figure 10: Please add that these are AM DOAS measurements (see also my comment above).

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2017-395>, 2017.