Interactive comment on Hemispheric asymmetry in stratospheric NO_2 trends by M. Yela et al.

Reply to Interactive comments from reviewer #2

We thank you the reviewer for his/her helpful comments. For clarity, our responses to the reviewer comments are in blue.

Anonymous Referee #2

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This paper presents sound analysis of four data series that, in keeping with NDACC requirements, have measured stratospheric species to the best available standards over decades. The literature is well surveyed, and the analysis techniques follow established practice, but in doing so the paper does not add much new insight. The results are at odds with cited previous studies, and that should be discussed further. My main criticism is that, starting with the title, the representativeness of the sites seems to be exaggerated; one site in the Canary Islands serves as a proxy for the northern hemisphere, while three sites in Tierra del Fuego, the Antarctic Peninsula, and the southern Weddell Sea purportedly represent the southern hemisphere. It would be better to acknowledge the respective limitations, especially as the NDACC as a whole gives much wider (if still irregular) coverage.

While certainly a subtropical station is not representative of the NH, all 4 NDACC GB stable instruments with long data series support the hemispheric asymmetry in stratospheric NO2 at different latitudes previously observed by satellite instrumentation. The title highlight what we think is the major outcome of the paper: The changing behavior of the stratospheric dynamics in last decades. The stations are already enumerated in the abstract, so we think there is no misunderstanding.

Both latitudinal and longitudinal gradients may be present, as implicit for Antarctic vortex displacement toward South America, but also cited as a feature of Gruzdev's 23-station analysis in 2009 ("Trends were found to be mostly positive in the middle and low latitudes of the SH and mostly negative in the European sector of the middle latitudes of the NH" - both different from this study).

Latitudinal and even longitudinal gradients in the tracers distribution in the stratosphere are a well known feature. NO_2 display a strong meridional gradient during solstice due to differences in illumination time. Longitudinal small gradients can be present as well due to the quasi-stationary planetary waves, but we don't see the reason why this should affect the long-term trends. On the other hand Gruzdev data analysis ends in 2007 and we found that last years have a strong contribution to the observed trend.

It may also be that the difference depends heavily on the analysis period. The Izaña residuals in Fig 2 to 2008, as comparable to a 2009 analysis, show no upward trend, and the Ushuaia and Marambio data to the same time show indistinct trends.

Time period determine the magnitude in any analysis trend. In figure 5 the Izaña trend resulting from calculation using different time periods show that the trend remains essentially unchanged if the data series is shortened by up to 5 years at the start and up to 4 at the end, but also that most of the trend resulted from the increase in last decade. A similar situation occurred in the SH. (See Fig.2 and text in lines 255-256).

On the subject of time periods, the authors should perhaps also mention why their paper in mid 2017 only includes data to the end of 2014. My guess is that it represents a lengthy period of trying different analyses in what is routinely a frustrating attempt to tease meaning from such datasets, but it would still be better for the 25 years of NDACC special issue to use the 24 years of data from Izaña and 23 from the others.

The reason why the data series ends at the beginning of 2015 is that the work was mostly elaborated during 2015 and delayed later on due to a number of reasons. Adding 2 more years of data (10%) at the present time implies a complete recalculation that authors do not consider justified enough since it would produce only small changes in the magnitude of the results not compromising the main results of the paper.

Specific points:

Lines 41-43: The technique of zenith-sky DOAS was pioneered by Noxon, and the first regular monitoring was at Lauder, continuous from 1980, and later from Scott Base.

We corrected this omission. Authors are well aware that Lauder station is the first regular NO_2 monitoring station. The sentence now stands as: *"Regular monitoring of stratospheric NO_2 started in 1980 with the deployment of Zenith DOAS (Differential Optical Absorption Spectroscopy) scanning spectrometers at Lauder, New Zealand by* Mckenzie and Johnston, 1982 *followed by two Antarctic stations (Scott Base at 78° S by Mckenzie and Johnston, 1984 and Dumont D'Urville at 66° S by Pommereau and Goutail, 1988)".*

We have included reference: McKenzie, R., L. and Johnston, P.V.: Seasonal variations in stratospheric NO_2 at 45º S, Geophys. Res. Lett., V9, p. 1255-1258 , 1982

Line 106: "... degradation ... at a rate of 4.33%/year." Confidence limits on this figure are relevant to the inferred trends.

The scanning spectrometer initially installed in 1993 is still in operation. We added the PDA and CCD to improve the accuracy and to extend the spectral range toward the UV and IR. We do not estimate a larger error due to degradation since it was a very slow and linear effect, and was corrected with the scanning-photomultiplier spectrometer operating in the same site (430-450 nm band).

Line 147: The second beta terms should be subscripted 2k, not 2k-1.

Corrected.

Line 154: "... since the start of measurements, in months". The scaling of t by 2pi/12 in the trigonometric terms of the equation means that t must be expressed in months.

Added

Line 156: "annual, semi-annual, and four-monthly waves". A quarterly term would have k=4.

Quarterly term has been removed from the analysis and text since it has no impact at all.

Line 160: "alpha > 0.1" is less conventional than p > 0.1 to express significance level.

We simplified the sentence *"to exclude the proxies exceeding α > 0.1, corresponding to significant values of less than 90%"*. Now it stands as *"to exclude proxies with confidence intervals below 90%"*

Line 178: "in the first few years of the time series, strongly affected by Pinatubo, which would otherwise bias the trend" would make it clearer that the bias was a genuine geophysical effect rather than a measurement or statistical artefact.

The sentence has been modified: "SA *proxy accounts for the short-range contribution of the volcanic aerosols on the NO_2 column which would, otherwise, affect the long term trend".*

Line 197: "negative trend (Schwarzkopf and Ramaswamy, 2006)", rather than "i.e." Alternatively, "e.g." might have been meant here.

Corrected.

Line 244: "large nitrate aerosols load injected following Pinatubo's eruption". Pinatubo injected 20 Mt of sulphur dioxide, which was oxidised to sulphate aerosol over a few weeks. There was no large injection of nitrate aerosols by Pinatubo, or following it.

This is a miswriting. It is obviously sulphate.

Line 247: "quarterly" is "four-monthly".

The K=3 term does not affect anything and has been removed.

Line 265: "negative decadal trend of -5.9 ± 1.5%" would be clearer, without any risk of tautology.

Changed.

Lines 303-306: "The negative trend in the SH increases ... At Ushuaia, the negative trend is reduced to values close to the annual minimum (-7%), whereas in Marambio, the trend increases with respect to previous months". Talking of minima, and reductions, in negative trends is unnecessarily ambiguous. While the reader can perhaps correctly assume a reduction in the absolute value of a negative trend is meant, it is better to choose different words: "The downward trends in the SH become more negative further south ... At Ushuaia, the trend is less negative ...".

The proposed sentence is clearer. We have modified it.

Line 340: "The MIPAS data have been combined with the DOAS averaging kernels ...", or "converted", rather than "corrected".

Changed

Line 348: Fig 10, lower panel, does not show similar seasonalities; that would require a monthly plot like Fig 8.

"*annual means*" in line 347 should read "*monthly means*". Lower plot displays the monthly means. This has been corrected.

Line 351: "MIPAS decadal trend of +3.0 ± 0.4%" appears as 5.0 ± 3.5 in Table 5. Please check all such figures.

Figures in Table 5 are correct. We have corrected the text in line 351.

Line 364: "... evidence that N_2O oxidation is not the cause ..." Please clarify. Is that argument that N_2O is sufficiently well mixed that any change would expect both hemispheres the same?.

Global NO_2 increase was expected at the beginning of the analysis due to the very long N_2O lifetime at a maintained increasing rate of 2.5%/decade. We have reformulated the sentence to make it clearer: *"The opposite sign in the NO_2 trends observed at the NH and SH stations shows that the NOx distribution in the stratosphere does not directly reflects the increasing N_2O in the atmosphere, at least when individual stations are analyzed".*

Line 369: "Negative trends were found in the lower stratosphere ..."

Corrected

Line 391: "... change of sign between hemispheres in the stratospheric trend" would be better, except for comments above about whether the four sites really represent their respective hemispheres.

Corrected.

In Table 5, please check the calculation of the "T statistic" (usually "t statistic") as the values appear to be at odds with the trends and their standard errors.

The stated errors are not the standard errors but the trend estimate errors based on Weatherhead et al, 1998 which account for the data autocorrelation. Since the t-statistics only contributes to generate confusion, we have removed that column from the table.

NOTE: Ushuaia data in figure 2, 4, 5, 6 and 8 are slightly different than in previous version due to the correction of few data from a recent quality control.

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