

Review on ACP Manuscript No acp-2023-2877 Revised

Observational and model evidence for a prominent stratospheric influence on variability in tropospheric nitrous oxide by Nevison et al.

The manuscript has significantly improved and I appreciate the effort the authors have put in shortening and better structuring their paper. I have only some minor comments that are left and should be considered before publication.

Abstract: The abstract is much better now, but I still have some issues with it. For example the introductory two sentence you provide are somewhat independent from what you write in the next few sentences. To understand this connection the reader needs to be either an expert on the topic or have to read the paper first.

Some thoughts on the abstract:

- I would suggest to make the transition at line 25 a bit smoother. I think my major problem here is that you as motivation mention ENSO, but then discuss the results you get from the model without mentioning ENSO again until you come to the results concerning the correlations.
- Isn't the point here that you investigate the stratospheric influence on the seasonal cycle? Or are you investigating all processes that influence N₂O cycle? This did not come really across in the abstract.
- ENSO is a tropical circulation, but then you discuss the influence of BDC and polar descent on the seasonal cycle how do then these processes fit together?
- My suggestion for the abstract would be: 1. Introductory sentence, 2. Data/model that are used, 3. There are hemispheric differences and then provide at the end your results and then the closing sentences.

Specific comments:

P1, L22: What forcing?

P1, L23: Which issues?

P1, L28: What is meant with similar cycles?

P2, L41: delete "ozone-depleting substance" since to my knowledge O₃ is not directly reacting with N₂O, but due to the conversion of N₂O to the photolysis, ozone is destroyed by the resulting products and thus it is indirectly depleting ozone. If you would like to keep the sentence as is I would suggest to add the rereference of the paper by Ravinshankara et al. in Science (<https://www.science.org/doi/10.1126/science.1176985>).

P4, L90: Here it is not clear from your sentence if Ruiz et al. (2021) did not find such an influence in the SH or if they did not investigate the SH. Thus, I would suggest to rephrase the sentence so that this becomes more clear.

P5, L118: add "can be" so that it reads "can be distinguished from tropospheric tracers....."

P5, L122: Is the aircraft data only used/available for the NH? Thus, SH solely based on model data? Please state more clearly what data has been used for which hemisphere.

P5, L124: reflects? I would rather write "is used as tracer for".

P6, L160: Again I have to ask why Mauna Loa is used. You provide an answer in your reply to my comments, but you have not added a reasoning here. I would suggest to add here a short explanation why you picked Mauna Loa and not one of the other stations.

P7, L178: you mean “approach the end of their lifetime” or are these instruments just getting old? In the latter case I would rather write “as the instruments are aging” than “as the instruments approach their lifetime”.

P7, L187: Listed above? Which ones? Are these the same ones as for NOAA? Please rephrase the sentence to be more clear with which stations you mean.

P7, L208: See my comment at P6, L160. Again here you mention that Mauna Loa has been used for detrending the data, but without given a reason. You should at some point in the manuscript provide one sentence why Mauna Loa is used and not another station.

P14, L336: What one can clearly see from the cross sections is the downward transport of the air. But I have difficulties to see horizontal transport and mixing. Since this are known transport processes I would suggest to add here some adequate references.

P17, Figure 5: Why do you use for the aircraft data a different color scheme than for the other datasets?

P19, Figure 6: Not for all panels the p value has been added.

P29, 618ff: You are not really providing here a summary at least not in the sense what was the aim of the study and what has been done. Further, the order as you discuss the results in your summary and conclusion is somewhat weird. Why do you start with the aircraft data instead with the model and NOAA data which are the main data sets of your study? Further, the discussed results cannot only be derived with aircraft data. This can be achieved with other measurement data sets as well.

Additional comment: Concerning my comment on P21, L473 of the previous version of your manuscript concerning if the reference of the text book by Holton (1995) is still valid for the hemispheric differences in the BDC. First of all, here you should rather cite the 2nd edition of the text book published in 2006 or check the following papers by Garny et al. (2013), Butchart et al. (2014) or Fu et al. (2019).

Technical corrections:

P7, L191: Empirical background -> empirical background

P8, L211: Abbreviation HIPPO not introduced. It's done on L219, but this should appear at the first instance where the abbreviation is used.

P8, L223: Abbreviation ORCAS has not been introduced.

P17, L390: “an annual sequence” appears twice. One is thus obsolete.

P17, L395: Add here the section number.

P19, L425: Since all the correlations you consider are rather weak I would suggest to omit the term “strongest”. I would rather use the term “highest”. Further, when the correlation is negative you should either clearly state that this correlation is negative or call it an anticorrelation.

P20, Figure 7: Also here in some of the panels the p-value is not given.

P21, Figure 8: The grey lines are hardly visible on a printout version of your manuscript. Please use a somewhat darker grey for these lines.

P25, L515: The reference Khosrawi et al. (2009) is missing in the reference list. Instead you still have there the Khosrawi et al, (2013) reference which is actually not cited.

P26, L541: Didn't you state before that the strongest correlation for the QBO is found at 50 hPa? Please check the numbers and levels if everything is correct and consistent discussed.

P29, L617: "Summary and" should also be in bold face.

P31, L665: Check the formatting of the references. Indents for the consecutive lines of each reference are missing and different style for the references is used. This should be done in a uniform style and according to the ACP guidelines.

P33, L754: Reference Khosrawi et al. (2013) appears twice, but has not been cited in the manuscript. Further, Khosrawi et al. (2009) which has been cited in the manuscript is not listed in the reference list.

Note: Figure should appear as Fig. in the text, except at the begin of a sentence (see ACP manuscript preparation guidelines).

Supplement, 2nd page, 2nd paragraph: What do you mean with "a year prior"? Do you mean "a prior year"?

Supplement: Figure captions -> remove Supplement before S1, S2, and S3.

References:

Butchart N., The Brewer-Dobson circulation (2014), *Reviews of Geophysics*, 52 (2), pp. 157 - 184, DOI: 10.1002/2013RG000448.

Garny H., Bodeker G.E., Smale D., Dameris M., Grewe V. (2013), Drivers of hemispheric differences in return dates of mid-latitude stratospheric ozone to historical levels, *Atmospheric Chemistry and Physics*, 13 (15), pp. 7279 - 7300, DOI: 10.5194/acp-13-7279-2013.

Fu Q., Solomon S., Pahlavan H.A., Lin P. (2019), Observed changes in Brewer-Dobson circulation for 1980-2018, *Environmental Research Letters*, 14 (11), art. no. 114026, DOI: 10.1088/1748-9326/ab4de7