Atmospheric Chemistry and Physics Discussions

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Comment on acp-2022-689

Anonymous Referee #2

Referee comment on "South Pole Station ozonesondes: variability and trends in the springtime Antarctic ozone hole 1986–2021" by Bryan J. Johnson et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-689-RC2, 2022

Review of Johnson et al., *"South Pole Station ozonesondes: variability and trends in the springtime Antarctic ozone hole 1986-2021".*

General Comments

In this manuscript, Johnson et al. provide an overview of results from the long-running ozonesonde program conducted by NOAA at the South Pole station. This is an incredibly valuable dataset for atmospheric science and everyone involved should be congratulated for simply keeping the program running for such an extended period, not to mention maintaining a high standard of quality over multiple decades.

There are two main area of focus of the manuscript, firstly presenting details of the three specific years 2019, 2020 and 2021, and secondly, showing trends in various metrics derived from the observations over the full course of the program.

Both subjects are of interest and importance, but the long-term behaviour much more so because of the unique ability of the South Pole ozonesonde program, as against other observing systems, to provide high-resolution in-situ measurements from the core of the Antarctic Ozone Hole.

The subject is certainly central to the range of Atmospheric Chemistry and Physics and I definitely recommend publication.

The scope of the manuscript is essentially limited to presenting observations and simple

derived quantities (such as loss rates), rather than attempting any attribution (although refer to my comment below about LOTUS).

Although temperatures are visually presented along with ozone in all cases (ie Figure 2 upper and lower panels, Figure 3 upper and lower panels, Figures 6&7, Figures 8&9) showing qualitatively the very tight relationship between these two quantities at both intra- and inter- annual scales, there is no quantitative analysis of this relation.

The manuscript is very clearly written and well referenced.

My only substantial negative comment of substance is on the topic of using the LOTUS regression model to establish trends in the crucial 14-21 km partial ozone column (Figure 4). By using the term "LOTUS model" the reader will understand this to mean you are regressing the ozone partial column against the LOTUS proxies including QBO, ENSO, Solar cycle and aerosol loading. (If you are in fact not using these proxies then you shouldn't say you are using the LOTUS model). These proxies, while selected by LOTUS for understanding vertically resolved ozone in the mid-latitudes and tropics, are still relevant for Antarctica, however if you want to use them you have to give a much lengthier description of how the 14-21 km partial column is related to each of them. These relations will be quite different to those found in the mid-latitudes. You would also want to make sure the proxies are relevant for Antarctica (eg the aerosol loading and the lag of the QBO, ENSO and Solar Cycle). The trend result will not have convincingly accounted for the effects of these factors unless you provide much more detail of these regressions.

Doing this properly would require a not insignificant expansion to the current manuscript, so I offer as suggestions two alternative approaches, either to adopt a much simpler regression using only one well-established proxy such as eddy heat flux, or just calculate the trend and don't attempt to attribute its cause.

Specific comments

Line 17 – I think you could highlight the fact that the trend in the mid-September 14-21 km partial column is now significant at the two sigma level. This seems to me quite an important finding.

Line 41 For a specific reference like this you should refer to the particular chapter (in this case chapter 4 - Langematz & Tully et al. see full reference below)

Line 57 "Most" doesn't seem correct, eg ground-based and satellite lidars and microwave instruments

Line 73 You can check the style guide but I think "cc" would be better as cm^{-3}

Line 74 You should explain where the value of 0.07 comes from.

Line 79 You should explain what you mean by "18k-19k"

Line 88-89 Do you mean the previous GAW report? (Noting the publication dates you give as the homogenization in 2018 'following' the report of 2021).

Lines 95-97 Yes, but the Dobson and its algorithm has known problems, such as the temperature dependence of the cross-sections, calculation of the airmass and stray-light – for an extreme location like South Pole I would have the thought the uncertainties would be at least 2% (but I might be wrong), in other words this offset might be well within the uncertainties of the two measurement systems.

Lines 100-103 The current recommendation (from the GAW Reports) is:

McPeters, R. D., and Labow, G. J. (2012), Climatology 2011: An MLS and sonde derived ozone climatology for satellite retrieval algorithms, *J. Geophys. Res.*, 117, D10303, doi:10.1029/2011JD017006.

The climatology is based on ozonesondes and MLS so polar night shouldn't matter. If you have found you get better results with extrapolation of the mixing ratio you should say that.

Lines 101-103 I'm afraid I don’t follow the reasoning here.

Line 109 Explain "Met Service" for an international readership.

Line 111 I don't understand why you say the comparison "is important in the dark and coldest winter months"? I would expect the disagreement would be worst in sunlight due to radiation bias.

Lines 113-116 You've talked about the temperature but I don't think you make any quantitative use of temperature data anyway? What about pressure, which is also known to have problems in the older radiosondes? Couldn't that also introduce an inhomogeneity in your time series when you are reporting on pressure levels or altitudes which would have been calculated using pressures?

Lines 185-187 As written in more detail in the "general comments" above, if you are using the LOTUS model then you have to give details of how you treated the proxies and what you found for the dependence on the 14-21 km partial column on 15 September of each of the proxies. As well as pinning this down being a very interesting scientific question, it's also important to know because the credibility of the trend depends on how well the regression has accounted for the explanatory variables.

Lines 204 You don't really explain to the reader why it is appropriate to be looking for a linear fit, so I suggest referring here to figure 3 would be helpful.

Lines 211 "improved" seems too value-laden (but I appreciate when talking about the loss rates "increase" and "decrease" can be confusing).

Lines 211 You say the minimum of -3.8 DU/day – wouldn't this be the maximum of the loss rate, not the minimum?

Lines 224-225 I don't have any objection to the grouping into 5 year blocks but I do have some comments. (i) If you're trying to avoid biennial influence, don't you need an even number of years not an odd number? (ii) Saying "other dynamical processes" is very vague – what exactly do you mean here? Couldn't there be a confounding trend in these "other dynamical processes"? (iii) In fact, no biennial effect is very evident in figure 5 (iv) Presumably, you have already accounted for the QBO influence when you used the LOTUS regression model - this could be referred to here.

Lines 229-236 It is not clear to me what the purpose of figure 7 is – is it to support figure 6 by showing the absence of temperature trends? Or perhaps to put 2019, 2020 and 2021 into context?

Line 255 Interestingly the lower boundary did not seem to be moving downwards during this time.

Lines 238 Antarctic ozone hole

Lines 266 58 km! (Carr et al.)

Line 267 Probably more pertinently, unprecedented water vapor in the stratosphere – Millan et al. 2022, Vömel et al. 2022

Lines 274 As mentioned earlier, I would reword this slightly to say a bias with respect to the Dobson of 2± 3% - the Dobson would have considerable uncertainty of its own.

Line 290 I think you could be a little more definitive given the trend is significant at the 2 sigma level.

Line 321-323 It is very good to acknowledge the sad loss of your colleague in this way.

Line 540 Have you tried to calculate the effect the homogenization has had on the trends? By eye It looks like the 1985-2000 values have on the whole been decreased and 2000-2005 increased.

References

Carr, J. L., Horvath, A., Wu, D. L., & Friberg, M. D. (2022). Stereo plume height and motion retrievals for the record-setting Hunga Tonga-Hunga Ha'apai eruption of 15 January 2022. *Geophysical Research Letters*, 49(9), e2022GL098131. https://doi.org/10.1029/2022GL098131

Langematz U, & Tully M (Lead Authors), Calvo N, Dameris M, de Laat ATJ, Klekociuk A, Müller R, & Young P (2018). Polar stratospheric ozone: Past, present, and future, chapter 4 in scientific assessment of ozone depletion: 2018, global ozone research and monitoring project–Report No. 58. Geneva, Switzerland: World Meteorological Organization. [Google Scholar]

McPeters, R. D., and Labow, G. J. (2012), Climatology 2011: An MLS and sonde derived ozone climatology for satellite retrieval algorithms, *J. Geophys. Res.*, 117, D10303, doi:10.1029/2011JD017006.

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Vömel, H., S.Evan, M. Tully (2022). Water vapor injection into the stratosphere by Hunga Tonga-Hunga Ha’apai. *Science* Vol 377, Issue 6613 pp. 1444-1447, DOI: 10.1126/science.abq2299