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Interactive comment on "EPP-NO_x in Antarctic springtime stratospheric column: Evidence from observations and influence of the QBO" *by* Emily Gordon et al.

Anonymous Referee #2

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Gordon et al. 2019 present a study of the Antarctic springtime stratospheric NO_2 column from Aura/OMI measurements, and they correlate those columns to the geomagnetic Ap index and the QBO. According to the authors this is the first study to use the OMI NO_2 column data product in a study like this. As middle atmospheric physics and chemistry is part of ACP, the manuscript fits the scope of this journal. However, in my opinion some revisions are necessary before the manuscript fits the quality standards of ACP.

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Discussion paper

General comments

Although the data and methods used are sound and the authors have taken great care to be in line with earlier studies that have investigated the EPP indirect effect, some questions on the methods need to be clarified.

1 Parts of the paper seem to be disconnected and there does not seem to be a clear logical thread to guide the reader. Some of the sections seem to focus on describing figures without explaining why they are relevant to the study and how they relate to the other sections. In particular Sect. 3.1 (esp. Fig. 3) seems unrelated and not relevant to the rest of the paper.

2 There seems to be a mixture of terms throughout the manuscript, "NO_x", "NO_y", and "NO_2" seem to be used interchangeably. For example the title states "EPP-NO_x" whereas the study focuses on NO_2 only. Although according to Brasseur and Solomon, 2005, NO_2 makes up around 80% of NO_x in the stratosphere, this fact should be noted. The EPP part is not defined at all, Funke et al. 2014a,b use tracer correlations and Randall et al. 2007 use CH_4–NO_2 correlations to identify *EPP*-NO_y, how do the authors discriminate between EPP and non-EPP NO_2? A clear definition of these terms and how the authors use them should be given in Sect. 2.

3 Do the authors average the 3h Ap or the daily mean Ap? Although other studies use average Ap as well (e.g. Funke et al. 2014a), Ap does not follow a normal distribution and the authors should be aware of that when using the mean as an estimator. This non-normality manifests itself in a very skewed distribution and a large standard deviation, particularly the 3h values. Has this been considered in the correlation analysis? I suggest that the authors check that the mean is a valid estimator for the distribution or cite a relevant publication. I also suggest to present the $\hat{A}_p$ values with error bars in Figs. 2, 4, 7, and A1 and Table 1 (probably based on appropriate quantiles).

4 Is the 60°–90° average area weighted? If it is, it should be stated somewhere, Sect. 2

seems the obvious place (l.112). If not, higher latitudes may be artificially amplified in the polar cap average column. And in that case a discussion would be needed to assess the possible differences when taking area weighting into account.

5 In Sect. 4.1 the authors discuss the possible impact of out-of-vortex air on reducing the correlation between $\hat{A}_p$ and the NO_2 column in October. Why is this presented in the "Discussion" section and not the "Results" section? As the authors seem to have an indication about the actual vortex available to them, why isn't the whole study based on vortex averages instead of whole polar cap averages? That would remove the ambiguity of including non-EPP-NO_2 from horizontal transport/mixing in the polar cap average.

Specific comments

Abstract

- ll.9–11: Does it really contribute to NO_2 or is it just the fraction that changes due to a varying background? I suggest to rephrase these sentences to be clearer, for example how is it linked to the ozone hole? What is cause and what is the effect? See also my other comments below.

Introduction (Sect. 1)

- I believe the introduction would profit from some additional subsections, e.g.:

- ll.33–48 "EPP indirect effect"
- ll.49–83 "Previous work"/"Earlier studies"

- ll.84ff "This work"

- ll.85–86: This is a repetition and can be removed.

- l.87: A verb is missing: "... this *is* detectable ..."

- The authors do not mention in the introduction that they are going to use (MLS) HNO_3 observations, and how they are going to be used. HNO_3 is only mentioned in relation to other studies, see also my next point.

- At the end of the introduction, a guide through the manuscript connecting the parts to the objective raised in the abstract would be helpful, i.e. something like: "We use the NO_2 column data and anomalies correlated to Ap and QBO to assess the impact..." and "To identify another possible mechanism contributing to the stratospheric EPP-NO_x variability, we evaluate MLS HNO_3 according to the QBO phase during the same period."

Observations and methods (Sect. 2)

- I couldn't find any methods presented here.

Sect. 2.1

- are the latitudes geographic or geomagnetic? I assume that the authors refer to geographic latitudes, for completeness, I suggest to state this somewhere in the (sub)section.

- l.97: I suggest to add some more details about the Aura satellite, such as orbit altitude, inclination, period, and local time.

- ll.102–104: I suggest to use: "*The latitudinal* coverage is illustrated... . *The figure*

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shows ..."

- ll.105–107: This is a repetition of the earlier statement and can be removed.

- l.112: Noted in general comment 4, have the measurements been weighted according to their area when calculating the polar cap average (using cos(latitude) for example)? How do the authors account for the lack of measurements north of 70° during Aug–Sep? Has any correction been applied or is it implicitly assumed that the NO_2 column is constant (or zero) there? The latter is probably wrong, judging from the curved contours in Fig. 1.

- Table 1: Please indicate a range for all the values, for example using $\pm$ (2$\times$) standard error of the mean as in Fig. 4 or appropriate quantiles.

Sect. 2.2

- This section appears seemingly without relevance (see comment above about the introduction). It only becomes clear later in Sect. 4.3 when the authors discuss the possible influence of denitrification due to the formation of PSCs. I suggest to better explain how the data are relevant to the study.

- l.116: Geographic or geomagnetic latitudes? I suggest to state that somewhere at the beginning.

Sect. 2.3

- Mentioned in general comment 3, Ap has a non-normal distribution, how do the authors deal with that?

- l.122: the reference should be probably to Funke et al., 2014a instead of b.

- l.132: How was the confidence interval estimated?

Sect. 2.4

- Strahan et al., 2015 use a different definition of QBO which results in a different division of eQBO and wQBO years compared to the one presented here. The authors should comment on that and how it would influence the results (see also below).

- ll.136–137: This sentence is confusing, "take" does not seem to be appropriate here, please rephrase.

- Fig. 2: Error bars and a $\hat{A}_p = 8.5$ line would be helpful to visualize the Ap ranges and the division into low and high Ap years (only needed if Sect. 3.1 is kept in the manuscript, see below).

Results (Sect. 3)

- A little guide through the results would be helpful, as in "We investigate anomalies to assess ...", "Then, polar averages are correlated in order to ...", "Latitudinal correlations are used to ..."; either at the beginning of Sect. 3 or at the beginning of the respective subsections.

Sect. 3.1

As mentioned in general comment 1, this section does not seem to play a role in the rest of the manuscript and raises a lot of questions. For example, I count only two years (2005 and 2012) for panel (a), five (2007, 2009, 2010, 2014, and 2017) for panel (b), and three each for (c) and (d). How robust are those means then? How does it vary with the choice of QBO definition? Strahan et al. 2015 list 2011 as eQBO, not wQBO, how does that affect the results? How robust are the results with respect to the Ap distribution? 2017 for example could also be a high-Ap year (it is close), how would that change Fig. 3?

- l.142: "... the mean deducted ..." What mean? The mean as shown in Fig. 1? If yes, please refer to that figure.

- However, I suggest to remove that section entirely and to start the results with the scatter plots in Sect. 3.2. The split into high and low Ap is not used later, the authors then only divide into eQBO and wQBO years.

Sect. 3.2

- Fig. 4 caption: "*The* yellow line ..."

- l.151: Again, please indicate if the data have been weighted by the area. It is only needed once, though. And again, what about the missing data in Aug–Sep?

- l.153: How was the linear fit achieved? Were the data weighted by their uncertainties or not? What about uncertainties in $\hat{A}_p$? Please be more specific here, in particular since this is later related to Funke et al., 2014a.

- l.156: "... [not] fully encompass the entire polar region ..." How do the authors deal with it? Are the averages calculated only up to 70° in those cases? Is the missing area filled with a constant value or even with zeros? I suggest to clarify these points.

- l.162: "... have consistently lower NO_2 column values, especially in August–September." May this be the result of omitting higher latitudes or implicitly replacing them by a constant or even zero? What about the influence of area (cos(latitude)) weighting?

- ll.163–174: Related to my general comment 2, how do the authors define the EPP part of the measured NO_2 columns? Why is Fig. A1 put into a non-existing appendix and not included here? I suggest to move that figure here as Fig. 5. Why not use the same Ap weighting scheme as described in Funke et al., 2014a? Note that they used

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that procedure for a reason and it would make the two studies really comparable on an absolute scale.

Sect. 3.3

- l.182: Again, what part of the OMI NO_2 column is **EPP**-NO_x here?

- l.185: How was the significance determined? Similar in caption of Fig. 5.

- l.186: I suggest to replace "from Fig. 5" by "*shown in* Fig. 5".

Discussion (Sect. 4)

- l.192: I suggest to add an article "... presented in *the* previous sections ..."

- l.192 cntd.: "less significant" than what? Using the frequentist language as in the other parts of the manuscript, the results are either significant or not (according to the chosen significance level). Do the authors mean "less correlated" (ρ is around zero)? Or: "[the correlations] ... are less clear/smaller/weaker"?

- l.195: I suggest to remove "the month of".

- ll.196–211: As suggested in general comment 5, the study could be based on the polar vortex averages instead of the polar cap mean. I also suggest to move this part to the results, not the discussion.

- l.205: What about the vortex shape variability in other months?

- ll.208-211: I couldn't make any sense of that rather convoluted sentence, I suggest to rephrase it to be clearer; "thus" seems to be the wrong word here.

- l.212: The word "now" seems to be misused, I suggest to use "*in our study*".

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- ll.213–214: Leaving the complications with Ap aside, the implication is only valid if the authors have a particular model/mechanism in mind that "generates a proportional response". Without that model or mechanism, the results merely *suggest* this response. I recommend to soften the wording accordingly, or to present a clear mechanism that links cause and effect.

- Fig. 6: Is this the October OMI ozone average column using all years? Or just one example month? What about the year-to-year variability of the vortex shape?

Sect. 4.2

Since this is the "discussion" section, the influence of the different QBO definitions should be discussed. The decreased N_2O concentrations were observed in the average eQBO according to their (Strahan et al., 2015) definition of QBO (which is different from the one used here). Similarly, the mechanism that connects N_2O and NO_2 could be repeated to make clear why the Strahan et al., 2015 study is relevant here.

- l.216: I suggest to swap "the" and "that".

- l.218: I suggest to remove "clearly".

- l.220: I suggest to replace "more" by "*a larger fraction*".

Sect. 4.3

Fig. 8: The panels are missing the (a), (b), and (c) indicators to be consistent with the figure caption. Caption (b): "anomaly from the mean", I assume the 3-day mean as shown in panel (a) is subtracted, please clarify that.

- l.222: I don't understand this sentence, what is meant by "the affected transport"? I suggest to rephrase that sentence to be clearer, and to remove "obviously" from it.

- l.224: An article seems to be missing: "*A* colder polar vortex ..."

- l.225: "As discussed earlier", where? A reference to the relevant section would be helpful.

- l.226–228: This sentence is hard to understand, I suggest to rewrite it, for example using *Thus* or *Therefore* instead of "So".

- l.234: I suggest to use "*down* to -1 ppbv" and to remove "clearly".

- ll.235–236: If PSCs are really responsible for the loss of HNO_3 due to denitrification, have the authors considered additional observations of e.g. PSC fraction or temperatures during eQBO or wQBO that would support that mechanism? I suggest to include a short comment or reference.

Conclusions (Sect. 5)

- ll.244–248: I suggest to move that part or a some version of it to the discussion section as it summarizes the assumed mechanisms. It would also fit at the end of the introduction to help the reader to understand the purpose of the study.

- l.252: This is a confusing sentence, how does the ozone hole suddenly come into play?

- ll.256–259: This conclusion is stretching it a bit too far in my opinion. According to the presented study, the EPP-NO_x (in form of NO_2) does not change with QBO phase. Instead, the background NO_2 changes due to source and sink changes. As a consequence, the fraction of EPP-NO_x (NO_2) on the overall amount varies with QBO phase. The authors may consider rephrasing their last conclusion a bit, such that the larger EPP-NO_x *fraction* may need to be considered when considering the net effect of NO_2 on ozone chemistry (resp. recovery).

References

- There are two Seppälä et al., 2007 references listed, they should be separated with (a) and (b). They are referenced in ll.34 and 76 at least, which is which?

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