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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Continuous decline in lower stratospheric ozone offsets ozone layer recovery" *by* William T. Ball et al.

Anonymous Referee #2

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The authors use a new dynamical linear modelling method to identify slowly varying trends in the global ozone profile. They find increasing ozone in the upper stratosphere since the late 1990s, little change in mid-stratospheric ozone since the 1990s, but significantly declining lower stratospheric ozone over the entire 1985 to 2016 period. These results are generally consistent with a number of recent ozone trend studies. However, this study is the first to focus on the lower stratospheric decline, whereas many other studies do not show significant decline in the lower stratosphere, and/or do not focus on this region. The decline in lower stratospheric ozone would explain why, so far, no significant increases in total column ozone have been observed, despite the decline of ozone depleting substances since the late 1990s, and despite expectations from model simulations. Model simulations, in fact, indicate that lower stratospheric

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ozone should be increasing. As pointed out by the authors, a decline in lower stratospheric ozone, as reported here, seriously questions our understanding of global ozone trends, and our ability to model them.

1 General Comments

Overall, this is a good paper, well suited for ACP, and deserving publication. There are aspects, however, where I am not certain, and where I feel a bit more scepticism would be appropriate.

1. Originators of the merged SBUV (Frith et al., 2017, https://doi.org/10.5194/acp-2017-412), CCI (Sofieva et al. 2017, https://doi.org/10.5194/acp-2017-598) and SOO (Bourassa et al. 2017, https://doi.org/10.5194/amt-2017-229) data sets do not have the same confidence as the authors into the stability and reliability of their ozone records in the lowermost stratosphere. Frith et al., (2017) do not report trends below 30 hPa (25 km), Sofieva et al. (2017) do not report trends below 20 km (50 to 70 hPa). Bourassa et al. (2017) do not report trends below 18 km (70 hPa). Given this, more caution on the reliability of lower stratospheric ozone (147/100 to 32 hPa; 13/16 to 27 km) in this study would be appropriate. Uncertainties in this region are large, and easily exceed 5% (see e.g. Fig. 9 of Sofieva et al.). With such low accuracy, small trends like the one reported here (-2 DU / decade, for a value of maybe 100 DU), of the order of a few percent per decade, are always questionable, and have to be put into perspective.
2. Figure 3 demonstrates steps in the lower stratospheric ozone time series. These might be due to instrumental changes relevant for all merged data sets. The large downward step by about 2 DU in 2004/2005 occurs at exactly the time when the SWOOSH and GOZCARDS merged ozone records switch from the very sparsely

sampling solar occultation SAGE II instrument (operating until 2005, with even more reduced sampling since 2001) to the very densely sampling Microwave Limb Sounder (since 2004), an instrument with characteristics very different from SAGE II. Similar things apply for the CCI data set at the switch-over from SAGE II to ENVISAT instruments around 2002/2003, where, e.g., Figs. 8 and 9 from Sofieva et al. (2017) demonstrate the large changes in sampling and the large uncertainties in the lowermost stratosphere. These changes could be very important for time series and for trends in the lowermost stratosphere. I think the authors should add some more caution here. How do the curves look for CCI and SOO?

3. The tropospheric OMI/MLS column in Fig. 4 does not provide an independent piece of information. It just shows that the difference between OMI total column ozone, which should be very similar to SBUV total column in the present study (and have no trend since about 2000), and MLS stratospheric column (essentially the same as SWOOSH and GOZCARDS used in the present study) has a positive trend. Since upper stratospheric is increasing, this just means that lower stratospheric ozone from MLS (and GOZCARDS, SWOOSH) must be decreasing. While this confirms the findings of the authors, it still hinges on the same MLS data, and does not provide an independent piece of information. Independent information about tropospheric ozone trends must come from somewhere else. However, recent studies show no trend for zonal mean tropical tropospheric ozone based on GOME/SCIAMACHY/GOME2 data (Leventidou et al., 2017, https://doi.org/10.5194/acp-2017-815), or provide little confidence on our ability to identify large scale tropospheric ozone trends (e.g. Cooper et al. 2014, http://doi.org/10.12952/journal.elementa.000029).

I acknowledge that in parts of the manuscript, the authors are mentioning these open questions. However, I do feel that they should be a more integral part of the manuscript. Therefore, I suggest that the authors reword / change parts of their manuscript, to better reflect these open questions. Below, I'll indicate in more detail which specific parts I am talking about.

2 Detailed Comments

Title: Given all the uncertainties, I would put a question mark behind the title.

Lines 6, 13, 14, . . . : I find the abbreviations TCO, StCO, TrCO unnecessary and annoying. Every time I read them, I have to re-think what is meant. I would prefer to have them spelled out, throughout the manuscript: total column ozone, stratospheric column ozone, tropospheric column ozone. Text length would not change.

Lines 14-15: Delete "and harmful to respiratory health". This is irrelevant in the context of the paper. In fact, given the uncertainties mentioned above, I think the entire sentence about tropospheric ozone increase could be omitted, or at least reworded. Certainly, tropospheric ozone changes are not investigated thoroughly in the present paper.

Lines 17 to 20: Not investigated in the paper. The last sentence should be removed.

Line 30: I think a reference is required here.

Lines 32-33: I think we are far from attribution in the IPCC sense. Therefore I would suggest to delete "an attribution", insert "due" before "to decreasing ODS", and replace "possible" by "reported."

Line 34: after "rates" add "and by accelerating ozone transport through the meridional Brewer Dobson Circulation".

Line 36: A reference is needed here.

Lines 37 to 97: This is quite longish and wordy, and seems to have been written in sev-

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eral steps and at different times. I would recommend to shorten and compact this: The paragraph about total ozone (around line 40) should include the newest results from Weber et al. (2017, https://doi.org/10.5194/acp-2017-853). The part about differences between MLR, EESC, PWLT (lines 42 to 60) should be moved to the end (line 98), and should be shortened and combined with the paragraph starting in line 98. The paragraph around line 90 should mention more about the general uncertainties of ozone measurements in the lower-most stratosphere, see also my general remark above. Overall, I think the entire introduction could be shortened by 20 to 30%, because many things are clear to an ACP audience, and are also mentioned again later.

Line 65: Frith et al., 2017, https://doi.org/10.5194/acp-2017-412, should be added here.

Line 77: This could/ should also include relevant references from lines 64, 65.

Line 84: I think this is a key point here: Instrumental uncertainties are 10 to 15%, and the "observed" lower stratospheric ozone decline is only about 2 DU out of maybe 100 DU. Can we believe a 2% effect measured by a system that is only accurate to within 10 or 15% ?

Lines 86-87: There are good reasons, why many of the data providers do not trust derived trends below 18 to 20 km. See e.g. Fig. 9 of Sofieva et al. (2017).

Line 100: The work by Damadeo et al. (2014, https://doi.org/10.5194/acp-14-13455-2014; 2017, https://doi.org/10.5194/acp-2017-575) should be referenced as well.

Line 106, 107: It is no big achievement to not report ozone changes as percentages. Suggest to drop ", i.e. . . . in percentage"

Line 115: The sentence does not make sense. Something is missing here.

Line 121: Replace "trends have been" by "about ozone trends"?

Lines 125 to 140: Reduce duplications with what has already been said in lines 42 to 60.

Line 139: Probably better to say "PWLT" instead of "linear trend".

Line 144: What is the correlation between F30 and F10.7 on the time steps used in the present analysis? What is the correlation of the two proxies with ozone, and are there any significant differences between F30 and F10.7 for this type of ozone trend analysis?

Line 157: Delete "being considered"?

Lines 166 to 170: Since a lot of these data sets have changed recently, e.g. from Tummon et al., 2015 to Steinbrecht et al. 2017, I think it is absolutely necessary to indicate already here which data and versions were in fact used. This may require a small table. Mentioning the SPARC LOTUS initiative, which brought together many of the datasets, would also be a good thing.

Lines 185, 187: I think Frith et al. (2017) needs to be added here.

Lines 191 to 199: It would be better to drop this here, and include the relevant information into the paragraph from lines 166 to 177.

Lines 206 to 213: Given my major comment above, and the general question about relevance / independent information content of the OMI-MLS data set: Maybe drop the entire paragraph? I think a short mention in the description of Fig. 4 would be enough. Only if the authors decide to make a stronger point about tropospheric increases, e.g., by adding an analysis of ozone trends from ozone sounding stations, then a separate sub-section would be appropriate.

Line 239: As mentioned in my major comments, I am still only $\approx$90% convinced that ozone has declined in the lower stratosphere. Therefore, I suggest to replace "clearly indicate" by "give a strong indication".

Line 250: I think more words of caution about the high variability of ozone in the lowermost stratosphere, and about the poorer accuracy of the measurements there (compared to the mid- and upper stratosphere) would be required here. See also Fig. 9 of Sofieva et al. 2017.

Around lines 300, 327: Also compare with / better compare to Weber et al. 2017.

Line 310: Again: This fairly small change by 1.5 DU is challenging the limited accuracy of the instruments, which is around 1% or 3 DU for total column ozone, and around 5 to 10% for the lowermost stratosphere (= 2 to 5 DU, assuming 50 DU sit in the lower stratosphere). A large part of the observed 2 DU drop in the lower stratosphere around 2004/5 hinges on poorly sampled data from SAGE II, at the end of its lifetime.

Lines 331 to 366: Given my major comments about the OMI/MLS tropospheric ozone results, and in favor of conciseness of the paper: Would it not be much better to drop much of this discussion, drop Figs. 4 and A13? Instead just mention possible tropospheric ozone increases from OMI/MLS and other, more independent sources of information and put them into perspective. Essentially, this could be done with an expansion of the paragraph in lines 367 to 376. The main messages of the paper would remain. Questionable information would disappear, and conciseness would be improved.

Lines 418 to 427: Is this paragraph necessary? I think it could easily be dropped. The entire section 4.4 is quite long and wordy. I think it could be shortened and made more concise.

Line 452: Here is one place, out of many, where TCO left me very confused. I was thinking of TCO = tropospheric column ozone, and saw little sense in the paragraph. As mentioned, spelling out TCO, StCO, TrCO, . . . would help readers like me.

Line 475: If you do the numbers, this is still a very small effect for past total ozone columns, maybe 0.2 DU per decade. I think this should be said here.

Lines 458 to 494: Again, I think this is quite long and wordy, and would benefit from

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substantial shortening.

Figure A7: Can you show similar plots for the altitudes where it really matters, e.g. 18 km? And also include SWOOSH / GOZCARDS?

To summarize again: I think this is a good paper. I think it would benefit greatly from addressing my major points raised above. It would also benefit substantially from fleshing out redundancies and shortening the text. When this has been done, I fully recommend publication.

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