

Interactive comment on “On instrumental errors and related correction strategies of ozonesondes: possible effect on calculated ozone trends for the nearby sites Uccle and De Bilt” by R. Van Malderen et al.

Anonymous Referee #1

Received and published: 14 February 2016

1 General remarks

Using long-term ozone records from operational soundings at Uccle, Belgium, and De Bilt, The Netherlands, the paper demonstrates the effects of various correction methods on ozone profiles and ozone profile trends at the two stations. Investigations like this are important and are relevant for answering the questions whether the stratospheric ozone layer has started to recover from the effects of anthropogenic ozone depleting substances, and whether tropospheric ozone levels are increasing due to human activities.

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Overall, the paper is written reasonably well. Results are put into the context of existing work. However, I do feel that some of the Figures could be made much clearer, and that the paper would benefit from clearer focus and clearer statements. The paper would also benefit greatly from streamlining the discussion and editing out redundancies. Currently the paper presents a "smorgas-board" of corrections which do or do not improve agreements between Uccle and De Bilt ozone profiles and ozone profile trends. The reader is left wondering, which corrections really should be applied, and how significant the changes are. Also: How significant are effects of the various corrections on ozone trends? To me it appears that effects on trends are usually small and within the statistical uncertainty of the trends. So overall, I would urge the authors to decide what their clear "take-home" messages are and to focus on bringing these messages out clearer.

2 Suggested larger changes

I think the following changes would make the paper clearer:

Figure 1 is really an important key figure. However: substantial information is hidden by the large amplitude of the annual cycle. I think it would be very helpful to have an additional Figure where the average annual cycle has been subtracted (e.g. subtract the 1998 to 2008 annual cycle), and anomaly time series are shown. This additional Figure might come after Fig. 1, or later before discussing trends (Fig. 8). The average annual cycles for Uccle and De Bilt ECC sondes would also deserve an additional Figure - and this would help discussion of the substantial geophysical(?) differences between the two sites. In my opinion, Figure 1 does not really need trend lines at this point (and much of the trend uncertainty would come from the annual cycle being there). I would suggest to omit the trend lines in Fig. 1. They could be plotted in a Figure with the anomaly time series.

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Figure 5: I find this Figure confusing because it mixes two different things: On the one hand it shows the differences in Uccle ECC sonde profiles caused by different corrections. On the other hand it shows relative differences between Uccle and De Bilt ozone profiles (cyan and green lines). I think De Bilt vs. Uccle is a different thing, and I would suggest to move the Uccle ECC vs. De Bilt ECC comparison (cyan and green lines) to a separate Figure (e.g. after Fig. 7).

Figure 6: The same argument as for Fig. 5 applies to Fig. 6: The De Bilt ECC vs. Uccle (PRESTO) comparison (dark blue line) is a different thing and should be moved to a separate Figure (e.g. after Fig. 7).

Figure 7: I think this Figure is confusing, because it uses the very unrealistic non total ozone corrected Uccle BM ozone profiles as the reference. I think it would be much clearer / better to use the Uccle PRESTO BM ozone profiles as the reference in Fig. 7. After all, these are probably closest to the true ozone profile as measured by BM sondes at Uccle. The clearly unrealistic non total ozone corrected Uccle BM ozone profiles should be ignored at this stage and should not be used as a reference this late in the paper. Also, it would be very helpful to put in other BM vs ECC comparisons (e.g. the De Backer line from Fig. 10 of Stubi et al., 2008; or results from De Backer et al., 1998a; and/or Smit et al., 1998).

In addition, using the most realistic BM profiles from Uccle to compare with De Bilt, will also help to put the Uccle ECC vs De Bilt ECC comparison (additional Figure suggested by me) into better perspective. If these two comparisons look consistent, they would provide a strong indication that the BM to ECC in Uccle is smooth and not affecting trends very much.

After Fig. 7: As mentioned above, I think it would be good to have a separate Figure showing the various Uccle ECC vs. De Bilt ECC comparisons, currently mixed into Figs. 5 and 6.

Also: The large differences between Uccle and De Bilt (De Bilt 5 to 10

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Figures 8, 9: As explained above, I feel that the "Uc standard pump" and "Uc altitude corr" lines should not be included in these plots. These trends use unrealistically low BM data and give clearly wrong ozone trends. For simplicity and clarity these lines should be omitted.

Also: Is Figure 9 really necessary? It contains more or less the same information as Fig. 10, and a substantial part of the information is also presented in Fig. 8. The period 1993 to 2014 also has no simple geophysical meaning: It is affected by Mt. Pinatubo and it includes the end of the increasing stratospheric chlorine and bromine period, but much of the period has declining bromine and chlorine. I think Fig. 9 could easily be omitted, and I suggest to do so.

3 Minor changes

pg. 2, line 19: I don't believe that many of the ozone-sondes were "calibrated thoroughly" prior to launch. The JOSIE experiment might have provided a thorough calibration for a few ozone sondes. Most ozone-sondes were probably "prepared thoroughly", but only very few were calibrated. Please reword.

pg. 3, line 1: delete "being"

pg. 3, line 3: delete "significantly". In looking at the results from this paper, I wonder how significant the improvements are. I think they are improvements, but the changes are often small/ marginal. Not all changes are certain to correct things and some assumptions, e.g. about "older" backgrounds are just guesses.

pg. 3 lines 16-19: Is this reference still relevant, given all the later De Backer et al. references and the improved BM processing at Uccle? It is certainly in contrast with e.g. Stuebi et al., 2008, who found little systematic difference between correctly processed BM and ECC profiles. Maybe remove the lines, or also mention the Stuebi et al. results.

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pg. 3, line 32: There are much better references for ECC sonde performance than Hassler et al. 2014. Please use Smit et al., 2007 and Deshler et al., 2008 instead of Hassler et al., 2014.

pg. 4, line 21: The authors might want to point out, that this is of minor importance, especially for trends where these effects should remain the same over time.

pg. 4, line 32: While there are references that BM sonde performance depends a lot on preparation procedures, I am not aware of any reliable references that document dependence on material used, changing specifications or changing provider. Please provide references or omit the speculations in the brackets.

pg. 5, line 6: Are these reports of large overestimation of tropospheric ozone by BM sondes consistent with the findings of the Uccle - De Bilt comparisons in this paper? Does the Uccle time series show a 25% drop in upper tropospheric ozone after the switchover to ECC in 1997? Are the differences consistent with the findings of Stuebi et al., 2008? Please comment, here or later in the paper.

pg. 7, line 13: please also state how much $0.1\mu\text{A}$ is in ozone (nbar or ppbv or percent of tropospheric ozone). Also: Does the different background measurement at Uccle (smaller background, before ozone exposure) and DeBilt (larger background, after ozone exposure) explain the observed higher (upper) tropospheric ozone values at Uccle (compare Fig. 5). Could these be due to smaller background subtraction at Uccle, or too high background subtraction at De Bilt (lines 19 and after, Fig. 3)? Please elaborate later in the discussion of the Figures.

pg. 7, lines 24,25: So what was used for the O3S-DQA in De Bilt? The values from Fig. 3? Please make clear. Do the operational and O3S-DQA lines in Fig. 5 bracket "small background subtraction due to scaling by p/p_0 " and "large background subtraction due to using background after ozone exposure from Fig. 3"? If so, then should not the green and cyan lines in Fig. 5 be very close near the ground? I am confused. Please clarify what is done in the background subtraction, throughout the paper.

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Also: How high are the backgrounds in Uccle, in μA , nbar , ppbv . Please provide some numbers.

pg. 11, line 25: Why the year 2000? Fig. 3 suggests that background values only dropped after 2002. Please explain.

pg. 12, line 27: replace "smeared out" by "shifted" or "redistributed", since this quite a systematic process, not a smearing out.

pg. 15, lines 4 to 26: As discussed before, the use of the unrealistic "raw" BM ozone profiles in Fig. 7 is misleading and confusing. This Figure, and its discussion here should be changed - as discussed before.

pg. 17 line 25 to pg. 18 line 17: As discussed before, I think it would help conciseness and brevity of the paper a lot, if Fig. 9 and its discussion here would be omitted completely.

4 Overall remarks

Throughout the manuscript please streamline and shorten/eliminate the repeated discussion of background measurements and background subtraction.

Please dig deeper into the substantial differences between Uccle and De Bilt ECC ozone profiles in both upper stratosphere and troposphere. Are they real? Why? Could they be artefacts, in which case there is a lot to learn here.

Please clarify in the abstract that nearly all the (sensible) corrections change the ozone trends by not very much, and usually within their statistical uncertainty due to atmospheric noise.

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