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Atmospheric Measurement Techniques Discussions

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.

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We thank the reviewer for his/her careful and detailed review of the paper and his/her very constructive feedback. Our response to your questions, comments or suggestions can be found below, with different text formatting for **referee comments** and *author replies*.

General comments

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 pa-

per 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.
We tried to show in the Figures only the relevant and sensible correction strategies. The description of some additional corrections in the PRESTO method, usually extrapolations for the early period of BM sondes, were moved to an appendix and the effect of the individual corrections on the average profiles and trends are not longer shown for those additional corrections. So, the attention of the reader will not be longer distracted by these less important correction steps and redundancies (e.g. discussion of the impacts of the background current subtraction at De Bilt) were removed. We are also much clearer about the significance of the different (sensible) corrections on the trends, and we tried to streamline the most important messages throughout the discussion of the figures and conclusions.

Suggested larger changes

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. substract 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 uncretainty 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.
We took into account all these suggestions: Figure 1 show the monthly mean time series for the different atmospheric layers at both sites, Figure 2 presents the average annual cycles, and the monthly anomaly time series (with trend lines) are included in Figure 10 (before the "Impact on the vertical trends" section). The average annual cycle figure is also used to discuss possible geophysical differences between the two sites.

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).
The Uccle vs. De Bilt comparisons were treated in different figures, as suggested by the reviewer, and not longer mixed up with the impact of the corrections on the average profile at a given site.

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 pofiles 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 Stübi 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.
For the comparison of the average ozone profiles between Uccle and De Bilt, both for the Uccle BM period (1993-1996) and the ECC period (1997-2014), we use the Uccle average profile, corrected with the operational PRESTO algorthms, as the reference in (now) figures 8 en 9. In the discussion of Figure 8, we refer to other studies describing BM-ECC comparison at Canadian sites (Tarasick et al. 2002), Austrialian sites (Lehmann, 2005), at Payerne (Stübi et al., 2008), and at Uccle (De Backer et al., 1998a). We also add a discussion on the comparison of the Uccle-De Bilt average profile differences between both periods.

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.
In the figures presenting the vertical ozone trends for different periods (Uccle BM 1969-1996 suggested by the other reviewer, Uccle 1969-2014, Uccle/De Bilt 1997-2014), we decided to included the "Uc standard pump", based on several reasons. First, for the BM period 1969-1996, the trend differences between this correction and the simple total ozone normalisation are really small, which points out that these corrections were consistently applied throughout the dataset. However, when mixing up the two different types of ozonesondes used in Uccle in the 1969-2014 time period, the trend differences between the standard pump correction and the simple total ozone normalisation are very significant. This can of course be explained by the fact this total ozone nor-

malisation corrects for the lacking total ozone response equivalent by the BM sondes at the begin of the period, while the ECC ozonesondes have a nearly full total ozone response equivalent, so that the ozone concentration trends will be smaller after this correction. A last reason why we want to include the "Uc standard pump" vertical trend profile is that, for the 1997-2014 period, the trend differences between the O3S-DQA and PRESTO correction can then be easier explained, as the O3S-DQA correction uses the same standard pump efficiency correction factors.

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.
This figure has been omitted and replaced by another one: the vertical ozone trends calculated for the Uccle BM 1969-1996 period, as suggested by the other reviewer.

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.
We changed this into "Consequently, every ozonesonde needs to be prepared and checked thoroughly prior to launch."

pg. 3, line 1: delete "being"
Done

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.
Done, also in the text, we emphasize at several times that some early corrections are based on extrapolations and therefore add uncertainties to the corrected profiles and trends.

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.
We rephrased the sentence as "Moreover, this study is an update of De Backer et al., 1996, who already reported on differences between profiles obtained at Uccle and De Bilt with different types of ozonesondes in the early years of the De Bilt time series." At this point, we just want to point the reader to previous comparisons between the Uccle and De Bilt ozonesonde data.

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.
Done.

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.
Done: "However, for trends, this effect is of minor importance, since these time lags do not change much 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.
We copied this sentence literally from Stübi et al., 2008: "the accuracy limitations appear to come from manufacturing aspects (material used, specifications, provider, etc.) as well as from details of the preparation procedures (Stübi et al., 2008)". We want to point here that, also at Uccle, quality changes between different BM pumps and Styrofoam boxes, have occured, coinciding with the use of another subcontracter for the production of the BM ozonesondes.

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.
As already mentioned above, we compared the Uccle BM- De Bilt ECC tropospheric ozone differences in the average profiles with earlier studies describing BM-ECC comparisons at one site, and with the ozonesonde-MOZAIC comparison performed by Staufer et al., 2014. This discussion is in Section 3.3.1.

pg. 7, line 13: please also state how much 0.1 μA 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.
We included a statement about the contribution of the measured background currents (in μA) to the ozone amounts: "The BGC after ozone exposure is higher than the one measured before ozone exposure, but never exceeds 0.1 μA (around 0.37 mPa or 3.75 to 18.5 ppbv or 7 to 17% of tropospheric ozone) at Uccle, because this is the estab-

lished upper limit for accepting the ozonesonde for launch". Indeed, the higher (upper) tropospheric ozone concentrations observed in Uccle than in De Bilt might be explained by the fact that higher background currents (because measured after exposure to O_3) are subtracted at De Bilt than in Uccle. This finding is mentioned in the discussion of Fig. 9 (comparison of average ozone profiles of Uccle and De Bilt for the period 1997-2014).

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/p0" 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. Also: How high are the backgrounds in Uccle, in μA, nbar, ppbv. Please provide some numbers.
Both the operational and O3S-DQA corrections for the De Bilt dataset use the measured background current values shown in Fig. 3. The only difference is the background current subtraction method: constant for the O3S-DQA and the operational correction after Nov 1998, but pressure-dependent for the operational correction before Nov 1998. However, because the measured background current values are so high, the O3S-DQA guidelines recommend to use a climatological value. On this point, the O3S-DQA correction at Uccle is not fully consistent with the O3S-DQA guidelines. The text in the manuscript is changed into "Although the constant BGC subtraction with the measured values shown in Fig. 3 has been applied for the O3S-DQA correction in this paper, this remains questionable for the De Bilt record, as the measured BGCs are too high in the early years. As a matter of fact, the O3S-DQA guidelines would recommend to use a climatological value of 0.045 μA $\pm$0.03μA for the BGC after exposure of ozone instead. So, on this point, these O3S-DQA guidelines were not followed for the De Bilt O3S-DQA correction applied here." We also add a line about the background current value used in Table 2. The fact that the De Bilt ozone concentrations at

the ground are differing by 2% between the O3S-DQA and the operational correction should indeed not be ascribed to the background current subtraction, but are due to the O3S-DQA corrections of the pump flow rate at the ground: the correction for the humidification effect and the piston temperature. The mean background current value measured at Uccle (before exposure to ozone) is 0.018 μA (around 0.075 mPa or 1.5 ppbv or 2.5% of tropospheric ozone).

pg. 11, line 25: Why the year 2000? Fig. 3 suggests that background values only dropped after 2002. Please explain.
This is a combination of the drop in background values in both the years 1998 and 2002, but also interferes with the change of operating procedures and corrections in the operational method. But, it should also be noted that the precise year where the total ozone underestimation with respect to the Brewer is hard to identify by eye, so we removed the part of the sentence referring to the year 2000.

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

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.
Done.

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.
Done.

Overall remarks

Throughout the manuscript please streamline and shorten/eliminate the re-

peated discussion of background measurements and background subtraction.
We tried to do this at several places in the manuscript.

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.
We add a whole paragraph about the differences in ascent rates and possible geophysical differences between both sites. From the instrumental point of view, we could not identify other possible causes (e.g. pressure offsets between the radiosonde measurements at both sites).

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.
Done, we add the sentence "Despite their large impact on the average ozone profiles, the different (sensible) correction strategies do not change the ozone trends significantly, usually only within their statistical uncertainty due to atmospheric noise."

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