

Atmos. Chem. Phys. Discuss., referee comment RC1
<https://doi.org/10.5194/acp-2021-54-RC1>, 2021
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Comment on acp-2021-54

Anonymous Referee #1

Referee comment on "GUV long-term measurements of total ozone column and effective cloud transmittance at three Norwegian sites" by Tove M. Svendby et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-54-RC1>, 2021

General comments:

The manuscript describes both the quality assurance of total ozone column (TOC) and effective cloud transmittance (eCLT) measurements at three Norwegian sites and presents the analysis of the time series. The manuscript is well written and easy to follow, but needs some rearrangement of the text as suggested in the Specific comments. As stated in the Abstract, this work is a good demonstration of how GUV TOC measurements can be used to complete state-of-the-art instrumentation. However it is not clear if the methodology used to correct for the seasonal drift is location dependent or not. The approach of using the effective cloud transmittance to assess changes in albedo is interesting, but would benefit from comparison with albedo/snow cover data if such data is available. The data analysis of both TOC and eCLT is more an explanation of observed changes than trend analysis, so I suggest to change the wording from trend analysis to observed changes. I also miss uncertainty estimates of the measurements.

I think, that after revision, this work is very valuable for the scientific community by describing and analyzing ozone measurements in the Arctic and by giving information of albedo changes at areas of special interest from the point of view of climate change.

Specific comments:

Line 49: Generally, a decrease in total ozone column leads to an increase in UVB radiation
 That's true assuming no changes in cloudiness (or other UV-affecting parameters).
Please update the sentence.

I suggest to change the title of Chapter 3 to Material and Methods, as you describe also

the instrumentation.

Please add a brief description of the Brewers and SAOZ in Chapter 3.

Section 3.2., line 135. Please give a brief description of the methods, including needed equations, for "annual assessments of drift and determination of correction factors".

Line 149 and eq. (2) : Please explain how the possible drift in V_i or V_j is taken into account, when calculating the total ozone time series. How does the calibration or comparison with the traveling reference is taken into account?

Line 168: Could you please justify the choice of 340 nm instead of 380 nm?

Lines 177-187: I suggest to move this paragraph to Section 3.3. in which you describe the TOC retrieval theory.

Lines 195-212: Please move these informations to a separate Section in Material and Methods. Please add information of the uncertainty/error estimates of Brewer and SAOZ TOC.

Line 217: For the Brewer DS measurements: Which measurements did you use, the daily mean? Or DS around local noon?

Figure 3.: This Figure 3 doesn't convince me. The problem is that in June, the data points stop at SZAs when in April the "spread" or "SZA dependence" only starts. What would this figure look like if you would do it for e.g. Oslo, where also high SZA:s are reached during the summer?

Section 4.1.: I would like to see the effect of the seasonal correction. Please add a Figure similar to Figure 2, but plotted using the applied correction. Consider showing it with data having all corrections applied.

Section 4.1. Please add statistics of the GUV vs Brewer/SAOZ comparison at all three sites, for non-corrected and for corrected data.

Figure 5: For consistency in Fig. 2 and Fig 5., choose weather you prefer to show the results as percentage or as ratio. What is the red continuous line?

Line 273: Was the GUV data already corrected for seasonality as shown in Section 4.1.?

Line 302: Please quantify the mean bias for all stations.

Lines 295-302: I suggest to move this info in the Section Data analysis, and show a Figure (see my comment above) including the comparison GUV_all_correction_applied/Brewer.

Line 305: GOME-2 data: Please add a brief description in Material and Methods. Is the time series homogeneous?

Line 314: How would the albedo affect GOME-2 data? Could Andoya's albedo conditions also affect GUV TOC measurements? I think the GOME-2 TM3DAM data includes some error estimates, have you looked at them?

Table 4: Please include also the mean bias as percentage in addition to DU.

Line 331: "...can be moved to a new location..." Are the corrections / harmonization independent of location? What is the effect of possible differences in the ozone profiles between locations?

Line 338: Please add a brief description of differences between Brewer DS, ZS and GI measurements in Materials and Methods.

Line 371: Dynamical transport in the stratosphere?

Line 416: Please describe OMI TOC in Material and Methods.

Figure 8: Now that you have included OMI TOC in this plot: why not performing the comparison between GUV and OMI TOC in Section 5.1.? Also when looking at the plot, the question rises, why GOME-2 and OMI differs from each other (and GOME-2 from GUV) after mid-April ?

Section 5.2. in general: I am wondering if the word "trend" is the best one to use. For me, the results show more "observed changes", as no trend analysis including the "explanatory terms" mentioned in lines 372-374 is done. Please consider changing the wording.

Lines 442-446. Please check if Oslo was inside the polar vortex on that day. If not, I don't think the explanation is valid, as it's only the start of the chemical ozone loss period. Oslo's latitude is quite low, and the station is not obviously inside the vortex.

From: https://ozonewatch.gsfc.nasa.gov/ozone_maps/movies/OZONE_D2019-11-01%25P1D_G%5e360X240.IOMPS_PNPP_V21_MMERRA2_LNH.mp4

it can be seen that the ozone loss associated to the polar vortex forms later in the season 2019/2020. Could the December 4 case in Oslo be as a result from ozone poor air transport from the mid-latitudes or related to changes in the height of the tropopause / dynamics?

Line 475: Please move the description of cloud data to Materials and Methods.

Line 488-489: It's not clear how you calculated the correlation coefficient. And what was the purpose of the correlation calculation? As eCLT includes the effect of aerosol, albedo and cloudiness, please explain the point of calculating the correlation with cloudiness observations. How about doing it only for seasons with no snow? Please remind the reader of the criteria for the classification "overcast" for GUV data. Also it's not clear in Table 6 that the correlation coefficient is for overcast days.

Figure 16: Do you have an explanation for the "jump" or "high" value in 2009. Do you have snow cover data/ ice data which could explain it? Overall, do you have any albedo data / snow cover information which could correlate with this data?

Data availability: Please add cloudiness data from NCCS.

Technical corrections:

Line 60: Brewers spectrophotometers Brewer spectrophotometers

Table 1: For Ny-Ålesund, you have included the serial number. In case you wish to keep it, please add it also to the other stations. If not, then delete it also from Ny-Ålesund.

Section 3.3. line 140. Title: Please use the whole words instead of eCLT.