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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Re-evaluation of the 1950–1962 total ozone record from Longyearbyen, Svalbard" *by* C. Vogler et al.

C. Vogler et al.

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Ad Referee #1

General remarks:

TOMS Version 8 was used. See comment by G.Hansen.

It is true that the approach assumes that the underlying dynamics has not changed. In fact, there was a clear change in dynamics between the 1950s and the 1990s and therefore the 1990s were excluded from the validation presented in Figure 8 (this is stated in the manuscript). Between the 1950s and the 1980s the change in dynamics seems small on average, even though it may be larger for specific calendar months. Concerning the use of 100 hPa temperature as a reference series, as is suggested in the WMO manual, one has to consider the greenhouse cooling in addition to the

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changing dynamics and ozone depletion (see reply to referee 2). Because of the long gap between the historical period and the reference period (which necessarily must be TOMS) the relation between Longyearbyen total ozone and 100 hPa temperature might have changed to an even larger extent than the total ozone ratio between Longyearbyen and Tromsø.

Error bars can only be calculated for the monthly mean values, but not for monthly mean relations (see reply to referee #2).

Specific comments:

Section 2, *Para. 1: The wording has been modified.

*Para. 2: There was no major volcanic eruption in the study period (the eruption of Mount Agung was in 1963/4). We have added a corresponding note in the text.

*Para. 3: The expression “artificial light sources” has been replaced.

Section 3, *Para. 4: The paper was confusing with respect to the expression “airmass”. This is now corrected. Throughout the paper we clearly distinguish between airmass (m) and ozone slant path (mu). It was not clearly stated in the original manuscript that mu was calculated according to Komhyr’s algorithm (Komhyr, 1980), with an assumed ozone layer height of 18.2 km. The airmass (m) was derived from (Young, 1994).

*Para. 5: We used the Bass-Paur absorption coefficients. In the revised paper, we have changed the corresponding section and are more specific. Of course, today’s standard absorption coefficients have been used. An extract from the revised paper: “The ozone absorption coefficients used in 1950 differ significantly from the ones used today. For this reason one has to use today’s standard coefficients (defined by WMO), see Komhyr (1993). For a compilation of the used ozone absorption coefficients and Rayleigh scattering values see Table 1. For the derivation of ozone values the following equation was used: (single pair equation) As mentioned in chapter 2 aerosol scattering was neglected.”

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*Last Para: For the data set based on DS mode we only used measurements from the single wavelength pair C. An explanation for this choice is given in chapter 4.

Section 4: We have changed the title to ĎComparison with Tromsø total ozone in 1950-1962 and in TOMS data“

*Para. 4: After investigating this issue we found that the effect of the actual station pressure (assuming that daily pressure variations are within +-2%) is causing an error between 0.5 - 1% in total ozone values for all possible values of mu and total ozone. As this error is smaller than the instrument accuracy of 1% it can be neglected. Subsequent changes in the paper have been made.

*Para. 5: There were investigations into the double vs. single pair ozone. But it gave a very unclear picture. The first problem is the availability of the AD and CD measurements (see Table 1, now Table 2). The ratio between C_DS and AD_DS (CD_DS) seem to be different for 1957-1962 than for 1951. Since there are only a few double pair measurements in 1951 and there is a long gap thereafter, it is not possible to draw any conclusions regarding a possible shift in the data. Furthermore the investigation in chapter 4 which resulted in Fig. 2 (now Figure 3) showed that there is a problem with the double pair measurements. As we do not have any information on calibration status of the instrument we can't say where the problem is coming from. For this reason we do not think that the comparison of single and double pair measurements results in reliable information on calibration or aerosol error.

References:

Basher, R.E., Review of the Dobson Spectrophotometer and Its Accuracy, WMO Global Ozone Research and Monitoring Project, Report No. 13, December 1982.

Komhyr, W. D., Mateer, C. L. and Hudson, R. D., Operations handbook - Ozone observations with a Dobson Spectrophotometer, WMO Global Ozone Research and Monitoring Project, Report No.6, 1980.

Walshaw, C:D:, (Ed) 1975. Papers of Professor G.M.B. Dobson FRS. Publs.Inst.Geophys.Pol.Acad.Sci. 89, 61-115.

Young A. T.: Air mass and refraction. Applied Optics, Vol.33, No.6, 1994. Ad Referee #2

General remarks: - In the WOUDC there are data from Longyearbyen (STN044) from 1957 - 1966 and 1984 - 1993 and from Ny Ålesund (STN089) from 1966 - 1968 and 1995 - 1997. See also comment by G.Hansen. Inconsistencies in the paper have been corrected.

- Our manuscript might have been confusing with respect to the measurement locations. This has been improved in the entire document. The discussed and re-evaluated total ozone record is from Longyearbyen (as was indicated in the title), which is the administrational centre of Spitsbergen, the largest Island of the Svalbard Archipelago. Obviously the instrument was moved to Ny Ålesund in 1966 (see comment by G. Hansen). Ny Ålesund is a research station, 114 km northwest of Longyearbyen. For details of the geography of Svalbard see http://geography.about.com/library/cia/blcsvalbard.htm

- TOMS Version 8 was used in this investigation. See comment by G.Hansen.

- A reference to Basher (1982) is now included and the section on the measurement principles is more detailed and includes the most important equation (see comment to referee #1) as well as a table with the wavelengths and corresponding absorption and scattering coefficients. In Komhyr's Dobson Manual (WMO report No.6) there is no information on the observing range of DS_C (single pair) measurements. On the other hand we were referring to an investigation of G.M.B. Dobson (described in Walshaw (1975), now included in section 3) which showed that it is possible to extend the usable range of the C (single pair) observations from an ozone slant path of 4 up to 6 by using the focused image mode. Despite this, our actual investigation (initiated by the referee's comment) showed that only C_DS measurements with a mu$<$4.5 are reliable.

This finding is shown in the new Figure 2. Therefore, in the revised paper the maximum mu value was set to 4.5 for DS measurements. (See new text at the end of section 3). For the ZB measurements we did not follow the recommendations from Komhyr (1980). We included measurements up to a SZA of 90° (as described in section 5). We now have added some error estimates for the ZB measurements at high mu values. Also in this case the new Figure 2 indicates that the included data are reliable. For the ZC measurements we have chosen a maximum mu value of 4.5 (SZA=78°), as it is restricted by the calibration of the ZB values. Again Figure 2 shows that this range is good.

Special comments:

- Title: The existing title is already the best description of the location according to the official use of language in Norway. With the expression "Longyearbyen, Svalbard" we clearly say that the main objective of this paper is the re-evaluation of the Longyearbyen data record.

- Abstract/Introduction: In the revised manuscript we mention the instrument relocations to Ny Ålesund in the Introduction.

- Introduction, page 3:

1. We have added one sentence stating that the effect of dynamical processes on Arctic ozone and their relation to large-scale climate variability are still not completely understood

2. See general remarks.

3. Changed according to remark.

- Chapter 2, page 4:

1. We have changed the text.

2. We have changed the text according to the suggestion.

3. C’ is added to the listing of wavelengths pairs.

4. It is well-known that measurements on double wavelength pairs are more reliable. For the reasons given in section 4, we use measurements on a single (C) wavelength pair as standard. However, as suggested by the reviewer we discard the data for very low sun elevation and restrict the data to the mu range < 4.5 (see above).

5. Changes have been made in the paper in the description of the definition and maintenance of R-N-tables.

Chapter 3, page 5: The suggestion made by the reviewer has been adapted. As now mentioned in the text we have information on mercury lamp tests, standard lamp tests and wedge calibration tests for 1 July, 1958, 1 November, 1958, and 20 June 1959. In this period the instrument appears to have been stable. However, the information from this short period does not allow drawing conclusions for the whole 13-year period of the re-analysis, but this is an indication that tests have been made regularly. It is indeed a pity that we do not have more information about any SL-tests and other calibration information. Several attempts have been made to find such information, but .

- Chapter 3, page 6:

1. We found a change in the R-N-conversion which indicates a possible wedge calibration. Having no additional information, we simply apply the new R-N-conversion.

2. The manuscript was confusing with respect to the use of the expression “airmass”. This is now corrected by clearly addressing the airmass as m and the ozone slant path as mu. It was not clearly stated in the original manuscript that mu was calculated according to Komhyr’s algorithm (Komhyr, 1980), with an assumed ozone layer height of 18.2 km. The airmass (m) was derived from Young (1994).

- Chapter 3, page 7: As suggested by the reviewer we discard the data for very low sun and restrict the data to the mu range below 4.5 (see above). Rectifications about the requirements for DS observations have been made.

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- Chapter 4, pages 7 and 8:

1. Title has been changed. We do not compare 1950 to 1962 data with TOMS. We just determine a relation between Tromsø and Longyearbyen and compare this relation for 1950-1962 and 1979-2001 (TOMS).

2. TOMS Version 8 was used (see comment by G.Hansen). It is true that the used approach is not without problems. A cautionary comment has been added to the paper. Nevertheless we consider it to be the most appropriate solution.

3. As suggested by the reviewer we discard the data for very low sun and restrict the data to the mu range below 4.5 (see above).

- Chapter 5, pages 9 and 10:

1. As in section 3 and 4 described we chose the DS measurements from the C wavelength pair as a reference. mu values were accepted up to 4.5.

2. We have added more information on the expected errors for different modes of operation and mu range mainly in the form of additional text. The uncertainty of the ZB measurements for different mu ranges is shown in Figure 4 (now Figure 5), which shows that the uncertainty is always smaller than +-10% (see additional new details in section 5). For the ZC data, the uncertainty is shown in Figure 5 (now Figure 6, lower panel). See also new text describing this figure at the end of section 6. In addition, we have added a new Figure (now Figure 2) that shows results of a standard test of the data with respect to the mu range. We also have added an error range to Figure 7 (now Figure 8). In most other cases, the calculation of error bars does not make sense as the error is composed of many different components (e.g., in the plots of the Longyearbyen vs. Tromsø ratios, the total error includes the error in the Longyearbyen data, the error in the Tromsø data, and the standard error of the monthly means derived from incomplete data).

Chapter 8, (pages 12 and 13): We agree that the conclusion was rather optimistic.

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A more cautionary note was added in the revised manuscript. As now mentioned in chapter 8, we conclude from Fig. 7 (now Fig. 8) that there is no break in the data in 1956/57, after we applied a new R-N-conversion from 1956/57 on and we used two different polynomials for the ZB data. We also add an error range to Fig. 7 (now Fig. 8) which shows that the trend (which visually seems quite clear) is still very small compared to the confidence intervals.

Chapter 9, (page 14):

1. TOMS Version 8 was used. See comment by G.Hansen.

2. Yes, they are continued until today. However, since there is no national or international funding for the measurements, they are performed and analyzed rather irregularly.

3. We agree with the author's opinion that this is very optimistic, and we are aware of the limited applicability of such a series; it should not be used for the determination of (small) trends. However, it might be useful to some degree to analyse the interannual variability due to dynamical influences in months with large inherent variability.

4. Figure 8 shows July. It's corrected in the text.

Conclusion/Recommendation:

The referee suggests using 100 hPa temperature data from a nearby radiosonde station as a reference. While this would in principle be a good way of testing the series, the problem remains that we still need an ozone reference series, which must necessarily be based on TOMS data. Hence, there is a long gap between the historical period and the reference period. Because lower stratospheric temperature is affected not only by ozone depletion, but also by greenhouse cooling (and related dynamical effects), the relation between Longyearbyen total ozone and 100 hPa temperature might have changed to an even larger extent than the total ozone ratio between Longyearbyen and Tromsø during this long gap.

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