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

Anonymous Referee #2

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General remarks valid for the entire paper:

- The given dates of the various data sets are not consistent with those, which can be found in the WOUDC data base, e.g Longyearbyen 1957 - 1968, but in the Toronto records data exist only until 1996.

- Often only Svalbard is mentioned as location, especially in the title. Thus it is not clear whether the station is at Longyearbyen or Ny Ålesund.

- The use of TOMS Vs. 7 for the re-evaluation is no longer justified, as the since more than one year available Vs. 8 is assumed to be more accurate especially in the higher latitude. Thus some conclusions, e.g. that Dobson is higher in 1980s might be not correct.

- In some sections (e.g. see remarks for sections 2 and 3) the description of the measurement pri- ciples, calibration procedures and corresponding quality control methods and the explanation of data processing confirms the guess, that the authors are not so familiar with the Dobson, its mea- surement principle and potential error sources. The study of Basher's WMO Global Ozone Monito- ring Project Report No. 13 and its inclusion in the references would be of great help. Although Komhyr's Dobson Manual WMO Report No. 6 is mentioned in the references, its table of the limi- tation of the observing range (page 28) for the different observation types is not taken into consideration.

Special comments:

- Title: Please improve the title, because the general mentioning of Svalbard is somewhat mislea- ding or even confusing. Besides Longyearbyen also Ny Ålesund is located on Spitzbergen, which is one of the major islands of the Svalbard archipelago. The main objective of this paper is the re- evaluation of the Longyearbyen data record.

- Abstract/Introduction: Although Ny Ålesund is mentioned later in chapter 9, it should already be listed already in the abstract and introduction as intermittent location for the Dobson No. 008 and possible data source for further investigations.

- Introduction, page 3:

1. Only chemical processes are understood to a large degree, the effect of dynamical processes on the ozone depletion still needs more and intense investigations.

2. The given data set periods 1957 - 1968 (1966?) 1984 - 1997 (1993?) are not consistent with the Toronto archive, 1995 to 1997 belongs to Ny Ålesund (see also general remarks).

3. The amendment "in the 1950s and 1960s" at the end of section 2 defines the mentioned two decades more clearly.

- Chapter 2, page 4:

1. Manufacturing started in the late 1920s.

2. under cloudy conditions "using the so-called Zenith Cloud ZC method".

3. C' is missing in the listing of wavelength pairs.

4. CD is also used as double wavelength pair in higher latitudes instead of AD, when sun is low. Double wavelengths pairs either AD or CD at low sun are in any case better and more trustworthy than the single wavelength pair C. The maximum ozone slant path (mu-range) is about 4 to 4.5 depending on turbidity, ozone value and instruments sensitivity.

5. The explanation of measurement and data processing is somewhat confusing (see also gene- ral remarks): Firstly the R-N conversion tables for each wavelength pair are defined by a ca- libration of the transmission gradient of the optical wedge (so-called wedge- or two-lamp ca- libration) and secondly by a side-by-side calibration (more details s. Komhyr, 1980) with a standard Dobson.

- Chapter 3, page 5: After the first clause a more detailed description of the necessary information for an accurate re-analysis would be: "Necessary information are the raw data (R-Values) and ca- libration information (at least R-N-conversion tables, standard lamp (SL) reference values incl. the history of the SL-tests).

The mentioned mercury lamp test is basically no information about calibration (statement in last section), as it indicates only the correct wavelength setting. It would be really a pity, if there were no information about possibly done SL-tests.

- Chapter 3, page 6:

1. The described method how to derive the R-N-conversion out of the found R- and N-values is fine and acceptable, but it should be mentioned that various R-N-conversion tables have to be determined for each wavelength pair. A change of the instrument's

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calibration level is mostly accompanied by a constant shift (additive value) of the R-N-tables. A fundamental change of the R-N-conversion is only done after a wedge calibration.

2. What about calculation of the so-called mu-value (ozone slant path)? Its value differs from the airmass m significantly at low sun and dependents also on the assumption of the height of the ozone layer. Why isn't Komhyr's algorithm used for this purposes, it would be nice to know the differences in the m and mu calculation between both methods. There can be remarkable ef- fects on the ozone values at low sun, which is often the case in higher latitudes.

- Chapter 3, page 7: DS mode observations at a mu-value (not airmass!) up to 6 cannot be done, neither by AD, CD nor only C. The aerosol effect at low sun, even at high latitudes with clear air, cannot be neglected, especially at low altitude stations, thus C is surely not more trustworthy than at least the CD. DS does not need clear sky, the only condition is that the sun is not obstructed by clouds for several minutes.

- Chapter 4, pages 7 and 8:

1. Title of this chapter is again confusing: first it is Longyearbyen and not Svalbard and second its 1950 to 1962 - data cannot be compared with TOMS starting not before 1978.

2. In principle the determination of the relation between Tromsø and Svalbard by means of TOMS data is an interesting approach. There are, however, two issues, which could be limita- tion factors of the applicability of this method: first the used TOMS Version 7 is no longer the best available data set especially for higher latitudes. The use of Version 8 could possibly lead to different results, especially as the latitudinal distance between Svalbard and Tromsø is a- bout 800 km. Second it is not taken into account, that possibly the Xi-relation and its annual course might not have been constant since 1950 (pre-CFC-period), due to the CFC induced modifications of the ozone layer.

3. The only acceptable reason for the use of C-observations is the large number of data points. As already mentioned before double wavelength pair measurements AD and in peculiar at low sun CD are more trustworthy. In any case ozone data derived from measurements at mu- values higher than 4 to 4.5 (SZA $>$ 76 - 77o) are not appropriate for any use (e.g. determinati- on of the coefficients for the Zenith algorithms).

- Chapter 5, pages 9 and 10:

1. Here it is not clear which type of DS-measurements was used and in which mu-range (see comment just before), otherwise the method itself is O.K..

2. The lack of detailed error assessments and its presentation also in the graphs (error bars) is a major shortcoming of the paper especially in this section. It would be of great interest how reli- able ZB measurements are in the different mu-ranges below and above 80o SZA. It is suppo- sed that the uncertainty of ZB-data at very low sun reaches or even exceeds ś10% (see Kom- hyr's and Basher's publication); the same is valid also for ZC (chapter 6, pages 10-12) espe- cially under the condition that information about sky/cloud condition is not sufficient.

- Chapter 8, pages 12 and 13: The conclusion drawn from graph 7, that there is no trend or drift in the data set itself is rather risky and optimistic. Even the mentioned 2-3 % per 12 year are signifi- cant compared with the observed trend in the ozone layer. In addition it is not described, how the break in 1956/57 was treated (see Chapter 3, page 6) to get rid of it.

- Chapter 9, page 14:

1. The observed differences to TOMS Vs. 7 is probably overstated as already mentioned. The latest Vs. 8 could possibly bring different results.

2. Are the Dobson measurements at Ny Ålesund really continued until today?

3. The hope to combine the various observations from Dobson, Brewer and filter (M83, 124 and DOAS) instruments from different stations to create "one" Arctic ozone series

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seems to be ve- ry optimistic. It is even in the midlatitudes difficult to combine two data sets from e.g. Dobson and Brewer at one station to create a single homogeneous data record.

4. Does figure 8 show July or June.

Conclusion/Recommendation:

First the content of the paper deals with a topic which is surely within the scope of ACP. The paper beyond doubt addresses relevant scientific questions. There is really a need of reliable long term re- cords of total ozone in the Arctic region, which starts in the pre-CFC period, and the presented appro- ach how to come to such a valuable data set is interesting and on the first look promising. Neverthe- less this paper needs major revisions before publication because of the following reasons:

- Use of TOMS Vs. 7 instead of Vs. 8, being improved in particular in higher latitudes.

- Estimation of uncertainties, errors and presentation e.g. of error bars in the graphs would improve the reliability of the "new" data set with respect to possible trend analy- ses.

- The use of a possibly existing SL-test history would be extremely valuable and helpful. Is there really no chance to dig out this very important data?

- The re-evaluation method is based on the assumption that there was no significant change in the geographical distribution all-over the year of the ozone layer between Longyearbyen and Tromsø. As this assumption is doubtful due to the observed ozone depletion since the past three decades, the use of another independent method to check and possibly correct uncertainties in the data set would be helpful. If there were the 100 hPa-temperature from a near-by radiosonde station avai- lable in the corresponding period, "Bojkov's" correlation method, described in.Bojkov 1964, NOAA 1993 and WMO 1993, would be appropriate to check and hopefully confirm or at least improve the findings of the Longyearbyen - Tromsø correlation method.

- The use of DS-data at very low sun (SZA $>$ 75o) in order to increase the content of the data base probably introduces large errors into the correlation method. This should be checked.

Additionally mentioned references:

Basher, R., Review of the Dobson Spectrophotometer and its accuracy, WMO Global Ozone Re- search and Monitoring Project Report No. 13, 1982.

Bojkov, R., Variations in the total ozone content and their relation to temperature variations in the stratosphere, Geomag. Aeron., No. 4, pp. 137-140, 1964.

NOAA, Dobson Re-Evaluation Handbook, Technical Report NESDIS 74, 1993.

WMO, Handbook for Dobson Ozone Data Re-Evaluation, GAW, WMO Global Ozone Research and Monitoring Project Report No. 29, 1993.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 3913, 2006.