Atmos. Chem. Phys. Discuss., 11, C10940–C10951, 2011
www.atmos-chem-phys-discuss.net/11/C10940/2011/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Trends of solar ultraviolet irradiance at Barrow, Alaska, and the effect of measurement uncertainties on trend detection" *by* G. Bernhard

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Received and published: 27 October 2011

We thank the reviewer for his or her very thorough assessment. Reviewer comments are repeated below, followed by my response. The numbering of tables and figures refers to that of the ACPD paper. The numbering in the ACP paper will be different because one table will be added and one figure removed.

Comment 1: However, some parts, e.g. the explanation of the four correction methods (which did not give any significant difference compared with CIs), periods that have been omitted due to anomalous data, presentation of results from figures and tables

etc., appear to me too detailed and elaborate in the descriptions. I would recommend tightening up the text.

Response: The discussion of the influence from instrumental problems will be moved to an appendix (see also Comment 8). The discussion of the different results obtained with Methods 2-4 will be shortened (Comment 10). Figure 5 will be removed (Comment 14).

Comment 2: Page 6, line 19-22: The method how UVI is calculated may be replaced by a reference to WHO/Intersun http://www.unep.org/pdf/Solar_Index_Guide.pdf

Response: This reference has already been cited in line 19 (WHO, 2002). The citation will be repeated in line 22, and the URL will be added to the list of references.

Comment 3: Page 8, equation 3: How is (db/dE(yi)) defined? The term is common in equations for u(b), uu(b) and ug(b).

Response: b is the regression slope, which is calculated via an analytical function from the measured data (years yi and associated irradiance E(yi)). yi and E(yi) are therefore independent variables. db/dE(yi), the partial derivative of b with respect to E(yi), expresses the sensitivity of b with respect to a change in the measured value E(yi). To make this clear, the following will be added to the text: "The partial derivative db/dE(yi) expresses the sensitivity of the regression slope b to a change in the measured value E(yi)." Note that more details on the calculation of db/dE(yi) is provided in the reference (Press et al., 1986), which is provided immediately above the first use of this term.

Comment 4: Page 8, lines 10-14: The trend analysis is based on the assumption that the data set is not autocorrelated, referencing to the Durbin-Watson Test. However, looking at Figure 2 and Figure 4, the residuals appear to have cyclical components, most readily seen for the E345 June monthly means (two cycles, each 6 years period). With reference to the work of Weatherhead et al. 1998, autocorrelation would expand the uncertainty and reduce the significance of trend estimates. This is missing in the discussion.

Response: I reviewed my calculation of the Durbin-Watson statistics and didn't find a problem. Note that this test only examined whether data are autoregressive of the order of 1 [AR(1)], that is, whether monthly averages for the same month of two consecutive years are correlated. This is the most commonly used approach when autocorrelation is used in regression analysis and this method is also used by Weatherhead et al. (1998). The apparent periodicity in the E345 June monthly means is therefore not detected by the test.

The paper will be modified as follows: - In Section 3, where autocorrelation is discussed, the sentence "No autocorrelation was indicated." will be changed to "No autocorrelation was found between measurements of consecutive years." - The following will be added to Section 5 (Discussion): "Autocorrelation was not taken into account when calculating uncertainties of trends because the Durbin-Watson test did not indicate that measurements of consecutive years are autocorrelated. The effect of cycles with periods of longer than one year was not considered. For example, the monthly means of E345 for June (Fig. 2) appear to be cyclical with a periodicity of 6 years. If such periodicity had been considered (for example by adding a harmonic function to the linear regression model of Eq. (1)), trend uncertainties would be smaller."

Comment 5: Page 11, line 11-12. The q-ratios for correcting systematic errors in the calibration scales are based on selecting clear sky measurements, with a reference to Bernhard et al. 2008. Looking into this reference, page 4802, the requirement for flagging clear sky cases is that the temporal variability between 3 neighboring ratios

E600/F600 are differing less than 1%. However, it seems this would also apply for stable overcast conditions. So how was the selection in order to avoid mixing overcast conditions with clear sky conditions made, which otherwise would have biased the q-ratios?

Response: It is correct that stable overcast conditions also fulfill the clear-sky criterion used by Bernhard et al. 2008. At Barrow, typically 70 spectra per year that are measured under cloudy skies fulfill this criterion compared to about 1100 clear sky spectra. To exclude incorrectly labeled spectra, measurements at 600 nm are first extracted from the filtered dataset. These data are then plotted versus solar zenith angle. Clear sky measurements fall into a narrow band and overcast measurements that incorrectly passed the clear-sky filter are distinctively lower. The algorithm identifies the lower envelope of the clear-sky measurements and all data points below this thresholds are excluded. To minimize the risk that a few (<5) overcast spectra that might have slipped through this algorithm bias the correction, q-ratios are based on the median of the ratio spectra rather than the average. The following will be added to the text: "Spectra measured under stable overcast conditions that passed this criterion were identified and removed."

Comment 6: Page 11, line 5 : The data set was based on daily noon spectra. How many days were used, compared with the theoretical number of days for SZA <80 degree? And how many days were excluded due to improper quality and gaps, respectively? And was the excluded days evenly distributed for each year? The information could be given in a figure, or Table 1 expanded.

Response: Page 11, line 5 (page 26629, line 4 of ACPD paper) refers to a discussion of spectra used for the comparison of measurement and model in order to calculate correction factors (q-factors). For this task, all clear sky spectra measured at solar zenith angles smaller than 80° were taken into consideration, not just daily noon spectra, as indicated in the comment. If only daily noon spectra had been used, the data basis would have been far too small for deriving meaningful statistics because periods with clear sky are the exception at Barrow. Only the trend analysis was based on noon-time spectra, as explained on page 26621, line 23-29 of the ACPD paper.

While the reviewer's intension is not entirely clear to me, I feel that the he or she is more interested in the number of days per year and month used to calculate the monthly averages for the trend analysis than the number of spectra used to establish the q-factors. I may point out that some statistics on the number of spectra used for monthly q-ratios (q_monthly(y,m)) have already been provided in Figure 1. In that figure, open blue symbols indicate all monthly q-ratios while solid blue symbols present the subset of these q-ratios where the median was calculated from at least 10 spectra. The latter are dominating and provide a robust estimate of systematic measurement errors, see also page 26630, lines 10-14, of the ACPD paper.

A new table will be added to the paper (see below), having the caption: "Number of days used for the calculation of the monthly average. Months with more than 10 missing days were not used in the trend analysis and are shown in parentheses."

Comment 7: Page 11, text referencing to the q-ratios in Figure 1: The differences between the q-ratios determined by methods 2-4 are small compared with no use of q-ratios (method 1), at least relative to the large CIs. To me, the text may be shortened, focusing on the finding that elaborate determinations of q-ratios did not significantly differ from the no-correction case.

Response: I reviewed the text on page 11 describing the correction method and think that shortening the text (e.g., by removing the examples given in parentheses) would make the text considerably more difficult to understand. I therefore decided to keep the level of detail, however, the section discussing outliners (page 26630 line 1 - 26631, line 12) will be moved to an appendix, see also Comment 8.

Comment 8: Page 11-13: The description of q-ratios and influence from instrumental problems etc. appears too detailed. Information may be considered moved to an appendix, or formulated in a more general way.

Response: The section discussing outliners (page 26630 line 1 - 26631, line 12) will be moved to an appendix.

Comment 9: Page 14: The gap correction is based on the shift in monthly mean that would result if a number of days are missing, assuming the missing days follow the seasonal mean. However, the real situation may be differ from the seasonal mean and induce a bias not accounted for. E.g. for the case of early or late days in a month missing, and the weather situation being significantly different from the seasonal daily mean. Instead of assuming a rectangular probability distribution, I would suggest modelling the probability distribution with a bootstrap technique, calculating the distribution of monthly means for 1,2, 3 etc missing days arbitrarily chosen, using a data set with no gaps (which could be a pyranometer or multiband filterradiometer data set). This could be further discussed on page 17, lines 22-25 where the gap corrections apparently are overestimated for February (us(T) from eq. 7 being negative).

Response: The gap correction takes year-to-year variations into account and does not just use a static climatological mean to "fill-in" missing days. I use the following hypothetical example to make this clear: suppose the UV irradiance for the month of April of 2006 is 10% smaller for all days compared to the climatological daily values for April. The gap correction expressed by Eq. (8) is designed such that the calculated monthly average for April 2006 would be low by 10% regardless of what days are missing. Thus, the correction is based on the conservative assumption that the relative bias for missing days with respect to the climatology is the same as the bias calculate for all available days of a month. As the reviewer correctly points out, there is of course the risk that the actual irradiance is lower than the climatology at the beginning of the month and larger at the end, or vice versa. The problem is mitigated by the fact that single missing days are "filled-in" by forming the average of measurements for adjacent days (page 26632, line 12). Considering that the gap-correction is relatively small and does not require ancillary measurements (e.g. pyranometer or multiband filter radiometer data as suggested by the reviewer), I think the application of a more complicated method such as a bootstrap method is not warranted. The gap correction for February was likely not overestimated, as suggested by the reviewer. Instead, the uncertainty of the gap correction was estimated very conservatively (i.e., it is proportional to the correction itself, page 26633, lines 12-17). To make this more clear the sentence "I believe that the uncertainty attributed to data gaps as unrealistically large for February." (page 26636, line 12) will be changed to "The uncertainty attributed to data gaps is likely too large for February because of its conservative estimate with Eq. (9)."

Comment 10: Page 16 lines 14-22: The text may be shortened, see the general comment above.

Response: Text was shortened to: "Trend calculations were repeated using the data sets corrected with Methods 1, 2 and 3, to quantify the sensitivity of the trend estimates on the treatment of systematic errors. Results of the trend estimates T and the associated uncertainty u(T) are shown in Fig. 3. Results of Methods 1, 2, and 4 are generally very similar, but trends determined by Method 3 are 1.7% larger on average. This is caused by the step-change of C_summer(y), with lower values for the years 1992 – 2001 than for the 2002 – 2010 period."

Comment 11: Page 17, line 5-6: 'Trends estimated with Method 2 are 1.7% larger on average'. I can't see this from the figure. To me there seems to be no significant difference within the groups of 4 bars.

Response: "Method 2" should have been "Method 3". The error was corrected. The "green boxes" in Figure 5 symbolizing the trends calculated with Method 3 are clearly systematically higher for all months than the boxes for the other methods. On average, the difference is 1.7%, as stated. Note that Fig. 5 will be removed in the final manuscript in response to a later comment. However, the information contained in this figure remains available in Table 1.

Comment 12: Page 28, Table 1: Why is n for ozone trend estimates different from n for E345 and UVI trend estimates (e.g. May, n is 19 and 17, and October n is 17 and 19)?

Response: As described in the paper, the total ozone dataset was retrieved from measured UV spectra using the method by Bernhard et al. (2003). The method is rather slow and computational demanding because the ozone profile is taken into account for every calculation. It is therefore not practical to calculate ozone for every UV spectrum. Instead, ozone was only calculated for times of satellite overpasses (i.e., TOMS N7, TOMS EP, and OMI) and times when NOAA Dobson measurements are available. These data are available at http://uv.biospherical.com/Version2/data.asp#O3. The number of days with ozone data is therefore smaller than the number of days with UV data. In response, the maximum number of missing days per month allowed for calculating monthly average ozone columns was relaxed from 10 days used for UV trends to 20 days for ozone. The same "gap correction" method as applied to UV data was used. The derived uncertainties in decadal ozone trend caused by gaps were smaller than 0.4% for all months, with the exception of February where it was 1.4%. (Column 7 of Table 1). These "gap" uncertainties are generally smaller than those for the UV datasets. I therefore concluded that the relaxation of the "maximum number of missing days per month" criterion is justified. If the criterion for UV data (i.e., maximum of 10 missing days) had been used, ozone trend calculations for February would not have been possible and the number of years for other months would have been significantly reduced from 19 (average number of years for 20-day criterion for the months Mar-Oct) to 16.5 (average number of years for 10-day criterion for the months Mar-Oct).

The following was added to the paper: “Ozone data are not available for every day when UV data are available. The maximum number of missing days per month allowed for calculating monthly average ozone columns had to be relaxed from 10 days used for UV trends to 20 days. The same “gap correction” method as applied to UV data was used. Uncertainties in decadal ozone trends caused by gaps were smaller than 0.4% for all months, with the exception of February where the uncertainty was 1.4% (Table 1).”

Comment 13: Page 33, Figure 3: Explanation of the four correction methods are missing. I suggest adding a line for short reference, e.g. Method 1 is without scale adjustements, whereas Methods 2-4 apply scale factors determined to fit clear sky measurements to model calculations, using annual, summerly and monthly mean ratios, respectively.

Response: The following will be added to the figure caption: “Method 1 is based on the uncorrected dataset. Methods 2 – 4 apply correction factors based on ratios of clear sky measurements and model calculations that were averaged over annual (Method 2), summerly (Method 3) and monthly (Method 4) periods.”

Comment 14: Page 33 (Figure 3) and page 35 (Figure 5): The figures are fairly similar. Consider if the two figures could be combined into one.

Response: Figure 5 will be removed. The information contained therein remains available in Table 1.

Comment 15: Page 9 – 95.45% vs 95%. I suggest to use 95% throughout the text

for better readability, since, at least for a user like me it has no practical implication. Rounded to integer values, the numbers are the same.

Response: The calculations were performed for a confidence level of 95.45% and I think it is therefore appropriate to use this value. As explained in the paper (page 26627, lines 1-3), a level of 95.45% was chosen because 95.45% is the percentage of values within ± 2 sigma of a normal distribution.

Comment 16: Page 9 – line 22, misprint “form” should be from.

Page 15, line 6, bracket] is missing.

Page 16, line 1: The trend uncertainty ranges between 3% (March and April): Replace March with February.

Response: Corrected as suggested.

Comment 17: Page 16, line 19: ‘.. but trends determined by Method 2 are 1.7% larger on average’: From figure 3 it looks like Method 3 (Cannual) is larger.

Response: “Method 2” changed to “Method 3” Note that this error was only present in the version sent around for initial evaluation. The error was corrected before the manuscript was published by ACPD.

Comment 18: Page 18, line 6: replace ‘.. scattered downward by either air molecules (clear sky case) or cloud..’ with ‘.. by air molecules (clear sky case) and cloud..’.

Page 18, line 14, add ‘ and overcast conditions’ after 80%.

Page 28, Table 1: The ‘n’ factor is not explained. Add text n = number of years.

Response: Corrected as suggested.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 26617, 2011.

	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct
1991	26	27	30	26	(19)	30	31	30	31
1992	29	31	30	27	30	29	29	30	31
1993	21	31	30	31	30	31	31	30	31
1994	(14)	31	28	31	25	27	27	26	24
1995	18	30	30	27	30	21	31	28	22
1996	29	27	25	26	22	26	31	30	31
1997	28	31	30	31	30	31	31	30	25
1998	28	31	30	(17)	25	31	21	30	31
1999	28	31	30	31	30	31	31	30	31
2000	29	31	30	27	30	31	31	30	31
2001	18	31	30	27	30	31	31	30	25
2002	28	31	30	31	30	31	31	30	31
2003	28	31	30	31	30	23	31	30	31
2004	29	24	30	28	30	31	23	(8)	31
2005	25	(20)	26	(12)	24	31	31	28	31
2006	28	(15)	(15)	29	30	31	31	23	31
2007	25	27	30	31	30	25	22	(18)	31
2008	26	31	30	25	30	31	29	30	31
2009	28	29	30	31	28	(12)	(0)	(0)	(0)
2010	(0)	(12)	(10)	(20)	(8)	31	31	30	31
2011	22	26	30	(1)	(0)	(0)	(0)	(0)	(0)

Fig. 1. Number of days used for the calculation of the monthly average. Months with more than 10 missing days were not used in the trend analysis and are shown in parentheses.