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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Variability of the total ozone trend over Europe for the period1950–2004 derived from reconstructed data" *by* J. W. Krzyścin and J. L. Borkowski

Anonymous Referee #1

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Review of: "Variability of the total ozone trend over Europe ...." by Krzyscin and Borkowski.

Anonymous Referee #1

This paper attempts a sophisticated statistical analysis of ozone data obtained since the early 1950's. Data from a very few sites has been converted to represent a wide area. The paper's goal is to then reconstruct UV information using cloud inference data from pyranometers that have operated for a long time. The reconstructed data are then used to estimate trends.

The definition of trend in this paper is a bit unusual for the ozone and UV analysis field. Customarily, (see Ziemke et al., JGR) a synthetic time series is constructed with linear and higher order terms. The higher order terms remove the larger cyclic changes in the data leaving the linear coefficient, which is the long-term linear trend. Your definition seems to represent significant deviations from the mean over short time subsets of the entire series. The other proposed definition, using the endpoints of a smoothed time series does not seem realistic for the satellite era of measurements since 1979. Previously published ozone analysis shows a smoothed variation of ozone clearly reaching a minimum after the Mt. Pinatubo eruption, and then shows recovery. The recovery continues because of the atmospheric reduction of chlorine producing compounds. This is not well reflected in the present paper, which arbitrarily ends the O3 time series in 2004 instead of continuing to the end of 2007. There is no reference to satellite era ozone trends, which would differ from the results presented here. This must be fixed before publication. For latitudes up to 60 degrees, the variations smoothing kernel do not appear to be significant relative to a straight line fit for estimating trends (see your figure 4).

The paper must include the identity of various ground-based data sets used in this analysis. A table giving location, instrument type, and duration of the data would be a good idea. Also, some indication of the accuracy and precision of the reference data sets should be supplied to allow the reader to judge the quality of the reconstructed ozone field. This would also allow the estimation of the smallest change that is statistically significant. Currently, the article is using purely synthetic estimates of error from just statistical analysis. Since you are proposing this as an extension of real data, the errors must be properly propagated through the model.

Because of the availability of satellite data from 1979 to the present, the current analysis should end at 1978 and then join to the satellite data. Additionally, the reconstruction analysis should be performed for the satellite era, 1979 to present, and then compared to the satellite data. The reconstruction should use the same number of

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stations for the satellite era as for the pre-satellite era. Such a comparative analysis would lend some confidence to the reconstruction for the entire 1950 to 2004 period.

This paper should show the long UV and ozone time series obtained in the Moscow area. Of particular interest is the large change seen before the satellite era that is present in the Moscow data (see Chubarova).

This paper should not be published until the additional analysis is performed to improve the credibility of the data reconstruction. A paper based on statistical inference without validation, is not suitable, particularly when the data are available. Some explanation needs to be supplied as to why simple linear trends are not sufficient. The ozone recovery is a good reason, but then the analysis should be extended to 2007. See your Figure 4.

The paper is reasonably well written, but needs some editorial help to clarify the English sentence structure. Most of the figures are clear, but the contour plots need to be improved to make the results more readable. As it is now, the contours are not easily readable.

Questions and short comments

Does the NIWA O3 database contain the trends from the satellite era?

How was vorticity calculated before 1979? It sounds as if the model is based on balloon sondes for temperature.

This paper should show the long-term Belsk and Moscow UV data time series. The reconstruction should be compared with the data.

Computing trends from start and end points would not apply to the era from 1979 to the present. Why would it apply to the entire period? Mt. Pinatubo effects destroyed this possibility for the 1979 - 2007 period.

What is the mechanism for the winter month trend?

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Ozone has increased since 2000 even in the winter based on satellite data (SBUV-2).

How do the trends compare with daily variability?

This analysis contradicts the ground-based data obtained by Bias from Thessaloniki. The data shows that the main cause of UVB increase is a reduction in pollution aerosols not from O3 changes.

The differences in Figures 1 and 2 do not seem significant given the errors in the underlying measurements.

Referring to Figure 4: The linear fit to this data at 60N has a slope of -0.1 +/- 0.02 or +/- 0.04 at the 95% confidence level. The change is 6% over the entire period. This implies about an 8% increase in erythemal irradiance for 60N since 1950 and considerably more (15%) for short UVB wavelengths such as 305 nm.

The deviations of your kernel smoother from the straight line fit are smaller than the 2-sigma error limit, and are not significant, except for the 1995 minimum.

The scatter at high latitudes is too large for the kernel smoother to give believable results. It is not clear that these results are any better than a simple straight line fit.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 47, 2008.