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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Search for evidence of trend slow-down in the long-term TOMS/SBUV total ozone data record: the importance of instrument drift uncertainty and fingerprint detection" *by* R. S. Stolarski and S. Frith

Anonymous Referee #1

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1 Summary

This is an important and very well written paper. It describes details for the construction of the MOD global ozone data set, derived by using data from several TOMS and SBUV satellite instruments. The MOD data set is one of the most widely used near global historical records for total ozone.

The paper also points out that instrument drifts and intercalibration errors can have a substantial impact when trying to assess the statistical significance of recent departures of ozone levels from the previous decline. Finally, the paper demonstrates that interhemispheric differences in the observed departure signals are not consistent with expectations from a chemistry climate model (CCM) simulation.

All this is important information for the current discussion about the possible beginning of a recovery of the ozone layer, following the reduction of harmful CFC emissions. I think it is a very good paper.

I can support all the comments made by referees 2 and 3, especially the major comment about using just one CCM simulation to define an expected fingerprint.

2 Major Comments

I think the discussion of the sensitivity of the results in the paper is generally too short. For example in Figure 10, or the left panel of Fig. 11, the (blue) 2σ envelope line, and the (red) CUSUM line for the MOD data are nearly parallel. Small changes in the parameters (e.g. in the drift uncertainty, in the inter-instrument offset, or in the regression model from which the CUSUM is derived) might easily change the (red) MOD CUSUM from inside the (blue) 2σ confidence region to outside (or vice versa). In particular the date at which the blue and red lines cross might change dramatically, by several years. I think the authors should discuss this in some detail.

I also have some questions on the "finger-printing". Several papers indicate that a large part of recent total ozone increases in the Northern Hemisphere might come from dynamical/ transport changes (Hadjinicolaou et al., 2005; Dhomse et al., 2006; Yang et al., 2006). Since the regression for the MOD data does not include parameters for dynamics/ transport, the CUSUM for the NH might increase much more than would be the case with the inclusion of such parameters. This might explain why the CUSUM

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for the SH increases much less. If transport changes were included in the regression, similar CUSUMs ("finger-prints") for the MOD data might be seen in both hemispheres. I think the above papers need to be included in the discussion. The discussion of the interhemispheric differences in the MOD data needs to be expanded.

Given what I just said above, I am wondering why the CCM simulations show such fast CUSUM increases in both hemispheres. I would expect CCM-simulated increases to look more like the slow increase of the MOD CUSUM in the SH. Why does the CCM model predict such early dates for the significance of the CUSUM (or the "beginning recovery"). Are there transport changes in the CCM-simulation and what are they doing to the CUSUMs? Reliance on just one CCM simulation might be critical and should be discussed in more detail. I am wondering whether the CCM-simulation is providing the correct "finger-print".

Finally if have questions about the construction of the MOD data set and the derived drift uncertainties. These are dicussed in more detail in my comments below. Most important, does the 1 DU uncertainty derived from the spatial difference patterns (Figs. 2 and 4) really need to be applied to large area averages? Do the uncertainties of all the inter-instrument offsets really have to be added up? Or does the fact that all instruments have been calibrated to the "truth", i.e. external information, not tend to reset to zero for each instrument? (Similar to the drift being reset for each instrument). I think these questions are important for the sensitivity issues, mentioned above. They should be discussed in more detail.

3 Detailed Comments

page 3884, line 19: Add "statistically significant" before "demonstration". The ozone levelling may well be a response to the levelling of chlorine. The paper only shows that our confidence in this should be low for statistical and fingerprint reasons.

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page 3885, line 18: It would be good to give one or two basic references for TOMS and SBUV.

Figure 3: Please make clear (e.g. by writing label-text close to the corresponding data) to which instrument pair the data points/colors belong. Also, some of the pairs seem to be SBUV/SBUV pairs, so the axis title TOMS-SBUV is not correct.

From Fig. 1 and Fig. 3 it does not become clear to me, how the calibration level is propagated through: From Fig. 1 there is virtually no overlap between N9 SBUV and any other instrument (N11 SBUV, N7 TOMS, EP TOMS). Yet, from Fig. 1, N9 SBUV seems to be THE crucial instrument for having comparable calibration levels before and after 1995. Or are the authors relying on N11 SBUV being on a constant level before and after the dashed line around 1995 in Fig. 1? Please explain in more detail.

End of page 3889: I did not quite understand why the 1.0 DU instrumental uncertainty (from the spatial variability e.g. in Fig. 2) always has to be included. If you just look at the 50S to 50N average, Fig. 3 shows that, after applying the calibration offset, total ozone averaged over 50S to 50N from the two different instruments (e.g. N7 TOMS and N7 SBUV) agrees within a noise of 0.45 DU for any given month. Why is there reason to believe that there could be another 1 DU difference for the large area average? From Fig. 2 I can see that this additional difference between the two instruments could occur for a limited region, e.g. North Africa, or Equatorial South America. But it should not occur for the large scale average, especially since the patterns are similar between different years (Fig. 4). Can you explain this better?

The question about the 1.0 DU instrumental uncertainty is important, because it seems to be a major contributor to the uncertainty of the applied calibration offset.

Fig. 6. suggests that there is a 33% probability that the MOD calibration level after 2001 is more than 4 DU away from the 1979 calibration level. How would this number grow in the future? Would a MOD data set in 2020 be 8 DU (33% probability) away from the 1979 level? Would this grow forever in the future? Probably not, because

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additional information is introduced every couple of years by trying to calibrate each new individual instrument to the "truth". I think the pure error propagation/ Monte Carlo simulations in the manuscript ignore that.

How does additional information contribute to this? Each SBUV instrument, or the V8 data set, is individually calibrated using additional information. To me the scatter of the V8 inter-instrument offsets in Fig. 3 suggests that this is successful within ± 1 or 2 DU (except for N14). Fig. 3 to me does not give reason to believe that any of the instruments (e.g. N16 after 2001) is off by more than 4 DU, for a near global average.

Putting the same question in another way: The Dobson network relies on a primary standard, which is absolutely calibrated every couple of years. In my opinion the additional information from these absolute calibrations to a "true" value should "restart" the uncertainty lines somewhere around zero every couple of years. Something similar must apply to the MOD data set. How does the MOD data set compare to the Global Dobson data set (e.g. from Fioletov et al., WMO Assessment, 2006)? I think this would be a very important graph giving a much better feeling about the likelihood of the MOD data being on a different level by more than 4 DU after 2001. I think the authors should comment on these questions about additional information reducing the uncertainty. They should also consider adding a graph that compares the MOD data set with other data sets.

page 3890, Section 4: This is a very long and very important section. Several related but different issues are addressed. I think subsections would make this section easier to read, e.g.

4.1 CUSUM analysis (beginning at pg. 3890, l. 26)

4.2 Statistical significance with and without possible instrument drift (beginning pg. 3892, l. 10)

4.3 Fingerprinting with CCM results (beginning pg. 3893, l. 24)

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Fig. 7 caption: "total zone" -> "total ozone"

page 3891, 1st paragraph: There is an important recent paper by Yang et al., 2006. It would be very good to mention it here, and probably also in the introduction and conclusions. If necessary, Dr. Newchurch could probably make a copy available.

page 3891, lines 19, 23, and possibly other places: Which paper is "Stolarski et al. (2005)"? It is missing in the references.

page 3891, last paragraph: A comment on possible interference between 11-year solar cycle and volcanic aerosol signal for a 1979 to 1996 time series (Solomon et al., 1996) would be good. What happens when the regression is fit over the entire 1979 to 2005 time series? Presumably this could account better for the various influences like solar cycle, QBO, or no aerosol effects after 1996. It would reduce the likelihood that these variation are mis-represented after 1996 and do contribute more to the recent increases than expected from a 1979 to 1996 fit only. All these effects could then be subtracted, except for the trend/ chlorine term. Then a linear trend can be fitted to this remaining time series, using 1979 to 1996 data only. Then consider the CUSUM of deviations from this extrapolated trend. These slightly different CUSUM results could be plotted as another line (similar to the red one) in Figs. 9 to 12.

page 3892, line 18: Actually Yang et al., 2006 would be an even better reference. The appendix of this paper has all the math for CUSUM with auto-correlation.

page 3893 line 6: Point out here that this information is given in Table 1. It might be good to also add the auto-correlations to Tables 1 and 2. Maybe move the text from lines 15 to 19 up here. It would also be good remind the reader where the numbers in Table 1 come from. If I understood it correctly, the statistical white noise and AR1 number come from the regression, the 3 DU/decade trend uncertainty comes from Fig. 6.

page 3893, lines 2 to 6: I is not clear to me how you introduced the possible instrument

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drift into the Monte-Carlo simulations. Did you just pick a random drift from a Gaussian distribution with 0 mean and 2σ of 3 DU/decade? Or did you do the same thing as for Figure 6? Please explain in more detail. (The net effect might be more or less the same in both cases.)

Fig. 10, page 3893. I am missing a discussion on the sensitivity to the chosen instrument drift uncertainty. The red and blue lines after 1996 are close and nearly parallel. With a different instrument drift uncertainty (e.g. 2.5 DU/decade) the red line very likely would be outside of the 95% confidence interval (blue line) for the "no change of trend" hypothesis. This also applies to the left panel of Fig. 11.

page 3894, line 10: The reference should probably be Stolarski et al. (2006).

page 3895, 1st paragraph: I don't know if the significance is that important. Even more striking is the similarity of the red curves between Northern and Southern hemisphere CUSUMs in the CCM simulations (Fig. 12), compared to the dissimilarity of the red curves between the two hemispheres in the MOD data set (Fig. 11). Where does this come from? The relative lack of a signal from the Pinatubo Eruption in the Southern hemisphere in the MOD data set? Large recent ozone increases in the NH (MOD), e.g. due to transport changes, that are not taken out by the regression? (e.g. Hadjinicolaou et al., 2005; Dhomse et al., 2006). More than the significance, to me the question is why the MOD observations and the CCM CUSUMs agree so poorly. It seems that the large observed increases in the NH are at least partly due to transport changes, whereas the smaller and insignificant SH changes might be due to chlorine and bromine levelling. So without transport changes the NH CUSUM (for MOD) might look more similar to SH CUSUM (for MOD). Yet the CCM model indicates that we should see large CUSUMs in both hemispheres. Can we trust this model? What is going on here. I think the authors should comment a bit more on that.

Also, mention that your regression does not account for changes in transport/ dynamics, and that this may be a major cause for the interhemispheric difference of the

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CUSUMs.

Another question is, whether it is appropriate to use a possible intrumental drift for the CCM-model at all. It is the same CCM-model throughout. It should not jump/ drift. The statistical significance of the CCM-model CUSUMs should, therefore, not include possible drifts.

page 3896, lines 2 to 10. I can't follow this paragraph, especially the sentence in lines 3 to 5. Both Newchurch 2003 (or better Yang et al. 2006) and this paper estimate uncertainty envelopes for the null hypothesis of no deviation from the previous trend (green and blue lines in Figs. 9 to 12). Newchurch 2003 (or better Yang et al. 2006) use mathematical formulas, this paper use Monte-Carlo. The results are pretty much the same (Fig. 9 of this paper vs. Fig. 3 of Yang et al. 2006). In this paragraph I am missing a statement about the major point of adding possible instrument drifts, which is done in this paper, but is missing in Newchurch 2003, or Yang et al. 2006.

4 References

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Hadjinicolaou, P., et al., The recent turnaround in stratospheric ozone over northern middle latitudes: A dynamical modeling perspective, Geophys. Res. Lett., 32, L12821, doi:10.1029/2005GL022476, 2005.

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