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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

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

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Major comment #1: We agree that the significance could change with small changes in assumed parameters in the instrument uncertainty analysis. This is particularly true of the 30oN-60oN CUSUM in the left panel of figure 11 where the 2 sigma line parallels the cumulative sum of residuals from data for several years. We believe, but cannot prove that these residuals are as large as they are because of dynamic variability in the northern hemisphere that happened to go in that direction. The southern hemisphere, shown in the right panel of figure 11 is probably more representative of the chemical response to the leveling off of chlorine and bromine. Since our point revolves around the southern hemisphere not yet reaching the 2 sigma significance for change of slope, the exact crossing date for the northern hemisphere is not particularly important at this time. We should emphasize that waiting a few years will make all of this irrelevant assuming that ozone reacts as expected to chlorine and bromine changes. We have added some discussion for figures 10 and 11 that make our point clearer in the paper.

Major comment #2: We think that the referee is exactly right that the dynamical/transport changes that contributed to recent total ozone increases in the northern hemisphere are the reason for a significant CUSUM residual in the northern hemisphere compared to that in the southern hemisphere. The model results present a bit more of a difficulty. They are from a CTM (not a CCM) with imposed circulation and temperatures generated from a free-running GCM (GEOS 4). The CTM does show apparent trend slowdown at an earlier date than we are determining from data. This is perhaps because the variability structure of the model has just the right variations in the key years to make it look more like trend slowdown. We cannot be sure. The suggestion of this referee and the others questions the meaning of using only one model to establish the fingerprint explanation. We have come to agree with that and have removed the model CUSUM results from the paper. We still assert the general statement that trend slowdown should occur in both hemispheres and have rewritten the text to say this in a more careful manner. We have also made references to the papers suggested by the referee because those papers make important points about the dynamical contributions to trends.

Major comment #3: The uncertainties in the construction of the MOD data set are, of course, estimates. There is no way to know exactly what they are and we may either under or over estimate them. We feel like the 1DU uncertainty from the spatial difference patterns is very important. It represents some unresolved problems in the algorithms for SBUV and TOMS. When we merge two data sets, we are not sure within that amount, how well we have taken the offset into account. For the large area average, we may be able to say we have a little less uncertainty. But, for specific locations

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or latitude bands, it may be that the offset should be different. We have taken the conservative approach to apply this uncertainty estimate to all locations and each pair intercomparison. The inter-instrument offsets are not added up. They are effectively reset to zero for each pair. When the monte carlo calculation is done, the offset may go in either direction each time a new satellite is added.

To better address the issue of uncertainty, we did a sensitivity study to some of the assumptions and added the following paragraph to the text: “The derived significance level is based in part on the size of the instrument uncertainty, which was computed using several assumptions. We therefore tested the sensitivity of our results to a possible overestimate of the instrument uncertainty. The CUSUM is most sensitive to the inclusion of instrument uncertainty for the extra-polar time series. This can be seen in Figures 9 and 10. In Figure 9, the instrument uncertainty is set to zero. The NH CUSUM results are always significant, and the SH CUSUM curve nearly reaches the 2 sigma level when the instrument uncertainty is set to zero (not shown). Given these boundaries, our primary conclusions will not change even if the instrument error is overestimated.”

Response to the detailed comments by the referee:

The 1-DU uncertainty due to spatial variability in the SBUV/TOMS difference is necessary because we are using a single value to adjust one instrument to the other, when the actual difference between the instruments varies in latitude and longitude. The 1-DU error, based on the variability of spatial differences, is an estimate of how far off our single adjustment might be from the true adjustment at each location, and therefore should be included. It is a measure of our uncertainty of properly matching to the new “truth.”

We have clarified the text in the abstract concerning the statistical significance of our results. We have added basic references for TOMS and SBUV. We have fixed Figure 3 to make it clear to which instrument pair each set of data points corresponds.

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We do use NOAA 11 as the key to bridge the gap between N7 TOMS and EP TOMS. We have added the following text to the paper: “As seen in Figure 1, both the N9 and N11 SBUV/2 have some length of overlap with both TOMS instruments, so either can be used to set the calibration of N7 TOMS to the EP TOMS standard. We choose to use the N11 SBUV/2 data to bridge the gap, because N11 has a longer overlap with both N7 and EP TOMS, and analysis by the instrument team indicates the N11 calibration is maintained over the terminator period (Matt Deland, personal communication). Therefore we treat the N11 data before and after the terminator as a single consistent record. We also note that there is no overlap between the EP TOMS data used in this analysis and the N16 SBUV/2 data. In this case, we use the NOAA-11 data, which overlaps both, as a transfer standard to estimate the difference between the EP TOMS and NOAA-16 data. The calibration information is then propagated through the data sets by first calculating the offset between the EP TOMS data and the N9, N11, and N16 SBUV/2 data, then using the adjusted N11 SBUV/2 data to establish the calibration of the N7 TOMS, and then the N7 SBUV data. Finally, all of the adjusted data sets are averaged during periods of overlap, creating a single consistent data set.”

The referee points out several issues with respect to the estimated systematic uncertainty of 1.0 DU for each potential offset between pairs of instruments. Indeed there is a 33% probability that the MOD calibration level after 2001 is more than 4 DU away from the 1979 calibration level. We note that the level of uncertainty 5 years later is about the same. As the length of data record is extended and new instruments are introduced and calibrated relative to the previous instruments to the best of our capability, the overall uncertainty in the calibration level increases only slowly, if at all, with respect to 1979. It is true that the pure error propagation can lead to an increasing overall uncertainty. If instruments do not overlap, or one is relied upon for so long that the drift uncertainty accumulates, then the estimated uncertainty can continue to grow. In this case, the satellite record likely will drift with respect to the ground-based network. The error propagation calculations that we have done ignore this situation. We think that the above discussion points out the importance of attempting to maintain independently calibrated records to provide checks against one another.

The referee's comments about Figure 3 and the fact that the offset calibrations seem to be successful within 1 or 2 DU raises an important point about the difference between our ability to adjust the mean offset and the uncertainty that we are doing it correctly. We can always adjust the mean offset so that it is zero. However, there always remains some uncertainty in whether the adjustment has been done correctly. We have made our best estimate. It is possible that we have been overly conservative in our estimate and that the real uncertainty is somewhat smaller. The V7 MOD comparisons was compared with other data sets in Fioletov et al. [2002]. The comparison of the V8 with other data sets will be included in the 2006 WMO/UNEP assessment.

Section 4 has been rewritten. The separate section on model fingerprinting has been deleted. Other sections have been separated into 'CUSUM Method', 'CUSUM Statistical Uncertainty' and 'CUSUM Instrumental Uncertainty' for clarity.

The referee is correct that the possible interference between 11-year solar cycle and volcanic aerosols for a 1979 to 1996 time series is important. When the regression is fit over the entire 1979 to 2005 time series, these two influences can be better separated. However, it was not our intention to change the CUSUM approach that has been used in previous papers. Our intention was to evaluate the impact of the inclusion of instrument uncertainty on the existing method. The basic problem with trying to statistically demonstrate the beginning of a slope change after 1996 is that the time period when the solar cycle signal is separating itself from the aerosol signal is the same time period for which we are looking for trend slow-down.

The referee had a number of comments related to how we determined uncertainties and how sensitive they were to our assumptions. We have moved the reference to Table 1 forward in the text and given a summary of the source of the table's values in the text. One specific note is that we added the instrument drift uncertainty to the CUSUM time series exactly as we did in the original uncertainty estimate for Figure 6.

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That is, we randomly selected a drift for each case from a normal distribution with zero mean and 2 DU width (1 sigma).

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