ATP Ms. 2013-437:
Response to the Author's Reply:
"Decadal-scale responses in middle and upper stratospheric ozone from SAGE II version 7 data" by E. E. Remsberg

Here are a few responses to the author's reply that are intended to assist with the revision:

(1) The reply did not address whether the manuscript showed that the SC-like response profiles from SAGE II and HALOE during 1992-05 are really consistent with representative 2D and 3D model estimates. This is an important issue as previous studies (e.g., Soukharev and Hood 2006; WMO 2007; Dhomse et al. 2011) have found significant disagreements, e.g., in the uppermost stratosphere, where observations indicate a response of 2-4% from solar minimum to maximum while most models that account for solar-induced temperature changes yield a response of less than 1% (see, e.g., Figure 6 of Dhomse et al. 2011). As argued in my review, the manuscript does not clearly show an agreement because the reduced SAGE II response in the tropical upper stratosphere during 1992-2005 can be attributed to interannual dynamical variability during the short measurement record (Figure 11 and discussion beginning on p. 11, line 22). Please address this aspect in the revision or (preferably) just delete the part about comparisons with the 2D model.

(2) As stated in comment 3 of the reply, the primary purpose of this study is to demonstrate that the SAGE II v7 data is of better quality than that of v6.2 for evaluating decadal-scale variations in ozone and its trends. If so, then it may not be appropriate to include in the abstract and conclusions sections any strong statements about the nature of the observed solar-UV induced ozone response and whether it agrees with models. In my opinion, the latter requires a more detailed study using a physically based MLR model. However, I can agree that the adopted statistical approach has some value for identifying decadal-scale dynamical effects that are not of solar origin. A manuscript that is focused on this aspect would be an improvement.

(3) The author argues that a more physically based MLR analysis of the entire 22-year SAGE II v7 data record is unnecessary because Kyrola et al. have already submitted for publication in ACP such an analysis. However, their study (as described in their ACPD manuscript) analyzes a combined SAGE II / GOMOS data set and focuses almost entirely on trend analysis. There is only one figure (out of 16) and one short paragraph in the paper on the topic of the solar-induced ozone response:

"As an example of the fitted proxy terms, we show in Fig. 13 the solar term as percentage to the constant term of Eq. (4). The solar term is scaled by the constant term of the time series fit. The statistically significant solar contribution is 1-3% in the stratosphere and 2-4% in the mesosphere. Note that the values are not totally symmetric around the equator."

The Kyrola et al. manuscript appears to be a useful and detailed analysis of long-term ozone trends. However, the authors apparently did not intend it to be a complete analysis of the solar-induced response in the SAGE II v7 data (or the combined SAGE II / GOMOS data). First, it is not clear what the stated percentages mean. What is the assumed change in F10.7 from solar minimum to maximum? What does the "constant term" consist of and how does it vary with altitude? How was autocorrelation of the MLR model residuals accounted for in estimating the final solar regression coefficients? Is it possible to extract any information about the seasonal variation of the ozone profile response? Some example plots of ozone time series averaged over low latitudes at a series of altitudes should have been compared to the 10.7 cm solar flux to demonstrate the extent to which a solar cycle variation can be seen visually in the time series.

A thorough investigation of solar-induced signals in the SAGE II / GOMOS ozone data set therefore probably requires another full manuscript besides the Kyrola et al. work. Again, a physically based MLR model should be used and the entire available record should be analyzed if this is to be done by either the present author or by the authors of the Kyrola et al. manuscript. I can agree that such an analysis is beyond the scope of the present manuscript but I disagree that that the submitted Kyrola et al. manuscript is sufficient for this purpose.

(4) If the revised manuscript will still discuss the SC-like response derived from the v7 SAGE II data, then there needs to be some discussion and comparison with previous analyses of long-term satellite data sets. The latter consist primarily of the SAGE I/II data and the merged SBUV data. The most recent such analysis was reported by Dhomse et al. (2011), who analyzed the "SAGE-corrected" merged SBUV data set described by McLinden et al. (2009). As shown in their Fig. 6, the tropically averaged SC response derived from the latter data set compares reasonably well with that estimated by Randel and Wu (2007) using SAGE I/II data. Both show positive responses in the upper stratosphere (about 3.5% for the SAGE/SBUV data set and about 2% for the SAGE I/II data set). Similar results were obtained from v8 SBUV data by Soukharev and Hood (2006). The large positive responses in the uppermost stratosphere estimated from these alternate data sets over long time periods (up to 25 years) disagree with most 2D and 3D model simulations. This should be noted.

The author argues that the new v8.6 SBUV data set supersedes earlier versions, implying that earlier published results (e.g., WMO 2007) are now out of date. The v8.6 data set may indeed be better for estimating long-term ozone trends. However, for the purpose of estimating the solar cycle component of ozone variability, I must disagree with this assessment. The fundamental problem is that a large number of measurements from different satellite platforms must be merged together after the mid-1990's to construct this data set. These satellites had orbits that drifted so that the local time of measurement at a given latitude also drifted. Merging such data sets together inevitably introduces errors in apparent interannual variability. This is especially problematic in the upper stratosphere because of the ozone diurnal cycle, which becomes important at higher altitudes. Up until the end of 2003, a clear solar cycle variation of ozone is apparent in tropical averages of this data set at various pressure levels (see, e.g., Figure 2 of Soukharev and Hood 2006). Analyzing the earlier v8 SBUV data therefore yielded reasonably reliable results for the SC component of variability. However, similar plots of the v8.6 data set show a much weaker solar cycle variation after the mid-1990's. The clearest solar cycle variation is apparent during the period of operation of the Nimbus 7 SBUV instrument (1978-91), which did not drift significantly in local measurement time. For these reasons, it is unlikely that the 1979-2012 v8.6 data set will provide an improved estimate for the SC component.

(7) Finally, I still hope that the author will find a way to provide some realistic error bars on Figures 12-13. If averaging the data across several latitude bins yields standard deviations that are artificially small, then there should be some way to correct for this and provide error bars that are realistic in the author's judgment.