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## ***Interactive comment on “Analysis of SAGE II ozone of the middle and upper stratosphere for its response to a decadal-scale forcing” by E. Remsberg and G. Lingenfelser***

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Received and published: 9 September 2010

General:

Paragraph 2: The answer to your first question is Yes; to the second question, No. As to question 3, we were hoping to see similarities in the solar response profiles from the HALOE and the SAGE II ozone. We also wondered whether those responses would agree with modeled responses, as opposed to what was reported in WMO (2007). We posted a similar reply to Referees 1 and 2.

Paragraph 3: We found no significant structure remaining in the time series residual, particularly for the period of September 92 to August 05. We shall include estimates

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of uncertainty for the 11-yr and linear trend terms. On the other hand, an extension of the SAGE II data to 1984 will require that we repeat all the analyses and replace the 11-yr and trend terms with solar and EESC proxy terms. That new set of terms, while likely representative, would be fundamentally at odds with our goal of fitting an 11-yr term and checking its phase to see whether there might be decadal-scale, dynamical forcings affecting the ozone time series. We wanted to use the same approach as for the analyses of the HALOE ozone in Remsberg (2008). In addition, we have recently become aware of a manuscript by Dhomse, Chipperfield, Feng, and Haigh on the solar response in tropical stratospheric ozone that they submitted in August to GRL for review. They have conducted independent analyses of SBUV data, of the SAGE II data from 1985-2005 and of the HALOE data from 1992-2005, in addition to model simulations of the solar response. They find SAGE II and HALOE ozone responses that are very similar to what we are reporting. It is likely that their SAGE II ozone studies will satisfy your desire to see updated analyses extending from 1984 onward, such that it should not necessary for us to repeat their work. We consider that their separate analysis is confirming and complementary, rather than duplicative of ours. We will reference it, assuming that their manuscript is accepted for publication shortly.

## Specific Comments:

p. 17310, lines 20-23: Results in Figure 12 may be confusing to you. The SAGE II response of 1979-2005 (asterisks) is merely a reproduction of that in WMO (2007) and not a re-analysis by us. That WMO result is shown again here to emphasize the differences that we found with it using the later period of 1991 to 2005.

p. 17313: SR and SS points in our time series are latitude, bin-averages of the occultation measurements that were obtained as the satellite progressed in its orbital viewing opportunities of the Sun. Each bin-averaged point was obtained within no more than a few days at low to middle latitudes and is based on a least 5 profiles. Point spacing is somewhat variable, averaging from 20 to 25 days depending on latitude and season; we shall say that in our revised manuscript.

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p. 17314, lines 4 to 7: The sub-biennial cycle arises from the interaction of the annual cycle and the QBO (see also our reply to Referee 1). For our time span of 91 to 05 or 92 to 05 the linear term is the net result of any weak EESC effects plus the trends in ozone versus altitude due to the radiative cooling effects of increasing greenhouse gases. You are right to think that the effects of the chlorine ought to be seasonally dependent, though very weakly so at low latitudes. However, we are not electing to conduct analyses of SAGE II ozone based on data for a given month or season of the year because we would then only have 14 points for each of our data time series. Terms of such regression models would not be as significant in our opinion. To the contrary, each of our time series contains at least 200 points for the fit of the regression model.

p. 17320, lines 17-21: We could have generated results for the much wider latitude span of 25S to 25N, but it should be clear from Figure 6 that the 11-yr responses were larger in the NH than in the SH, in part due to a likely perturbation in ozone for the year following the Pinatubo eruption. Our analyses for separate latitude bins are providing more information in our opinion.

p. 17336: It is concluded that the anomaly for the 11-yr phase in Figure 10 is indicative of a decadal-scale NH response from dynamical forcings during 91 to 05, rather than from the solar cycle or reactive chlorine. Those latter two forcings ought to lead to 11-yr phases that are symmetrical across the NH and SH.

p. 17318, lines 17 to 18: Numerical values will be provided in our revised manuscript.

Technical corrections:

p. 17310, line 26: Yang, E. S., D. M. Cunnold, R. J. Salawitch, M. P. McCormick, J. Russell, J. M. Zawodny, S. Oltmans, and M. J. Newchurch: Attribution of recovery in lower stratospheric ozone, *J. Geophys. Res.*, 111, D17309, doi:10.1029/2005JD006371, 2006.

p. 17316, line 11: We will delete “cycle 22” and rewrite as “Solar maximum uv-flux

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values occurred broadly. . .”

p. 17316, lines 25-28: We will revise the sentence.

p. 17321, lines 24-25: For example, Nicolet (JGR, vol. 86, p. 5203, 1981) showed that Lyman-alpha intensities at 60 km are only about 0.1% of those at 80 km, while solar variations at the longer wavelengths of the Schumann-Runge bands of O<sub>2</sub> are very small. Thus, flux variations from solar max to solar min cannot explain the positive SAGE II ozone response of the lower mesosphere, which is at odds with that from the HALOE data as well.

p. 17328-17331: We prefer to leave them as separate figures. Each one is intended to convey a different visual aspect of the observed ozone variations. For instance, 11-yr and trend terms are apparent in Figure 2, SAO and OBO terms in Figure 3, interactions of AO, SAO, QBO, and IA terms in Figure 4, and AO and QBO terms in Figure 5. Figure 5 also shows the fact that the model does not fit the data well from 1991 to mid 1992, presumably due to volcanic effects.

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Interactive comment on Atmos. Chem. Phys. Discuss., 10, 17307, 2010.

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