

## ***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***

**Anonymous Referee #2**

Received and published: 27 August 2010

### **1 General remarks**

The paper by Remsberg and Lingenfelser provides yet another analysis of decadal (or solar-cycle) variations on the basis of SAGE II measurements. To me, the paper reads more like a report ("this is what we have done"), than like a scientific paper ("this is what we have learned"). A scientific paper should give novel results, error bars, interpretations, and advance our understanding. My main criticism is that this manuscript does not present any such take-home messages. We all know that probably there is a 0 to 4% amplitude 11-year cycle in ozone. We all know that matters are complicated by chlorine trends, QBO, volcanoes. We also know that different analyses, instruments,

C6886

time periods tend to give different results. Are they really different, if we consider error bars? What is the point of throwing yet another fish (=analysis of 1991/1992 to 2005 SAGE data) into the pond? What have we learned from this paper?

### **2 Major points**

I tend to agree with reviewer 1: The paper needs to be improved substantially to be acceptable for ACP. I think the following major points should be addressed:

- Bring out the new findings and new understanding achieved by this paper much clearer.
- Address error bars in much more depth. This requires deriving and plotting error bars, e.g. from the statistics of the multiple linear regression analysis (MLR) fitting coefficients, or through boot-strap methods. I think it also requires to consider what happens when different predictors / proxies / terms are put into or taken out of the MLR. Analysis with and without linear trend terms, with hockey-stick trends, with or without chlorine proxies like EESC (e.g. from Newman et al., ACP, 2007) should be compared. Comparing such different results would provide a much better idea about systematic uncertainties.
- The full SAGE II data set (1984 to 2005) should be compared with the shorter 1991/2 to 2005 HALOE time frame. Here I agree with reviewer 1. The long time frame is very important. Comparison of the HALOE period results with the longer time frame is necessary to put things into perspective with all the other studies. Would Remsberg and Lingenfelser even get the same results as previous studies, when the same underlying data are used?

- The altitude vs. pressure coordinate problem in the HALOE vs. SAGE comparison should be resolved. Right now, this is the rug under which much dirt is swept. I think it would be very important to see how HALOE trends and solar cycles would look in SAGE coordinates. Remsberg et al. would be prime candidates for such an analysis of the HALOE data. Since both instruments use solar occultation over the same time period, results should really be very similar. If not, it would be important to know why.

### 3 Minor points

page 17311, lines 8 to 10: If I remember correctly, chlorine effects were not fully neglected in that paper. Rather a linear trend term was used, like in the present paper. A linear trend term sort of represents the chlorine change from 1991 to 2005 (plus other linear changes, e.g. of temperature). However, chlorine did not change fully linear over that period. Chlorine (and other) deviations from linear (chlorine maximum around 1997 to 2000) will alias, e.g. into a solar cycle term. This should be pointed out here. It should also be quantified better. As indicated in my major points, it could be addressed by comparing regressions based on different input predictor time series.

page 17311, lines 25, 26: Again, I think this coordinate system effect should not only be stated. It should also be looked at.

page 17313, around line 15: What was the bin-size in the time dimension? A day? A week? A month? Please clarify.

page 17316, lines 25 to 29: Please also give the time periods and instruments used in these analyses. I think it is so important to keep these differences in mind.

page 17318, lines 15 to 17: Certainly ozone changes have also had a big effect on lower stratospheric cooling and on the downward movement of pressure surfaces in

C6888

height coordinates. This should be mentioned. Also, is the magnitude of the SAGE vs. HALOE trend differences consistent with Rosenfield et al.?

page 17321 line 11 to page 17322 line 7: This discussion is a good example for the short-comings of the paper: A lot of details and side-information are presented, but to me it does not become clear, what the main conclusion is. Is this secondary maximum at the stratopause robust/ significant in the first place? For the 1991 to 2005 SAGE data it is much less pronounced (Fig. 12), the HALOE data don't show it at all (Fig. 12). Water vapor changes, but how does that alias into the 11-year variation? The temporal shape of water vapour change is similar to the temporal shape of the chlorine change. So if water vapour messes things up, why can it be assumed that chlorine does not do similar things. Non-linear changes like these need to be considered in more detail!

Overall, I think this manuscript has good potential. However, when reading it for the first time a few weeks ago, I scribbled on the cover page: What are the main results? Error bars? Different regressions (MLRs)? In agreement with reviewer 1, I would hope that for an ACP paper these "scribbled questions" can be addressed in a majorly revised manuscript.

---

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

C6889