

## ***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 #1**

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### General Comments

It is always welcome to have a new analysis of real data concerning the problem of the response of stratospheric ozone and temperature to the solar cycle. I have a long-standing concern that we do not yet have the definitive word on what the atmospheric signal is that models should be expected to simulate. My concern is based primarily on the issues of shortness of the record and the robustness of the analysis used to extract the signal.

The present paper by Remsberg and Lingenfelser uses SAGE II ozone data between late 1991 and 2005 to revisit the issue of solar cycle variation of ozone. Although

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SAGE II data starts in 1984, they made the choice to restrict the data set to make it consistent with Remsberg's earlier analysis of HALOE data. I think that this is an unfortunate choice based on my comment above about the shortness of record being a major limitation.

I have a major issue with this paper in that it does not present uncertainties for the various fitting terms. Section 3.2 starting on page 17319 states that “It is difficult to provide definitive errors for the coefficients of the 11-yr and the linear trend terms of the foregoing subsection, because these two terms may not be strictly orthogonal for a 14-yr time series.” This statement is precisely the reason that the paper must devote significant attention to the question of estimating uncertainties. The paper mentions several times that the terms have confidence intervals greater than 90% in many cases without explicitly describing where they were significant or how that significance was determined.

There are methods for calculating uncertainties to fitting with non-orthogonal functions; bootstrap methods come to mind. My suspicion is that a thorough analysis of uncertainty, including assessment of auto-correlation of residuals, would lead to a conclusion that the solar signal derived from this limited data set is not significant over much of the domain. The derivation of an 11-year signal from a 14-year data set is fraught with dangers and uncertainties. The analysis would be improved with extension of the data set to the full 21 years of SAGE II measurements. I am not convinced that 21 years would be a long enough record with the SAGE sampling frequency to do more than get a general picture of the solar cycle response of ozone. Regardless of my opinion on this, a thorough uncertainty analysis by the authors could answer some of the questions concerning significance.

Other Comments:

I don't believe that the “good continuity” is a reasonable argument for the reality of the derived terms. I realize that they are independently derived, but unusual meteorological

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years near the end points can lead to false interpretations of signals in the analysis and these will show correlation to the structure of the unusual meteorology.

Sub-biennial oscillation seems to have been invented from fits in previous Remsberg papers. I cannot find any physical explanation as to why there might be such an oscillation. This leads me to ask, how do periodicities in the SAGE orbit potentially alias into time-series analysis? Furthermore, I do not believe that the data is sufficient to differentiate between a QBO and the sub-biennial oscillation.

#### Overall Recommendation

I do not believe that this paper should be published in ACP until the authors make use of the existing first 7 years of SAGE data and present a thorough analysis of uncertainties. I started this review somewhat skeptical of the existing data analyses for the ozone response to solar cycle. This paper did not clarify any issues regarding solar cycle impacts on ozone and it adds to the confusion.

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