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Atmospheric Chemistry and Physics Discussions

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 #3

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GENERAL COMMENTS

The paper "Anaylsis of SAGE II ozone of the middle and upper stratosphere for its response to a decadal-scale forcing" by Remsberg and Lingenfelser describes an analysis of stratospheric ozone derived from SAGE II from 1991/1992 to 2005. The authors used a multiple-linear regression model with several sinosuid terms, and a linear trend, to describe ozone variability in order to extract the signal associated with the 11-yr solar UV-flux. Similar analysis of a longer time period of SAGE II ozone have been published already (Lee and Smith, 2003; Soukharev and Hood, 2006), however, using the same time period as covered by HALOE is new. I recommend the manuscript

suitable for publication in ACP after some major revisions have been applied. The suggested revisions are outlined below.

After reading the manuscript it was not clear to me what the exact purpose of the paper was. Did the authors want to show that the SAGE II data is suitable to record the effects of UV forcing on stratospheric ozone? Or did they want to present a detailed comparison between results obtained from data sets measured with different techniques (SBUV and SAGE)? Or was the purpose of the paper to shed light on the differences between the solar cycle response profiles from HALOE and models versus results from SAGE instruments (as shown in WMO, 2007)? In my opinion the manuscript has to be reworked carefully to present a clear message about the purpose and outcome of the analysis. In addition, many minor (and in that sense maybe unnecessary) details are presented which further distract the reader from the storyline of the paper. Those details should be reduced to allow the reader easier access and understanding of the major points of the paper.

More information about the quality of the regression fits should be presented in the manuscipt. On page 17323, line 1 to 7, it is stated that it is only possible to resolve various long-term responses from data if all forcings and trends are either accounted for or insignificant. In my opinion it should be shown more clearly that the choice of time period and the choice of explanatory variables (linear trend and harmonics) is sufficient and that the obtained results are meaningful. I strongly recommend therefore, in agreement with reviewer #1 and #2, that more information about error bars and comparisons with the longer time period for SAGE II (start year 1984) is provided in the analysis.

SPECIFIC COMMENTS

Some of the following comments might repeat some of the points mentioned in the section above. They are listed here again to connect them to a specific section in the manuscript.

Page 17310, line 20 to 23: The apparent disagreement for the solar cycle response profiles from HALOE and models versus results from SAGE instruments for the period 1979-2005 is mentioned here (see also Figure 1). This disagreement still exists for the analysis from 1991/1992 to 2005 (see Figure 12). No further explanation about the reason for this disagreement is given, although the introduction section implies that one of the goals of the presented analysis is to investigate this problem. Please discuss in more detail.

Page 17313: It is not clear what temporal unit is used for the multiple linear regression calculations. Are they based on individual profiles? Please add some explanation to clarify this point.

Page 17314, line 4 to 7: It is not obvious why an oscillation with a period of 21 months (sub-biennial oscillation) is used here. Please explain the origin of this oscillation or explain in more detail the facts on which its usage is founded. In addition, it is not clear to me why a linear trend term is used in the regression model. The analyzed time period of SAGE II data is shortened to avoid having to account for fast changing stratospheric chlorine concentrations. However, EESC concentrations are not linear during 1991 to 2005 (see Figure 1-12 of WMO 2007). Is the linear trend term used here to describe EESC concentrations during the analyzed period (which would be an unfavorable choice in my opinion)? If that is the case, why is it not seasonally dependent in the regression model (although the influence of chlorine on ozone definitely has a seasonal dependence)? Or is the term used to describe the effects of slowly increasing greenhouse gases? Please explain in more detail what the linear trend term represents and why it is used.

Page 17320, line 17 to 21: How different would the results presented in Figure 12 look like if the original data points between 25°S and 25°N would have been used in a regression model, rather than taking the average of the ozone response profiles for the different latitude zones? Please add a few sentences to the manuscript that address this issue.

Page 17336: Figure 10 shows a phase difference of "-2" in the northern mid-latitudes up to about 37km. In this region the max minus min variations seem to be higher than 1% (see Figure 9). This phase difference increased by analyzing the one year shorter time period. Please discuss the implications of this in more detail in the manuscript.

In general, I would appreciate if more details about trend sizes (page 17318, line 17 to 18: "... Where the trends in Fig. 8 are appreciably different than zero...) or comparative analyses (page 17316, lines 23 to 24: '... will lead to max minus min ozone responses that are very similar") rather than relatively vague verbal descriptions would be given.

TECHNICAL CORRECTIONS

Page 17310, line 26: Yang et al. (2006) cited but not listed in reference list

Page 17316, line 11: Please explain the term "Solar cycle 22".

Page 17316, line25 to 28: Please rephrase this sentence so that it becomes more clear that the similarities between the results of the given references and Figure 6 is confined to 30°S.

Page 17321, line 24 to 25: Please add a reference for the statement "...SC-variations of the Lyman-α flux for the dissociation of H2O are very small near the stratopause."

Pages 17328 to 17331: Is there is a specific reason why Figures 2 to 5 are not combined? If not, I would suggest combining them to facilitate comparisons between the graphs.

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