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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Further analyses of the decadal-scale responses and trends in middle and upper stratospheric ozone from SAGE II and HALOE" *by* E. E. Remsberg

Anonymous Referee #1

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Review on the paper by E.E. Remsberg "Further analyses of the decadal-scale responses and trends in middle and upper stratospheric ozone from SAGE II and HALOE"

The paper presents the analysis of a time series of mid to upper stratospheric ozone from the SAGE II instrument covering the period 1984 to 1998. The ozone time series are fitted by a combination of a linear trend, and some sinusoidal oscillations to account for annual and semi-annual cycles and the 11-year solar cycle. Further two sinusoidal terms with periods of 28 and 21 months derived from Fourier analysis are introduced to account for a QBO-type variation and another not further assigned variation. The paper

refers to two earlier publications by Remsberg and Lingenfelser (2010) and Remsberg (2008) where the combined SAGE II and HALOE time series of ozone for the period 1991 to 2005 were analyzed.

All three publications focus on the response of ozone in the middle to upper stratosphere on the 11-year solar cycle. The author of the current paper explicitly states that the current analysis is an extension of the previous analysis presented in Remsberg and Lingenfelser (2010), addressing two previously unresolved reviewer comments regarding the similarity of solar-cycle induced trends for different periods, and an inconsistency introduced by the choice of quantities (number densities on altitudes versus mixing ratios on pressure). Further some issues regarding data filtering and data binning have been improved compared to Remsberg and Lingenfelser (2010). The method of the time series analysis, i.e. the fitting by a linear and several sinusoidal terms, without using proxies for irregular variations, has been kept intentionally similar to the method applied by Remsberg and Lingenfelser (2010) for reasons of consistency. The paper starts with the presentation of the re-analysis of the combined SAGE II - HALOE time series covering 1991 to 2005, and then turns to the analysis of the SAGE II time series covering the years 1984 to 1998. Regarding the re-analysis of the first data set, the results of Remsberg and Lingenfelser (2010) were confirmed.

General comments:

Is this paper considered as technical note? If so, I wonder whether the criteria for technical notes are fulfilled with the manuscript. According to the ACP web site on manuscript types, "Technical notes report new developments, significant advances, or novel aspects of experimental and theoretical methods and techniques which are relevant for scientific investigations within the scope of the journal." The presented reanalysis does not introduce new methods, nor new data sets or novel experimental and theoretical methods and techniques. To my opinion, the paper should be considered as regular research article.

The paper is clearly written, and all methods are well explained. The motivation and the focus of the paper is clearly presented. The analysis of longer-term satellite time series on global ozone data is a topic of high interest, and many publications already exist, not only but also on the time series of the satellite instruments presented here. I think the scientific understanding of the atmospheric community has reached an advanced state such that new contributions need to be of high sophistication to add anything new to the available knowledge. I wonder whether an analysis of a time series which does not take into account proxies of the most prominent irregular variations in the atmosphere (QBO, ENSO, change of ODS) and only fits a few sinusoidally oscillating variations to the time series (annual, semi-annual, 28-months and 21-months oscillations, 11-year cycle) can contribute anything new. At minimum, the multiple linear regression analysis performed here would need to provide an assessment of the uncertainties of the derived phases, amplitudes and trends which would allow to judge on the quality of the regression analysis, and would help to resolve some issues which remain open in this paper.

Specific comments:

p 25014, l 20: It would be interesting to see the Fourier analysis of the time series; this could prove the existence of the sub-biennial term which was already challenged by one of the reviewers of the Remsberg and Lingenfelser paper.

p 25017 ff: The analysis of the SAGE II time series of the years 1984 to 1998 provides some pronounced differences to the later period, which, however, are not explained in depth. There is some speculation that the phase lag of the solar signal response found in the lower tropics could come from "bad" fits (page 25017, second para), but the analysis of the fits and their quality is not performed in a quantitative manner. Differences in the solar cycle max-min responses are found - even in parts of the time series which cover the same years - but not further analyzed. Residuals between the time series and their fits are assigned to irregular variations like unusual QBO amplitudes or ENSO events in a speculative and hand-waving way - this could have been

nailed down by just including proxies for such irregular variations in the fitting of the time series (page 25017, last para - page 25018, first para). For differences in the ozone linear decreases, the plausible explanation is provided that ozone follows the active chlorine compounds the variation of which is different during the two periods. Although plausible, this does not provide any new insight.

Fig. 9 and related discussion: it is very hard to judge from this presentation if there are periodical terms in the residuals or not. By sure it is not sufficient to judge just "by eye"; a more quantitative analysis, e.g. by Fourier analysis, would be appropriate.

Fig. 11 and related discussion: comparison to more recent model results would be appropriate. Further, although an inconsistency between representation on pressures and altitudes, respectively, has been identified as possible cause for the discrepancy between HALOE and SAGE (and this inconsistency has already been noted by one of the reviewers of the Remsberg and Lingenfelser (2010) paper), no attempt has been made to resolve the inconsistency by transforming one of the data sets from pressure to altitude levels or vice versa. Differences between the ozone responses in the lower stratosphere derived from the two SAGE II data sets of different periods are assigned to imperfect fits, however without any quantitative analysis - discussion of these differences in the light of the uncertainties of the fits would help to sort out if these differences are significant or not.

Fig. 12 and related discussion: Again, no attempt has been made to resolve the inconsistency between the HALOE (mixing ratio at pressure levels) and the SAGE data sets (number density at altitude levels) for the same period and to quantify which part of the observed discrepancy is due to this different representation (page 25020, first para).

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 25011, 2011.