Atmos. Chem. Phys. Discuss., 5, S5891–S5896, 2005
www.atmos-chem-phys.org/acpd/5/S5891/
European Geosciences Union

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Large decadal scale changes of polar ozone suggest solar influence" *by* B.-M. Sinnhuber et al.

B.-M. Sinnhuber et al.

Received and published: 30 March 2006

We thank both referees for their detailed review and constructive comments.

Following the suggestions of both referees we have included in our manuscript now a new section on the relation between the observed ozone anomalies and the equatorial wind in the upper stratosphere and have expanded the discussion and conclusion sections.

In our detailed response below we show the original comments of referee #2 in italics and our reply in plain text.

In the introduction very briefly changes in stratospheric ozone related to changes in solar UV radiation are mentioned. In the suite of the paper this is however no longer *referred to and the indirect influence of 11-year solar UV variations are neither compared to the effect of EEPs nor discussed and presented in detail. I agree with reviewer 1 that this work needs to be adequately reported (e.g., Kodera and Kuroda, 2002; Gray et al., 2001) and compared to the proposed EEP mechanism. To do this, further work is required as already suggested by reviewer 1.*

We have now included another section on the impact of equatorial wind anomalies on polar ozone. We thank both referees for their suggestions on this point. Briefly, what we found is that certain features in the modelled ozone anomalies can be related to anomalies in the upper stratospheric equatorial wind (in particular for the year 2002). However, there is no evidence that the observed decadal scale polar ozone changes may be related to changes in the equiatorial wind. See also our reply to referee #1.

A correlation with other more commonly used indices like F10.7, Ap, MgII etc should be presented as comparison to the proxy for the flux of energetic electrons.

We had already included the F10.7 index in Fig. 1(b) and from this it is clear that there is also a close correlation between the ozone anomalies and the F10.7 index. The correlation between F10.7 and the DJF ozone anomalies is $R = -0.72$, as compared to $R = 0.91$ for the correlation between DJF ozone and the GOES electron flux. We will add the correlation coefficient with F10.7 to the text.

Both F10.7 and the MgII index are good proxies for solar-UV changes and well correlated with each other. There is no particular reason why we have used F10.7 instead of MgII, except that the F10.7 index is commonly used and well charaterized.

The Ap index averaged over July to December is not correlated ($R = -0.2$) with the DJF ozone anomalies. A short discussion on the relevance of the Ap index will be added to the text (see also below and our reply to referee #1).

Abstract The abstract only mentions decadal oscillations in Arctic ozone but in the paper also results from Antarctic stations are presented which are however not that *clear. If the focus of the paper is on the northern hemisphere this should be then clarified throughout the manuscript. May be "Arctic" could be exchanged by "polar" like in the title.*

Will be changed.

Page (P) 12105, Line (L) 5: "Here we present long-term observations " The results from the three different stations cover only 14 years, i.e. only a little bit more than one solar cycle, this is not long-term for me. The question also arises how significant the results are for such a short period of time. Should be reformulated. The SBUV data cover a longer time frame but I still wouldn t call this long-term. Also in section 2 it should be mentioned for which years the observations are available, this is so far only seen in the first figure.

We will remove the word long-term.

P12107 L9/10: Why is the ozone anomaly for DJF shown together with the July-December average of the electron data. It is mentioned in the text that very similar results are obtained for different averaging periods. So why don't they use the same period for both?

The mechanism we propose is that electron precipitation produces enhanced levels of NOx and/or HOx in the mesosphere and subsequent catalytic ozone destruction in the stratosphere, as discussed in the following paragraph on P12107. I.e., one expects a lag between electron precipitation and ozone changes in the stratosphere. It is not well known how long it would take to propagate a signal from the mesosphere into the winter stratosphere. However, as we have indicated, our results are not very sensitive to the actual averaging period. This is partly due to the fact that the GOES electron fluxes have minima around the solstices, so that the interannual variability in the averaged flux will be dominated by the values around equinox.

We will modify the text to make this point a bit clearer.

P12107L5-7: electron measurements do not provide a direct measure of electron precipitation but it is used anyway as proxy for the flux of energetic electrons. I wonder whether it would be more appropriate to use e.g. the Ap index.

It is true that the GOES electron flux measurements do not provide a direct measure of the precipitating electron flux. Unfortunately it is not clear how the GOES electron relate to electron percipitation. What really is needed is a long time series (over at least one or two solar cycles) of precipitating electrons.

It may be that the flux of precipitating electrons is modulated by the Ap index. However, we do not find any correlation between the Ap index and observed ozone anomalies (see also our comment above).

We will add a short discussion on the Ap-index to the text.

Figure 3: What is the reason for removing 1990 and 1991 (it looks rather like 1991 and 1992!)? Obviously the line would go down if these points were included and would therefore not fit to the data from Ny-Alesund. The main conclusion that a positive correlation between ozone and the solar cycle exists would be destroyed, too.

As said in the text and caption to Fig. 3 the two data points have not been connected by the line for illustrative purposes only. However, in order to avoid any confusion on this point we will now draw the connecting line also through these two points.

(The two data points removed from the connecting line are October 1990 and October 1991. To account for the fact that the autumn measurements from the two hemispheres are 6 month apart October values are drawn here at year + 9/12 and April values at year + 3/12. In other words, the tick mark for 1990 is centered at January 1990.)

In any case we believe that our conclusions from Sec. 3.2 remain unchanged: Although the SBUV data alone are presently not fully conclusive, they support the idea that (a) there are decadal scale ozone changes in the order of 1ppm between solar minimum and solar maximum, (b) the changes occur more or less simultaneously over NH and

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SH and (c) these changes can also be found for the period before 1990.

These conclusions are based on the SBUV measurements over almost 25 years from two hemispheres. The fact that the two NH October measurements for 1990 and 1991 show a rather different behaviour does not destroy our main conclusions. In particular as at least for October 1991 there are relatively large differences between the SBUV measurements and the Ny-Alesund sonde measurements.

In addition to the years 1990 and 1991, also the NH measurements for October 2002 show unexpectedly low ozone mixing ratios. As we will show in our newly included section on the relation with the equatorial upper stratospheric wind, the low ozone values in autumn 2002 can be related to an anomaly in the upper stratospheric equatorial wind during that period.

P12110, Conclusion, first sentence: the large decadal scale variation is not shown for early winter but for DJF, so "early" should be removed.

The word early will be removed.

In general the conclusions are too short and leave many open questions. The results should be discussed here in more detail.

We will expand the conclusions and discuss in more detail the evidence and the remaining open issues.

However, it is inevitable that many questions remain open at this stage. The aim of this paper is to present observational evidence for large decadal scale changes in polar stratospheric ozone and to make some suggestions for a potential mechanism. Clearly more work is needed (but beyond the scope of this paper) to better characterize the ozone changes and to better understand the potential mechanisms.

The paper addresses the dynamically active season. The summer as dynamically undisturbed time of the year might be even better to look for a clear solar signal.

As can be seen from Fig. 2, the largest ozone anomalies occur during the autumn to spring seasons. The main intention of our paper is to report on these large decadal scale ozone anomalies. We defer a more detailed analysis of the summertime ozone time series to a future study.

Figure1a: It would be useful to include percentage ozone variations on the right hand side y-axis.

Although we agree that this could be useful we prefer not to add a percentage scale: By choosing a particular reference for the percentage deviation (deviation from average, deviation from solar maximum, deviation from model, etc.) one introduces already at this stage a bias towards a particular model one has in mind. Rather we prefer to show the observations (and the CTM results) standing on their own and refer only qualitatively to a $\approx 20\%$ variation.

Figure captions Fig1b: units of the F10.7cm solar flux?

Will be added to the caption.

Fig1c: " shown in panel (a) (red line) "

Will be changed.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 12103, 2005.