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

B.-M. Sinnhuber et al.

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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 #1 in italics and our reply in plain text.

(1) The primary evidence for an EEP influence is the high correlation (R = 0.91) shown in Figure 1c between negative residual ozone (model minus measured) and the > 2

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MeV July to December electron flux at geostationary orbit (6 Re) over a 14-year period. However, as shown in Figure 1b, the electron flux at 6 Re correlates inversely with the solar cycle as measured by the 10.7 cm radio flux (F10.7), a good proxy for solar UV variations. This means that residual ozone (measured minus model) correlates positively with F10.7. Although the correlation may not be as high as found for the electron flux at 6 Re, it is probably high enough to be statistically significant. If so, then it would be difficult or impossible to distinguish between these two forcing mechanisms on the basis of statistical correlation analysis alone. The authors should calculate correlation coefficients against suitably averaged F10.7 and all other relevant indices (e.g., Ap index) to investigate in an unbiased manner whether other forcing mechanisms can actually be eliminated at the 95 per cent confidence level.

First of all we want to emphasis that the main aim of our paper is to report on the observations of unexpectedly large decadal scale changes in polar stratospheric ozone. We do not insist that our observations provide clear evidence for the effect of energetic electron precipitation. Rather, we propose energetic electron precipitation as the only "known" mechanism that could possibly explain the observed ozone changes.

Although we haven't discussed this in detail, it is almost certain that the large ($\approx 20\%$) decadal changes in polar ozone can not be explained by direct solar-UV effects. However, we cannot completely rule out that solar-UV changes influence polar ozone indirectly through some dynamical coupling. However, there are a number of points that argue against this mechanism: (a) if the decadal scale polar ozone changes were due to indirect solar-UV effects, why do they not appear in modelled ozone from our CTM driven by analysed wind fields? Remember that the CTM captures many other features of the observed ozone changes, including the apparent QBO influence. (b) Why do coupled chemistry-climate models not reproduce the large observed decadal-scale changes when driven with observed solar-UV changes? (c) It has been suggested that the polar stratosphere during winter has two different internal modes ("cold, strong vortex" as opposed to "warm, weak vortex") and the selection of a particular mode may

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depend on weak external forcings such as solar-UV changes (e.g., Kodera and Kuroda, 2002). However, the observed decadal ozone changes show an ozone deficit around solar minimum during all winters, largely independent of the specific meteorological situation during that winter.

We will make some changes to the discussion and conclusion sections to make clear that we cannot rule out other potential mechanisms for the observed decadal scale ozone changes.

Correlation coefficients between the ozone anomaly and the F10.7 as well as the Ap index will be included.

(2) Statistics aside, a major problem with applying the correlative result of Figure 1c to propose that EEP is the primary causal mechanism is that the electron flux at 6 Re is not an accurate measure of the precipitation flux into the polar atmosphere. As shown by Siskind et al. (GRL, v. 27, p. 329, 2002) and Randall et al. (JGR, v. 103, p. 28361, 1998), a much better measure of the precipitation flux (and the resulting effects on polar odd nitrogen and ozone) is the auroral Ap index. Siskind et al. specifically show that observed polar odd nitrogen interannual variations correlate well with the Ap index. The Ap index (see, e.g., http://www.ngdc.noaa.gov/stp/GEOMAG/kp_ap.shtml) correlates positively with the solar cycle rather than negatively as does the electron flux at 6 Re. Therefore, based on these previously published results, EEP should produce a variation of odd nitrogen that correlates positively with the solar cycle; the corresponding variation of ozone would correlate negatively with the solar cycle, opposite to that shown in Figure 1. Therefore, unless our understanding of precipitation-induced chemical effects on ozone in the polar stratosphere is flawed, it is unlikely that EEP can explain the decadal variation of polar ozone that is reported in this manuscript. The authors should provide a review of the Siskind et al. results (and other related work) in the manuscript. If they feel that EEP can nevertheless explain the results of Figure 1, then they should provide quantitative arguments for this proposal.

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 measurements relate to electron precipitation. 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 reply to reviewer #2). Siskind et al. (2002) show that NOx enhancements in the SH polar stratosphere can be related to changes in the Ap index. However, this time series is rather short and basically also consistent with the idea of an increased NOx production towards solar minimum (possibly due to EEP) in addition to NOx enhancements as a result of solar proton events.

A number of papers by Callis and coworkers (some of which we cite in our paper) did suggest that EEP could lead to reduced levels of polar stratospheric ozone around solar minimum times, as did the two more recent modelling studies by Langematz et al. (2005) and Rozanov et al. (2005). These studies indicate that EEP could be a potential candidate to explain the observed decadal scale ozone changes. Our findings thus provide additional observational evidence for the effect of EEP on stratospheric ozone. However, they do not provide a definite final answer whether or not the observed ozone changes are caused by electron precipitation.

We will change the text accordingly to make clear that the link between the electron flux measured by GOES and the flux of precipitating electrons into the atmosphere is still an open issue. This will also include a brief discussion of the Ap-index as a proxy for NOx enhancements due to particle precipitation.

(3) There is an increasing quantity of evidence that solar UV-induced variations in the low-latitude upper stratosphere can indirectly perturb the polar stratosphere in winter. For example, Gray et al. (Q. J. R. Meteorol. Soc., v. 127, p. 1413; p. 1985, 2001) have shown using both model and data that January-February mean polar tempera-

tures at 30 hPa are most sensitive to equatorial vertical zonal wind gradients near 50 km altitude in the previous September-October period. These zonal wind gradients are, in turn, directly influenced by solar UV variations through photochemically induced ozone changes, radiative heating changes, thermal wind changes, and resulting effects on the onset of the westerly semi-annual oscillation phase. Also, Kodera and Kuroda (JGR, v. 107, doi:10.1029/2002JD002224, 2002) argue that the stratopause circulation during the middle winter can either be in a strong polar night jet or in a weak polar night jet mode, depending sensitively on the relative importance of radiative and planetary wave forcing during early winter. Which mode is selected in a given winter can therefore be influenced significantly by weak external forcings such as the QBO and solar UV variability, both of which affect winds in the tropical upper stratosphere. Kodera et al. (GRL, v. 30, doi:10.1029/2002GL016124, 2003) also show that general circulation models are currently unable to accurately simulate realistic speeds for tropical winds near the winter stratopause and, therefore, these models are not yet able to fully simulate interannual variability caused by the solar cycle. Finally, Kuroda and Kodera (GRL, v. 32, doi:10.1029/2005GL022516, 2005) have recently reported evidence for a solar UV 11-year influence on the structure of the Southern Annular Mode, which includes positive feedback effects of ozone heating. While the details of the connection between solar UV induced effects in the tropical upper stratosphere in autumn and the wintertime polar lower stratosphere remain to be worked out, there is enough evidence for this connection that the authors should consider it in their manuscript. Is it possible, for example, that the autumn equatorial wind gradients identified by Gray et al. are also responsible (through wave-mean flow interactions, resulting influences on the occurrence of sudden warmings and the selection of preferred internal modes in the winter stratospheric circulation) for the DJF mid-stratospheric ozone variations reported in this manuscript? In their revision, the authors should consider this indirect UV mechanism on at least an equal footing with the EEP mechanism proposed in the current manuscript. Although the model employed by the authors accounts approximately for ozone changes caused by differing meteorological conditions, does it

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account fully for interannual ozone changes caused by changes in meridional ozone transport (associated with warming events, for example)? The latter ozone changes are most likely to be influenced by the solar UV flux.

First of all we want to thank referee #1 for the suggestion to look for a connection between polar ozone during winter and anomalies in the upper stratospheric equatorial wind. We have now included a new section on the impact of equatorial wind anomalies on polar ozone. Briefly, what we find 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, we don't find any correlation ($R < 0.2$) between measured ozone and the equatorial wind at 1 hPa. In particular, based on our analysis the large decadal-scale changes in observed ozone can not be explained by anomalies in the equatorial wind.

In summary, we still regard energetic electron precipitation as the most likely candidate to explain the observed ozone changes, although it is clear that more work is needed (but beyond the scope of this paper) to better understand the mechanisms leading to the observed ozone changes.

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