The authors thank the anonymous reviewer for the comments to improve the paper. Your comments are repeated in normal letters while our response is highlighted in bold.

Major comment

I am puzzled that the ozone response to the electron flux index in the combined satellite data is so different from the one in Ap or F10.7: see, for example, the second row of Fig3 or Fig5 (right column). In particular, there is a positive ozone response in August-September in the 30-50km layer, which is quite in contrast with the negative (expected) ozone response in Ap or F10.7 (especially in Fig3). This positive response is also clear in the MIPAS data. Intriguingly, there is a hint of a corresponding positive response in Ap in Fig5. The authors describe this positive ozone anomaly and mention that it is not related to NOx, but they do not seem to provide a clear explanation.

Is there an issue with the electron flux index (incl. electron flux measurement correction and detector issues), which the authors indicate to be contaminated by proton fluxes ? Many recent studies (e.g. Anderson et al., Nature Communications, 2014 and ref therein) rather use electron fluxes measured by polar-orbiting rather than geostationary satellites. Some additional discussion of this issue and discrepancy is needed, if the authors believe that the electron flux composites need to be retained in the paper.

We believe there is a good reason to keep the 2MeV related results, because Ap and ≥2MeV represent different particle populations. Ap is supposed to represent particles of lower energies compared to the ≥2Mev flux. Further note the time series shown in Fig. 4, which reveal a different behaviour for Ap and 2MeV. Assuming a data set without gaps, "high Ap years" are 2002-2006 while "high 2MeV years" are 2005, 2007, 2008, and 2010. Thus when calculating the O3 amplitude [(years of high index – years of low index)/mean] the results are expected to be different. This is supported by the corresponding correlation coefficient "r" between these two time series. The respective values are r = 0.0 (2002-2010) and r = 0.5 (2005-2010). Therefore a different O3 response to Ap and 2MeV is reasonable, from 2002-2010 (Fig. 3) in particular. We do not discuss the results presented in Fig. 3 in much detail due to the possible cross-correlation between Ap and F10.7 (see p.31261, l.18-23). Considering the results for 2005-2010 (Fig. 5), in fact the general agreement between the O3 pattern associated to Ap and 2MeV is quite strong, in MIPAS observations in particular (p.31262, l.24-25), although they present different particle energies and are only moderately correlated.

Thus we added:

p.31261, l.12 (description of Fig. 3)

"Considering that the Ap responds to lower particle energy levels compared to 2MeV and

that the behaviour of both indices is essentially different from 2002-2010 (Fig. 4), the different O3 amplitudes associated to Ap and 2MeV are still reasonable."

We also changed p.31263, l.1-2 (description of Fig. 5):

"Since Ap represents lower particle energy levels compared to the 2MeV and both indices are only moderately correlated (see Fig. 4), the similar results strongly indicate a related source mechanism, suggesting solar wind variability."

p.31264, l.7-14 (end of section 3.1)

Since we have no definite explanation for the positive correlation for Aug-Sep at 35-50 km we slightly modified the section (highlighted in red):

"Additionally, the area of high positive Ap/O3 structure between 35 and 50 km from August–September cannot be completely explained by the NOx/O3 cycle. In detail, the respective Ap influence of NO2 is close to 0 and consequently well below the 95% significance level, while the respective MIPAS ClONO2 amplitude (not shown here) reveals positive values which are also mostly below the 95% significance level. These results are at least not in conflict with a higher O3 amplitude. Furthermore, this positive Ap impact on O3 is essentially less visible in the composite results than in MIPAS data, and a corresponding composite analysis for Ap/NO2 is necessary for a more detailed investigation. But this is not possible due to non-existing NO2 measurements from SABER and SMR. Thus no definite explanation can be given at this state and this feature is a subject of a future work. However, it should be pointed out that this structure does not harm the underlying mechanism proposed to explain the identified negative O3 amplitude and subsequent downward transport."

Regarding the issue with the electron flux data set we rearranged p.31257, l.13-19:

"The ≥2MeV electron flux (2 MeV), including the flux of all electrons with energy levels above 2 MeV, was measured by the Geostationary Operational Environmental Satellites (GOES) and the corresponding time series were downloaded from ftp://ftp.ngdc.noaa.gov/STP/SOLAR_DATA/SATELLITE_ENVIRONMENT/Daily_Fluences/. Note that the 2MeV data set also considers contamination effects on the electron detectors on the spacecrafts due to protons >32 MeV. Furthermore the 2MeV data is obtained from geostationary satellites which perform in-situ measurements in the radiation belts and consequently do not directly provide observations of precipitating particles. However, it is very likely that there is at least a positive relation between 2MeV and precipitating relativistic radiation belt particles. Thus, the 2MeV is not used as a proxy of precipitating particles but as an indicator of the influence from the magnetosphere. Precipitating particle integral fluxes in polar regions are observed by sun-synchronous Polar orbiting Operational

Environmental Satellite (POES) detectors and the corresponding data correlates better with geomagnetic indices than the GOES electron fluxes (Sinnhuber et al., 2011). However, the respective measurements of the POES instruments tend to underestimate the fluxes from ground-based observations during weak geomagnetic activity (Rodger et al., 2013). Since this study focus on 2002 – 2011 and an essential part of this time interval overlaps with low geomagnetic activity, GOES data and Ap are used instead of POES measurements."

Rodger, C. J., A. J. Kavanagh, M. A. Clilverd, and S. R. Marple: Comparison between POES energetic electron precipitation observations and riometer absorptions: Implications for determining true precipitation fluxes, J. Geophys. Res. Space Physics, 118, 7810–7821, doi:10.1002/2013JA019439, 2013.

Sinnhuber, M., S. Kazeminejad, and J. M. Wissing, Interannual variation of NOx from the lower thermosphere to the upper stratosphere in the years 1991–2005, J. Geophys. Res., 116, A02312, doi:10.1029/2010JA015825, 2011.

Minor comments

Section 2.1 A word of caution might be warranted on the fact that the ERA-Interim data is poorly constrained by actual observations in the mesosphere. The analyses are mostly model-driven.

p.31253, l.8:

We added "Note that ERA-Interim data is primarily model-driven at mesospheric altitudes but the individual PV results look reasonable at each height interval.

Abstract: "Inter-annual" is not appropriate here. You are looking at a "climatological" seasonal cycle and not at inter-annual (i.e. year-to-year) variability. Intra-seasonal (?)

Changed "inter-annual" to "intra-seasonal" throughout the paper, including title and abstract.

Section 3.1.2. Shouldn't N2O5 be also mentioned in addition to HNO3 and other reservoir species? The elevated NOx would also be sequestered in N2O5. The conversion to HNO3 through the hydrolysis of N2O5 is believed to lead to the EPP-induced HNO3 polar enhancements.

We slightly modified the respective section (highlighted in red), p.31263, l.20-24

"A possible reason for this behaviour might be that NO_2 is stored in reservoir species, like

$ClONO_2$, HNO_3, and N2O5, due to reactions with ClO, OH, and NO3, respectively. However, N2O5 is converted to HNO3 via water ion cluster chemistry (López-Puertas et al., 2005, their reactions 1, and 8-12) which was also investigated with respect to EPP for conditions without solar proton events by Stiller et al. (2005). These reactions eventually lead to lower NO_x concentrations, consequently slowing down the catalytic O_3 depletion."

Stiller, G. P., G. M. Tsidu, T. von Clarmann, N. Glatthor, M. Höpfner, S. Kellmann, A. Linden, R. Ruhnke, H. Fischer, M. López-Puertas, B. Funke, and S. Gil-López: An enhanced HNO3 second maximum in the Antarctic midwinter upper stratosphere 2003, J. Geophys. Res, 110, D20303, doi:10.1029/2005JD006011, 2005.

CLONO2 should be written ClONO2

done

The work "feedback" is used on many occasions. Wouldn't the word "response" be more appropriate since ozone is responding to the EPP forcing but there is no feedback from ozone on the forcing factor? (unless when applied to the ozone self-healing where there is a feedback mechanism).

We used "feedback" in order to avoid to many repetitions of "response". But since "feedback" is not used in a correct way, we changed:
-"feedback" to "signal" (p.31250, l.11)
-"O3 feedback to both indices" to "O3 structure associated to both indices" (p.31262, l.26-27)
-"feedback" to "response" (p.31263, l.20; p.31265, l.5; p.31266, l.7)