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Interactive comment on "Energetic particle induced inter-annual variability of ozone inside the Antarctic polar vortex observed in satellite data" *by* T. Fytterer et al.

Anonymous Referee #2

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This paper examines the relation between ozone satellite measurements in the southern hemisphere high latitudes and various indicators of energetic particle precipitation (EPP). Satellite measurements are obtained by combining of SMR, SABER and MIPAS instruments over the years 2002-2011. The data is sorted according to the boundary of the austral polar vortex. The results nicely show a downward-propagating negative response in ozone, originating in the lower mesosphere-upper stratosphere in early winter. Below about 25km, there is a positive response in ozone, which the authors attribute to the sequestration of NOx into longer-lived reservoirs, hindering halogen-induced ozone loss, and ozone self-healing.

The paper is concise and well-written, and it provides a significant advance in the field by demonstrating a climatological (albeit still based on few years) ozone response to EPP. I recommend publication in ACP provided the authors address my major comment below.

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.

Minor comments.

1) 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. 2) 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 (?) 3) 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. 4) CLONO2 should be written ClONO2 5) 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).

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 31249, 2014.