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Atmospheric Chemistry and Physics Discussions
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Interactive comment on "Stratospheric ozone intrusion events and their impacts on tropospheric ozone" *by* Jesse W. Greenslade et al.

Anonymous Referee #3

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The paper by Greenslade and coauthors presents an analysis of ozone soundings from three locations between -31°S and -69°S over several years. The authors analyse the profiles for ozone enhancements in the troposphere, which they link to stratosphere-to-troposhpere exchange associated with cut-off lows and cyclones. Based on these enhancements they estimate the fraction of excess ozone from the stratosphere as percent of the tropospheric ozone column. They calculate a flux of ozone for the southern hemisphere by using tropospheric columns from GEOS-Chem times this fraction times the frequency of occurrence of STT events. This flux estimate is more than an order of magnitude lower than estimates from literature using various techniques. The authors conclude that these differences are due to conservative estimates of several thresholds and assumption regarding their method.

Observationally based estimates of ozone transport especially in the southern hemi-

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Discussion paper

sphere are sparse and therefore valuable. However, the authors provide a flux estimate which is far from other studies, probably due to the sparse spatial and temporal coverage. If this however is the case, the method is simply not applicable in this case and the deduced flux does not mean anything. If the method is valid for the given data set I miss a careful analysis of the reasons for the discrepancy. Therefore I don't see the paper as an ACP paper in the current form.

Major comments:

1) Overall the manuscript leaves me a bit puzzled, since I'm not sure what to take out of this work. The authors state that their value of the ozone enhancement fraction might be largely underestimated by a factor of ten. If this is the case it is difficult to get the benefit of the study. Though the approach is reasonable, maybe the statistics and spatial coverage is to small to cover the full variability and frequency of occurrence of STT events for a quantitative flux calculation for the southern hemisphere. If the difference between observations and models is really between factors 30-200 depending on the reference (p.21, l.8-12), this needs more clarification than simply replacing observation with model results to find agreement with other studies. I found this approach at least very questionable.

2) Further: I missed a quantification of the uncertainties. This is partly done in section 2.5, but it is e.g. not clear why a threshold of 99% is the best choice nor which factor specifically leads to the very low flux estimate. I suggest the authors use a bunch of northern hemispheric sondes with higher spatial and temporal density to gauge their approach before applying it to the southern hemisphere.

Minor points: p.1,l.5: Please add the period of observations

p.4, l. 9: At least mention the dynamical tropopause, it is more common than ozone...

p.4, l.11: Correct definition of the thermal tropopause "... provided the lapse rate averaged between this altitude ..."

p.4, l.20 (also Fig.1): The tropopause definitions are mixed here. Why do the authors not include the dynamical definition? The effect of the pure lapse rate criterion is misleading under specific synoptic conditions as correctly stated. This might explain the very low cases in Fig.1.

p.6, l.15: How many model levels are between the sea level and 14 km? How many model levels are between 8 and 14 km and how are sonde and profile data compared? Pointwise or vertically averaged to fit the model levels?

p.6, l.15+: The sonde profiles are compared against model data of 2 x 2.5 degrees grid sizes (and the vertical model resolution). How well does the model resolve the soundings? How do the authors estimate the fraction of ozone transport which is missed due to unresolved structures? Why do the authors don't interpolate to the time window of the sounding (or at least use the according model time step)?

p.11, l.17: Even if you did a subjective method, could you explain a bit more in detail in the manuscript, how you distinguished different potential situations? What are upper tropospheric "low pressure fronts"? Tropospheric intrusions (3D!) in the stratosphere or stratospheric cut-offs (fully detached)?

p.11,l.20: What are "ozone folds" without other sources of upper tropospheric turbulence and how are these related to the polar vortex?

p.11, l.25: Explain: "...ozone enhancements derived from dry stratospheric air..." - didn't you use the methods and criteria from sec.2?

p.12, Fig.6. and related discussion (shortly before section 3): Please show a cross section of PV since most likely the ozone peak is related to a tropopause fold.

p.12, last line: What is meant with increased winter activity? More tropopause folds, stronger tropospheric winds, cyclone activity, etc...? Please be more precise. How do you expect the vortex to affect the tropopause?

p.14, l.6-20: Why do you use N2 as indicator? The relation you found is interesting, but not necessarily valid since stability is not conserved. Why should it be 'retained' when crossing the thermal tropopause? In general the thermal tropopause is ill defined under these conditions. Why not simply taking PV for this excercise or humidity as a measurement based quantity?

Fig.11 and related discussion: Couldn't you provide scatter plots (or Taylor diagram) of the column ozone between sondes and model?

Fig.3 caption: Units: concentration or mixing ratio?

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