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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "The impact of polar stratospheric ozone loss on Southern Hemisphere stratospheric circulation and climate" *by* J. Keeble et al.

Anonymous Referee #2

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The manuscript studies the effects of ozone depletion on Southern Hemisphere atmosphere. The subject received a lot of attention in scientific literature during the last decade. The present study differs from the previous ones by the method used to isolate the impacts of ozone depletion. While previous studies typically used either prescribed ozone trends (thus neglecting chemistry-climate feedbacks), or varied the concentration of ozone depleting substances, ODS, (thus introducing the greenhouse effects of ODS), the present study suppresses the activation of ODS on polar stratospheric cloud particles. Although the technique is only applied in winter stratosphere in both hemispheres, its indirect effects extend beyond that. This can be seen

from total ozone changes, which is reduced globally, except in Northern Hemisphere mid-latitudes from November to February where total ozone is increased. These side effects of the method are not discussed enough in the manuscript.

Overall, the manuscript is well written and the results are presented in a clear way. My problem is that it is difficult to see what are the novel findings of the manuscript because the atmospheric impacts of the ozone depletion demonstrated here have been extensively discussed in previous studies. I suggest that authors should clearly emphasize novel findings of the manuscript, in particular paying more attention to the strengths and weaknesses of the method. The text dealing with the ozone depletion impacts could be shortened considerably. Also the authors should provide quantitative comparison between their results and those previously published. More specific comments are given below:

Specific comments:

1. The diagnostics shown in Figs. 1,3,4,5,10 have appeared in a number of previous studies. Can the authors comment on what are the new findings due to their method? They say that the total ozone loss is underestimated when compared to observations because, in particular, because ozone loss due to gas phase chemistry is not increased. Is that a weakness of the method? And what about greenhouse effects due to fixed ODS concentrations? Can they be diagnosed by comparing present results with results from studies where ODS were changing?

2. Although the technique is only applied in winter stratosphere, the ozone is changed globally. The authors only say that this is because a new equilibrium state is reached. But what are the exact mechanisms? Can the transport of ozone depleted air from the vortex explain it? And what about areas where ozone chemical lifetime is shorter than the transport timescales, such as the upper tropical stratosphere? I think such a discussion is needed in order to understand the applicability of the method for climate studies.

3. The increase of ozone in Northern hemisphere mid-latitudes is interesting. The authors speculates that it might be related to the NH ozone losses, but why not to SH ozone losses? The increase is seen already in November, when there is hardly any Arctic ozone loss. On the other hand November is the time of maximal ozone loss in the Antarctic. Might it be more than a coincidence? Also, Figure 9 top shows a significant strengthening of the equatorial winds from July to October. Why is that? Can it be linked to the NH ozone increases?

4. I find Section 4 not so needed in the manuscript, especially because it is difficult to see what is new here. The separation into shortwave cooling and dynamical heating was done e.g. by Keeley et al. (Geophys. Res. Lett., 34, L22812, 2007). The discussion of EP-flux changes was done in McLandress et al. 2010. If the novelty of the manuscript is to introduce the new method, then it can be restricted to reporting mean quantities (Sections 3 and 5) and be more focused on quantitative differences between this method and previous approaches.

On the other hand the authors could elaborate on the mechanisms. For example they state that the decrease of the EP convergence in spring is not well understood. They suggest that it can to be related to Charney-Drazin criteria linking wave propagation to critical values of zonal winds, but can they show it through calculations? Moreover, one can notice that, while the increased EP-flux convergence in summer is somewhat balanced by the induced residual circulation (since increased downwelling implies strengthened poleward circulation), there is no indication that the decreased EP-flux convergence in spring is consistently balanced by a weakened residual circulation. Can this point be elaborated in the manuscript?

5. The authors seem to use the term ‘wave breaking’ as a synonym to the convergence of EP-flux. Why is that? The generalized Eliassen-Palm theorem (see Eq. 3.6.2 of Andrews et al. ‘Middle Atmosphere Dynamics’ 1987) states that a nonzero EP-flux divergence can be related to either wave transience, or dissipative effects, or non-linear effects including wave breaking. Unless the authors can rule out the wave transience

or dissipation as the reasons for non-zero EP-flux divergence I suggest using ‘the convergence of EP-flux’ term, not ‘wave breaking’.

Minor comments:

Page 18058, line 27, and page 18061, line 26:It is more correct to say that ‘polar night jet shifts poleward’, not ‘polar vortex shifts polarward’, based on zonal mean winds.

Page 18059, line 15: deceases -> decreases

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 18049, 2014.