Anonymous Referee #1
General Comments

General comments:

The present study investigates the impact of polar stratospheric ozone depletion on stratospheric climate over Antarctica by means of two model simulations with the coupled chemistry-climate model UM-UKCA. For this purpose, heterogeneous chlorine activation was suppressed in one of the model runs. Compared to previous studies, which used prescribed ozone climatologies or fixed ODS concentrations, the presented method allows for chemistry-climate interactions, and does not affect gas-phase chemistry. The analysis of the model results focuses on stratospheric temperature and circulation changes. Changes in tropospheric climate are also briefly touched. The manuscript is very well written, the argumentation is easy to follow, and the figures are well prepared. I have a couple of remarks and suggestions (see below). After taking these comments into account I recommend this paper for publication.

We thank the referee for their positive and detailed comments. Our detailed response is given below

1. The introduction reads a bit like a textbook on stratospheric ozone chemistry including a history of stratospheric ozone research. I would suggest to shorten this part of the introduction, but to extend the discussion of previous studies of the climatic impact of polar stratospheric ozone loss. Furthermore, I would like to see a deeper discussion of the advantages and disadvantages of the different methods (prescribed ozone climatologies vs. fixed CFCs vs. suppressed heterogeneous chlorine activation). Currently one might get the impression that the presented method is without any failure, which is hard to believe.

We agree that there is merit in expanding the discussion on previous studies and the pros and cons of the different techniques, and have done so in the text. Certainly we did not mean to imply that our method was without its own shortcomings.

2. The impact of polar stratospheric ozone loss on circulation and climate of the stratosphere is investigated by suppressing heterogeneous chlorine activation in one of the model simulations. As stated in Sect. 2, PSC particles are allowed to form in both model simulations. Therefore, the radiative impacts of PSCs do not change between both model runs. I agree that the formation of PSCs and their radiative feedback are not artificially suppressed by the applied method, but due to the large temperature changes in the lower polar stratosphere, I would also expect large differences in the PSC formation (total surface area density, but also PSC composition (NAT vs. NAT/ice)) between the two model runs. This will influence the radiative effect, but also the denitrification and dehydration of the lower polar stratosphere. Especially a change in lower stratospheric water vapor concentrations might have a strong impact on the longwave cooling. Such feedback processes are completely neglected in the present study. They might be of minor importance compared to the ozone effect, but I think this needs to be shown.

The reviewer is correct in saying the radiative impacts of PSCs will not be identical in the two runs due to chemistry-climate feedbacks. We have clarified this point in more detail to avoid any confusion. It should be noted that modelled PSCs form predominantly in JJA, leading to dehydration and denitrification during the winter. The temperature changes on the other hand occur later, and so have very little direct impact on PSC formation. To confirm this we have assessed PSC occurrence

frequency and lower stratospheric water vapour differences and find no significant differences between the two runs. We have added this point to the discussion of the temperature changes.

3. In my opinion it's a pity that the paper mainly focuses on the southern hemisphere. Only Fig. 2 shows total column ozone changes also for the northern hemisphere. The ozone changes in the northern hemisphere look quite interesting, and I miss a more detailed discussion about the underlying mechanisms. Maybe the authors want to submit a companion paper, but in that case I suggest removing the northern hemisphere from the present study.

We do not focus on changes in the NH for two reasons. Firstly, high variability in the Arctic vortex means we feel 20 year integrations are not sufficient to identify statistically significant changes in this region. Secondly, this version of the model has a positive water vapour bias which results in PSC occurrence being over estimated in the Arctic vortex. Therefore the effects of chlorine activation will likely be overestimated too. However, we feel the identification of increased total column ozone at 60N due to high latitude ozone depletion is an interesting finding, with possibly significant implications for ozone recovery and worthy of remaining in the paper.

4. The presentation of tropospheric changes and surface impacts in Sect. 5 is rather poor. The discussion is limited to the presentation of temperature and pressure changes, the underlying physical mechanisms as well as the link to stratospheric changes are not discussed. The simulation of changes in tropospheric climate is limited by the use of prescribed SSTs and sea ice. Thus, the model runs do not consider the full oceanic and sea ice response. I leave it to the authors whether they want to remove or extend this section. Of course it would be interesting to see results from model runs with a fully coupled ocean model, but I don't know if this is feasible. The current discussion in Sect. 5 is not very helpful.

The reviewer is correct in identifying the use of prescribed SSTs and sea ice as a limitation in studying the surface response, and this is why we are cautious when drawing conclusions about surface impacts. However, we include the section in spite of these caveats to demonstrate that we model a response similar to observations and previous modelling studies, adding to the robustness of those findings.

Abstract, p 18050, l 15-21: The last part of the abstract is a bit confusing. The causal link between zonal winds, F_z, wavebreaking, downwelling etc. is not quite clear. I recommend revision of this part.

The discussion of the link between F_z, wavebreaking and downwelling in the abstract has been clarified to prevent confusion

- p 18051, l 5: reference missing

References added

- p 18051, l 12: quantify "large increase in the total amount of chlorine"

We have clarified this point in the text, adding both the absolute and percentage change in stratospheric Cl_y between 1960 and 2000

- p 18052, l 4: "Arctic vortex"

Corrected

- p 18052, l 4/5: Furthermore, the Arctic vortex is often shifted towards Europe/Asia and not centred around the cold pole.

This point has been added to the text

- p 18053, l 13-24: What are the disadvantages of the applied method? Please discuss.

We have added test to the introduction critically assessing the strengths and weakness of the method we use, as well as those of previous methods.

- p 18054, l 26/27: I think this statement is a bit misleading: Due to the large temperature effect of the ozone depletion in the lower polar stratosphere, I would expect both model simulations also to differ in the PSC formation. See major comment above.

This is not the case, as detailed above

- p 18056, l 19/20: What is the reason for the increase in upper stratospheric ozone?

This is a response to changes in stratospheric dynamics, a point which has been added to the text

- p 18057, l 8: “of which would lead to”, remove “to” between “would” and “lead”

Corrected

- p 18057, l 10-12: Even though the observed composite difference ends at 30 hPa, is there any observational indication of downwelling of ozone enriched air masses from the upper stratosphere as seen in the model? How about other model studies? Same for the discussion of temperature changes, Fig. 3.

There is evidence from both MIPAS measurements and ERA-Interim reanalysis that increased downwelling leads to increased ozone mixing ratios in late winter between 30-10 hPa, consistent with this study. Braesicke et al. (2013) present the MIPAS measurements and compare them to two experiments (one of which is this study) undertaken with UM-UKCA. We have included the ERA-Interim ozone differences between average 1999-2001 and averaged 1979-1981 below in response to a comment below regarding the comparison of our modelled ozone difference and observations. ERA-Interim reanalysis data can also be used to show temperature changes above 30hPa consistent with those modelled here. Our modelled temperature changes above 30 hPa are also consistent with other modelling studies (e.g. Manzini et al., 2003; Perlwitz et al., 2008).

- p 18057, l 16: missing space: 15DU

Corrected

- p 18059, l 20: “on the same order” -> “of the same order”

Corrected

- p 18060, l 2: “: : : occur at the time when: : :”

Corrected

- p 18062, l 5-8: Is the decrease in wave breaking statistical significant? If not (looks like in Fig. 8), why is this change then discussed at all?

Although the decrease in wave breaking is not statistically significant at the 95% confidence level, we feel it is worth a mention in the text due to the good agreement with results published by McLandress et al., 2010 who state that “the springtime decrease in wave drag during the ozone hole period…to our knowledge, have not previously been noted” and speculate that it may be captured by other CCMs. We feel that our results provide support for their findings.

- p 18065, l 15: remove “(_1000 hPa)”; I think it’s clear where the surface is.

Corrected

- p 18066, l 1-2: What is the reason for the zonally asymmetric temperature changes at the surface?

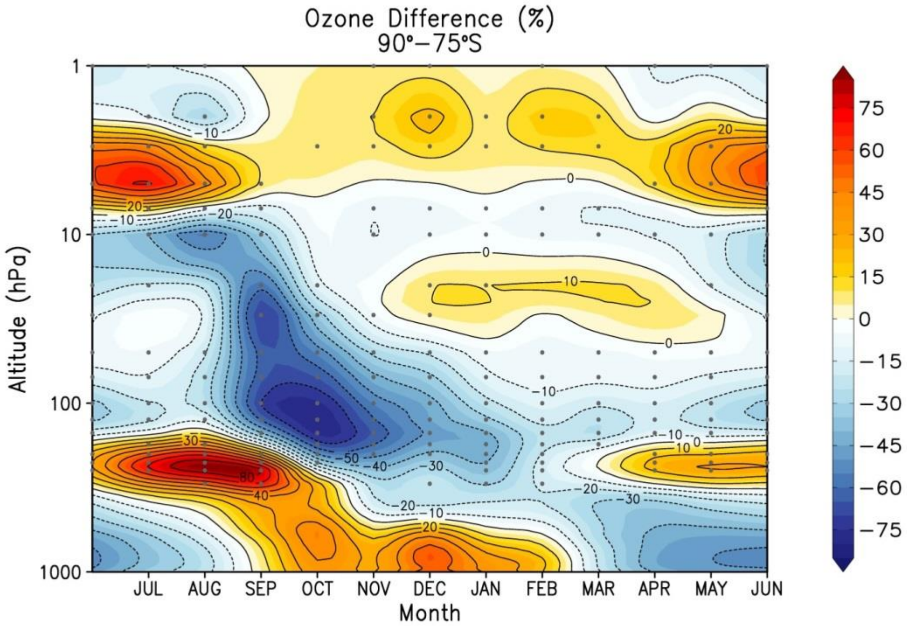
The drivers of the Antarctic Peninsula warming over the last few decades remain unclear, and we do not explore them here due to the limitations in assessing the surface response detailed above. However, we mention them in the text as 1) they are consistent with observations of surface temperature changes, and 2) an asymmetry in the temperature change is a necessity to drive the diagnosed increase in heat flux.

- p 18067, l 10: missing space: 15DU

Corrected

- Fig. 1: A direct comparison with observations would be nice, same for Fig. 3.

In the paper we do not compare the ozone difference directly with observations as we do not argue that suppressing heterogeneous reactions on PSCs is a reliable method for representing historic ozone depletion, but rather use it as a method to generate large polar lower stratosphere ozone depletion and then assess the climate response to this ozone forcing. Although the mechanism we use to generate ozone depletion in high latitudes is not a physical one, we highlight that the effect on stratospheric ozone is comparable to differences between the pre-ozone hole and ozone hole eras in the text. We have include a plot of ozone differences between average 1999-2001 and averaged 1979-1981 calculated using ERA-Interim reanalysis data, which shows good agreement with the modelled ozone differences presented in this paper. As in the paper, stippling denotes significance at the 95% confidence level. As with the ozone response, the temperature difference is in good agreement with ERA-Interim reanalysis data.



- Fig. 6: It would be helpful to highlight the 0 m/s contour in bold as in Fig. 4.

This has been added to the figure

- Fig. 7: There is no stippling. Are the shown changes not significant?

The method we originally used to calculate dynamical heating stored no variance information, so it was not possible to determine significance. We have altered the method so as to be able to calculate significance and have added it to the figure, and highlighted where/when dynamical heating changes are significant in the text

Anonymous Referee #2
General Comments

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.

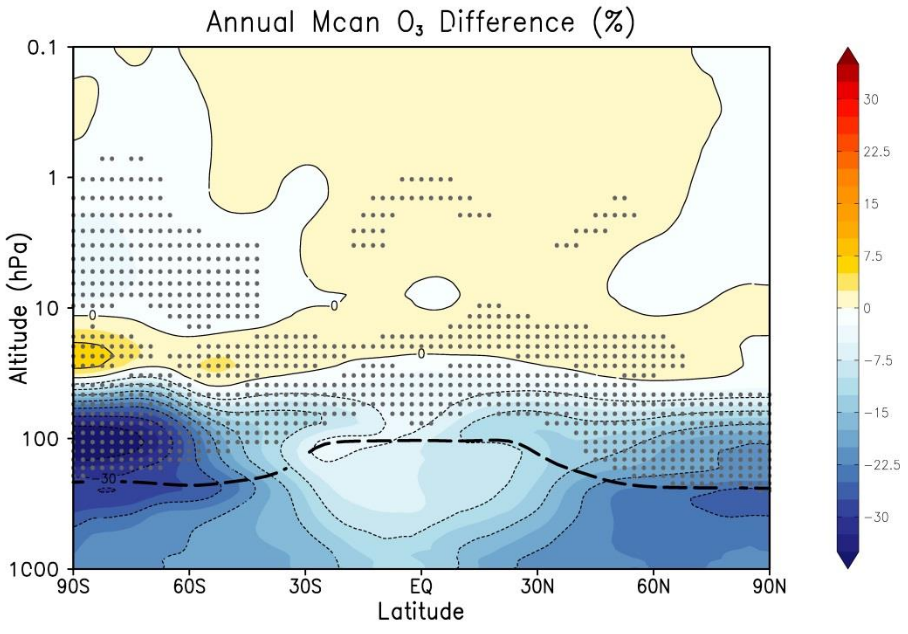
We thank the referee for their constructive comments. Our detailed response is given below

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?

We thank the reviewer for encouraging us to stress the value added by our study. As the reviewer has pointed out, the novelty lies in the setup of the integrations. That we can confirm many previous findings in a consistent way should not be seen as a negative result, but as an interesting result in itself underlining the robustness of some earlier findings. After illustrating the robustness of the fundamental results we provide new findings particularly in the analysis provided in section 4. We identify and characterise a link between zonal wind, F_z and w* at a given modelled altitude and identify a positive feedback in spring and a negative feedback in summer, supporting a mechanism suggested by Orr et al. 2012. Also, we suggest that changes to F_z entering the stratosphere from the troposphere may play a limited role in the resulting EP-flux divergence, building on results of McLandress et al., 2010. In response to Referee 1 we have added text critically assessing the strengths and weakness of the method we use, as well as those of previous methods.

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.

The main focus of the paper is to assess the climate response to polar lower stratospheric ozone loss, and so while we outline the method we use and its suitability for studying chemistry-climate interactions, as well as assessing the impact of this method on modelled lower stratospheric ozone mixing ratios, we do not focus on the mechanisms controlling ozone change beyond the polar vortex as we feel it is beyond the scope of the paper. Below we provide a plot of annual mean, zonal mean ozone changes between the two integrations. This figure highlights the large ozone changes in the SH polar lower stratosphere resulting from chlorine activation on PSCs, and provides evidence that changes to the BDC drive global changes to total column ozone. Increased tropical upwelling reduces ozone mixing ratios in the lower stratosphere. As the reviewer points out, dynamical changes should not influence regions where the chemical lifetime is shorter than transport timescales, and we find this to be the case, with ozone mixing ratios in the middle and upper stratosphere unchanged. The changes in the lower stratosphere dominate the column changes, resulting in decreased global column ozone. Braesicke et al., 2013, use long lived tracers from this experiment and a similar model set up to characterise the BDC response to high latitude ozone depletion, which is why the tropical response received little coverage in this paper. We make the point about a new climate equilibrium being reached as we do not believe that polar ozone depletion leads to simultaneous tropical changes, but rather changes beyond the polar vortex follow a succession of ozone depletion events. We have added a discussion on the impacts of the BDC changes on ozone beyond the polar vortex to the text, outlining the discussion presented here.



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?

We present the increase in total column ozone in the NH as the first piece of evidence that the chemical perturbation we have applied to NHC has led to a strong dynamical change. It would be more correct to say that we feel this is the result of a stronger Brewer Dobson circulation, and have amended the text to reflect this, and have removed the explicit link to NH ozone losses. We feel that the point raised by the referee is very interesting, and not one we had previously considered. Therefore we have also highlighted that the increased column ozone in the NH coincides with the maximum ozone loss in the SH, and possibly more importantly the time in which increased downwelling/reduced upwelling over Antarctic is statistically significant, and that the changes may be linked to SH ozone loss altering the BDC. However, we stress that it is difficult to determine a mechanism for this link, and further work on this is required. Again, it is likely that the northern hemisphere response results from the establishment of a new climate equilibrium, and may not necessarily follow on from that years ozone depletion

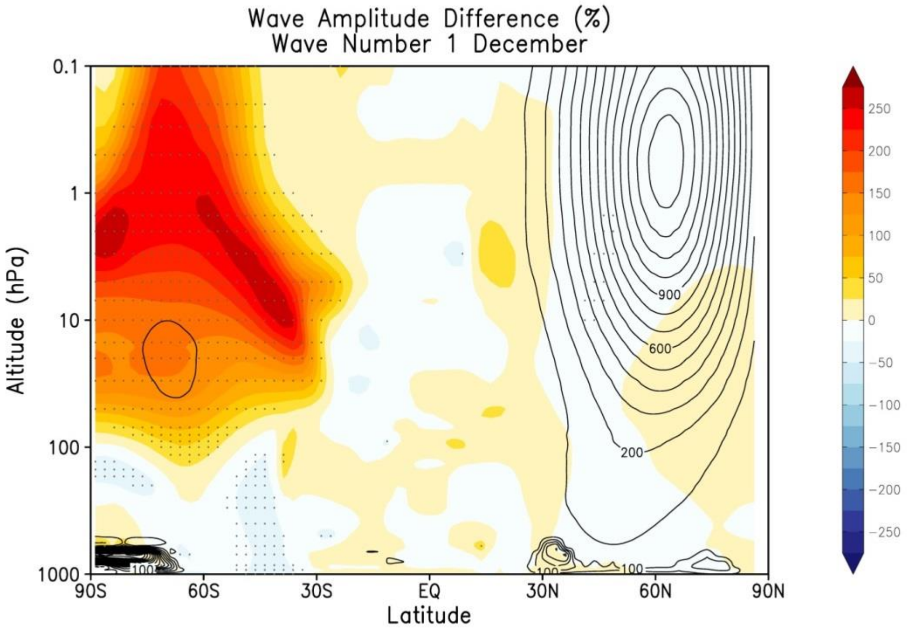
4. I find Section 4 not so needed in the manuscript, especially because it is difficult to see what is new here. The separation into shortwave cooling and dynamical heating was done e.g. by Keeley et al. (Geophys. Res. Lett., 34, L22812, 2007). The discussion of EP-flux changes was done in McLandress 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 EPflux convergence in spring is consistently balanced by a weakened residual circulation. Can this point be elaborated in the manuscript?

While the new method detailed in the manuscript is part of the novelty of the study, we feel that the analysis of the link between zonal wind, F_z and w* at a given modelled altitude detailed in section 4 is sufficiently different to previous analysis to be important. Specifically, the identification of how these are linked through a positive feedback in spring and a negative feedback in summer is the first support for the mechanism suggested by Orr et al. 2012 we are aware of. Also, we suggest that changes to Fz entering the stratosphere from the troposphere may play a limited role in the resulting EP-flux divergence, which we believe McLandress et al. (2010) do not. As we outline in an earlier response, we also feel that results presented in this study underline the robustness of previous studies.

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’.

Judging from the changes in the amplitudes of quasi-stationary waves (as mentioned in the original manuscript and now shown below), we felt that wave breaking would be an appropriate term. We understand the concerns of the reviewer and when using “wave breaking” to refer to EP-flux convergence. However, we would like to point out the large changes (in percentage terms) of the wave number 1 amplitude, centred around 70°S that seems indicative of enhanced wave breaking*. *While this does not mean that some component of the increased EP-flux convergence could not also result in changes to the wave transience or dissipation, it provides evidence for increased wave breaking, and so we feel confident using the terminology we have in the paper.



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
Both of these changes have been made