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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 #1

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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.

Main suggestions

- 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.

- 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.

- 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.

- 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.

Specific comments

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

- p 18051, l 5: reference missing

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

- p 18052, l 4: "Arctic vortex"

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

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

- p 18054, l 26/27: I think this statement is a bit misleading: Due to the large temper-

ature 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.

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

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

- 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.

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

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

- p 18060, l 2: ". . . occur at the time when. . ."

- 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?

- p 18065, l 15: remove "($\sim$1000 hPa)"; I think it's clear where the surface is.

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

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

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

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

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

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