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Interactive comment

Interactive comment on "Sensitivity of the southern hemisphere tropospheric jet response to Antarctic ozone depletion: prescribed versus interactive chemistry" *by* Sabine Haase et al.

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This is an interesting and timely paper attacking an important problem. The findings are novel and certainly merit publication. I do have a number of questions and comments that I hope the authors find helpful in revising their paper. I don't think these suggestions are necessary since the paper is already quite good, but I do think they may make it clearer and stronger.

Substantive comments 1) The paper does a very good job on probing stratospheric change, but doesn't cover the tropospheric linkages as clearly. WACCM's Antarctic sea ice retreat has long been an issue in this (see Arblaster et al. recent paper) so it may

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be necessary to say that it's not a good model to study the problem, but linkages still should show up better in the data presented for comparison, and I am puzzled by that. Much more could be done (for example, do you think sea ice and surface temperature trends should be further discussed?), but at least what is shown should be clear. I am surprised that in Figure 5, IGRA was chosen for the temperature comparison; there are now rather better databases out there including ERA5 and MERRA2. I am also very surprised that the temperature trend does not penetrate into the troposphere in January in Fig 5 and 7, can you please discuss/explain. Since there are linkages seen in zonal winds shown in Figures 8 and 9, I am very puzzled. Perhaps it's down to choice of latitude range? Not clear to me. Also, such linkages are often clearer when geopotential height is plotted rather than temperature, and that might be considered. I think the paper needs a clearer bottom line on whether feedbacks matter or do not matter for the tropospheric response, or whether this model's poor simulation of sea ice changes means that it's not suitable for such testing and that the troposphere is therefore not the focus here.

2) Total ozone is not a very sensitive diagnostic for evaluating a model's ability to simulate the coupling of interest here (Fig 4). Would it not be more useful to show some observations for comparison to Fig 4b instead. You could use the BDBP dataset or SWOOSH. It would be important to assess the vertical profile of the losses; much more so than the total column. Also, I would suggest that rather than showing the trends in ppmv per decade that you show the total trend over the period in percent. That is because what we really want to know is whether we see near complete depletion over the region from about 15-25 km at the peak, which gives us a good sense of the model's performance.

3) It is well known that there is a heating rate issue in the way that the SC version of WACCM handles mesospheric ozone; in particular, diurnal effects are not correctly accounted for and there are spurious rates of heating as soon as you get up above about 2 mbar, where atomic oxygen and ozone interchange from day into night and

drive a big diurnal variation that will give you an incorrect 24-hour average heating rate if you do not take account that SW heating is only present in daytime; this is all discussed in detail in Smith et al. Please comment on whether this may influence some of your results, particularly up near the 1 mbar level, and whether that has any potential to propagate downwards through a corresponding error in the residual circulation.

4) Lin et al. (J. Clim., 2009) showed evidence for a seasonal shift in the location of the polar vortex from July to November in the lower stratosphere. You have the perfect setup to test whether this occurs similarly irrespective of feedbacks and non-zonal forcing, and its relationship to BDC changes. It would be easy for you to reproduce their Figures 2-4, for your different cases; note the comparison to reanalysis shown in their Fig 8. It might help in understanding further what is going on in your Figure 11.

5) You emphasize the jet but it would be helpful to have more detail on the changes. It would be nice to make a simple scatter plot of the poleward shift of the jet (degrees) in the several simulations for various months, with the different simulations on the y axis and the observations on the x axis, or possibly using months on the x axis and plotting both data and models in the heart of the jet core on the y axis. We need to be able to see how many degrees the shift is in a simple way.

Minor comments

6) line 29 The paper's science is great but in several places the English is rather clumsy. Can this be checked over by a technical editor in one of the authors' institutions? Language like "The annually reoccurring depletion in polar stratospheric ozone was tremendous" distracts from an otherwise excellent paper. The annually reoccurring depletion in polar stratospheric ozone was striking or was of interest to scientists, the public, and policymakers alike, etc. would be better. 7) line 30 Political action was taken to ban the responsible substances (termed: ozone depleting substances, ODSs) under the Montreal Protocol in 1987 is incorrect. Political action was begun that ultimately led to a ban on the responsible substances (termed: ozone depleting substances, ODSs)

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under the Montreal Protocol in 1987. The original Protocol in 1987 did not mandate a ban, only a freeze on emission at then-current rates. 8) line 37 Need a reference to the first paper showing this by Shine (GRL, 1986). 9) line 103 Please clarify what the shortcoming of Rae et al. is; this is not very clear here. How does it work and why doesn't it capture heterogeneous loss?

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