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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Past and future conditions for polar stratospheric cloud formation simulated by the Canadian Middle Atmosphere Model" *by* P. Hitchcock et al.

Anonymous Referee #2

Received and published: 19 September 2008

The manuscript analyses the area and volume of PSCs derived from ensemble simulations of CMAM covering the period 1950 to 2100. The results show that in the Antarctic, ozone loss and climate change both contributed to the increase in PSCs for the past. In the future, ozone recovery is simulated to slow down the increase in antarctic PSCs. For the Arctic, the increase in strength of the Brewer-Dobson circulation exceeds the impact of the radiative cooling on the PSC amounts.

In general, I like the work very much. CMAM is clearly one of the better models in its ability to simulate PSC amounts and hence I believe that the conclusions are largely valid. Another positive point in the paper are the many discussions of the physical

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processes involved, including the impact of increases in atmospheric variability in the last few decades, and the counteracting effects of adiabatic and diabatic heating in determining PSC changes. On the negative side, I believe that almost all the figures are far too complicated with the result that each figure is used to convey too many messages and the key messages are unclear. I therefore recommend publication of the manuscript after the authors have responded to the following minor comments, and more substantial comments (below) on the figures. Revisions of the figures would of course necessitate text changes where the figures are described, but I haven't commented on that in the specific comments below.

General Comment

The impact of water vapour changes on the model diagnostics would be of interest, assuming that the distributions of the "stratospheric cold tails" are representative of actual PSCs. This could be added as a discussion point and need not include extensive calculations.

Specific comments

p.16556, l.1-6. This is really a summary from other work and has no real place in an abstract, which should be what this paper is about.

p.16558, l.23-25. The absence of NAT PSCs is regrettable, and presumably is being developed for the next version of CMAM. However, the chemistry on sulphate aerosols might be as effective in leading to ozone destruction as I believe has been recently argued by Drdla. It would round out the paper to add a discussion on this.

p.16558. l.27. Acronym needs to be defined: SPARC

p.16559, l.2-3. As we know, the statement in Eyring et al. suggesting that the

ACPD

8, S7287–S7292, 2008

solid lines were the "most reliable models" was not defended rigorously (despite one coauthor's request to do so, or remove the statement). It is therefore incorrect to propagate this inaccuracy.

p.16559. Acronyms need to be defined: CCCma, NCEP, NCAR

p.16560, l. 16. Acronym needs to be defined: ECMWF (Yes, I know it's tedious.)

p.16563, l. 17. Although fashionable, this technique of putting opposites in brackets — red (blue)...more (less) — is an abuse of the grammatical purpose of a bracket and should be rewritten in clearer form.

p.16565, l.4. It wasn't really clear to me why Newman et al. chose 40-80 latitude for the heat flux average, as the additional contribution to the pole is quite small. Either way, it is not really accurate to call this a mid-latitude average.

p. 16567, l.19-20. There can still be an ozone component to the downwelling, as the polar T has too much interannual variation to be quantitative. Other simulations (Li et al. J. Clim, 2008 and I believe unpublished results of the GEOS CCM) indicate that ozone has some role in driving the circulation.

p. 16568, l.9-11. Presumably the reason for choosing to highlight November and February is that these months have more chance of ozone depletion, because there is more sunlight than in december and January. If so, this should be stated. However, December or January could still be important, if the PSC region is far enough from the pole.

ACPD

8, S7287–S7292, 2008

p. 16568, l.23. Rex et al. I thought chose 5-year intervals. Please clarify also the starting and ending dates of the intervals used. The problem is that this is not a statistical measure, and the results may be dependent on the precise definition. An additional point should have been added for the model results of the last 5 years plotted. Also, the data have been extended well beyond 2001 and more recent data needs to be included. Even the WMO report has data to 2006. I would also like to see the actual gradient of the Vpsc trend line and the quantitative comparison with observations. If the argument is being made that this is relevant to the "coldest years" an alternative statistic would be better such as the trend in the 5-year variance.

p. 16569, l.8-10. The last sentence needs to be emphasised by comparison of observations of the last 8 years, and additional bold symbols added to Figure 10.

p.16569, l.18-20. I don't understand the second part of the sentence.

p.16570, l.15. This is rather old terminology. Can you remind us what Type Ia PSCs are? Denitrification is associated with water ice, but then NAT is also missing from the model.

p.16570, l.27. It is elitist to refer to "most reliable" CCMs without rigorous explanation. Several of the "unreliable" CCMs also produce a slight warming.

Figure 1: The information in the histograms is not used in the paper, so the figure could be simplified by presenting only the mean and variance.

ACPD

8, S7287–S7292, 2008

Figure 2: Arguably, the (clearest and) most informative figure in the paper!

Figure 3: (a) and (b) are fine, but I don't see the point of (c) in this form. I'm not convinced that the statistical analysis adds much information, and the main points can simply be left in the text. Figures 2 and 3 could then be merged and you would have a three panel figure showing the annual mean and contrasting with the differences for the chosen months.

Figure 4: This figure is far too complicated, as betrayed by the exceptionally long figure caption (a lot of which is in any case included in the text). It would be better to decide which factors are most important and then to redraw the figures to focus on just those points. It is difficult to see the dashed lines indicated in the text. The error bars are missing from some of the temperature values. Presumably this means that the error bar is smaller than the size of the symbol on the figure. This indicates that even in its current form, the symbol needs changing and the figure needs enlarging to communicate better the information contained.

Figure 5: This contains far too much data and some simplifying is essential. Of the heat flux-T relationship, there is no significant difference in the slopes as noted in the text. Either all the years could be put on the same graph, or the data could be put on to three separate panels for the given periods. A table showing the relevant statistics might be useful to avoid clutering the figure. If all the heat flux-T data are on the same graph, it doesn't help to include the distributions on the respective axes. These don't seem to be significantly different either, and the distributions add to the confusion, particularly on the T axis.

Figure 6: This has the same problems as Figure 4. There is an additional issue in

ACPD

8, S7287–S7292, 2008

Interactive Comment

that because the figure is so complicated the reader needs to refer back to the figure 4 caption, which will likely be on a different electronic page and hence difficult to read in conjunction.

Figure 7: This is fine!

Figure 8: Same comments as Figure 5.

Figure 9: This too is overcomplicated. In panel (c), I would like to see the statistical distributions removed, and curves fitted to the Vpsc values.

Figure 10: Not too bad!

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Interactive comment on Atmos. Chem. Phys. Discuss., 8, 16555, 2008.