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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.

P. Hitchcock et al.

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We thank the reviewer for their detailed and helpful comments, and for the time and effort invested. Our responses follow.

General Comments

A short comment on water vapour in these runs has been added to the Methods section.

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.*

The abstract has been shortened and the present work emphasized.

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.*

There are some pretty good reasons not to fudge the representation of NAT given the present state of understanding. These are now explained in some additional text.

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

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.)*

SPARC, CCCma, NCEP, NCAR, ECMWF and ERA40 are now defined, and the reanalysis project is now consistently referred to as the 'NCEP reanalysis' throughout, and references to Kalnay et al. 1996 and Uppala et al. 2005 have been added.

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

The problem we face is that for projections of ozone and temperature (Figs. 3, 5 and 7 of Eyring et al. 2007), there is a consensus among most of the models (including CMAM) but there are several notable outliers. In order to justify the use of CMAM for this study we need to be able to ignore those outliers. Eyring et al. (2006, 2007) explained away the outliers on the basis of their comparison with observations, especially chlorine loading (which of course is critical for ozone depletion and recovery). That is important for our purposes. However we have reworded our text to use the exact wording of Eyring et al. (2007) and attribute the judgement to them. Now our statement is simply a statement of fact.

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Interactive Comment

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.*

The construction has been changed.

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.*

We have dropped the 'mid-latitude' label for the heat-flux averages here, in section 5 when discussing the counterpart figure for the Northern Hemisphere, and in the associated figure captions.

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.*

The time series of polar temperature and residual vertical velocity is quite linear in these runs (see McLandress and Shepherd 2008, Fig. 4b), suggesting the dominant role of CO_2 in the circulation changes. This point is clarified in the text. The dominance of the CO_2 signal is likely a result of weak ozone depletion in the CMAM Arctic.

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.*

November and February are highlighted in order to draw a comparison with the Antarctic response (that is, a CO_2-induced signal in early winter and an ozone-induced signal in spring). The discussion of Fig. 9 has been changed to clarify this point. Note that

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Fig. 9c shows the evolution of V_{PSC} for the whole season, demonstrating that the only period in which there is a hint of an ozone signal above the variability is late-February. The value of Fig. 9c in its present form is further argued below.

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.*

We have corrected our analysis to use 5-year intervals; the conclusions remain unchanged. Our dilemma regarding showing recent data is that we would like to compare our results directly to Rex et al. 2006 but we do not have access to the ECMWF operational analyses. The ERA40 reanalysis was felt to be the closest substitute, but this ends in 2002.

Unfortunately five years are not enough to estimate a meaningful variance (even with three ensemble members) due to the strong variability and non-gaussian nature of V_{PSC}.

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.*

Bold symbols have been added to Fig. 10a indicating the coldest winters in each five year interval to clarify this sentence. Arctic polar vortices have been unusually disturbed over the past eight years, but they do not yet indicate that the trend in the five-year extremes has changed.

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

The sentence has been shortened.

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.*

We have clarified the text.

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.*

As for p.16559, l.2-3 we need to be able to justify ignoring the model simulations that disagree with ours. The models that show a cooling in the Arctic are all considered outliers by Eyring et al. (2007). So this is an important piece of evidence for our argument. However we have reworded the text to try to address the reviewer's concern.

Comments on Figures

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.*

Distributions of winter and spring monthly-mean temperatures are expected to be non-gaussian (Yoden et al. 2002). While it is strictly true that details of the histograms are not used directly, as the paper's central focus is the cold tails of the temperature distributions, we feel it is important to validate the whole shape of the distribution, not just the mean and variance.

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.*

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See our reply below the next comment.

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.*

Panel c in both figures serves two purposes. First, it shows the evolution of A_{PSC}/V_{PSC} as a function of season, emphasizing where the behaviour changes from CO_2 dominated to ozone-dominated. Secondly, it demonstrates that V_{PSC} in the Arctic is not only much more variable than in the Antarctic, but that the distributions are also severely non-gaussian. While the former is well appreciated and emphasized in the literature, the difficulties associated with the latter in applying standard statistical tests is not. The figures are now larger, so that the distributions in Fig. 9c are more easily discerned.

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.*

See our reply below the next comment.

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

We have increased the size of these figures and reformatted them to expand the vertical scale. The black contour lines are now wider and the contour interval is larger so that fewer contour lines are drawn. The faint coloured lines (which were artifacts

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of the plotting library used) have been removed. Additional labelling has been added, and error bars are now plotted for all points. The text has been updated to reflect the changes.

These figures relate changes in mean tempeartures to changes in the low-temperature extremes, similar to Fig 4-1 of the 2006 WMO Ozone Assessment.

Figure 5 (& 8): *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.*

The histograms have been dropped, and the symbols indicating the mean and confidence intervals for T and v'T' have been merged. Information regarding the statistical fits has been moved to tables following each figure. The error in the conditional mean has also been eliminated on all but one line in each figure to simplify the figure while retaining emphasis on the uncertainty associated with extrapolating to the intercept. Discussion in the text has been modified accordingly.

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