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Atmospheric Chemistry and Physics Discussions

Interactive comment on "An evaluation of the simulation of the edge of the Antarctic vortex by chemistry-climate models" *by* H. Struthers et al.

Anonymous Referee #2

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Observational analyses of O3 and meteorological quantities (PV and horizontal wind) are used to create diagnostics for the size of the Antarctic O3 hole (i.e., the 220 DU contour) and the meridional mixing barrier at the dynamical vortex edge (kappa). These diagnostics are applied to 5 chemistry-climate models (CCMs) to evaluate their ability to simulate 1) the latitude of the mixing barrier and 2) the location of the maximum column O3 gradient (ozone hole edge). Their results show that most of the models have a dynamical edge at about the right latitude but the width of the mixing barrier is too broad. The causes of the disagreement are not explored. The location of the maximum O3 column gradient in the models is examined and its location with respect to the dynamical barrier is plotted. Some models show the same relative positions as do the observations while some do not. The reason(s) for this could be interesting and might provide some insight into model deficiencies but it is not discussed.

There is not much new material here. While these diagnostics are potentially useful for evaluating CCMs, there is no process-oriented insight. The authors may say whether models agree or disagree with the observations but they do not say why and what it means to disagree. Statements are made about evidence shown in figures, but I frequently found that I did not draw the same conclusions from the figures. The phrase 'good agreement' is used too liberally.

This paper is sloppily written and this is detailed in the comments below. There are many statements that aren't quite correct. There are many conclusions that are drawn from evidence that doesn't quite support them; some conclusions are not new at all (already published) but had better supporting arguments in those publications. There are kernels of good work here (the diagnostics) but they are surrounded by weak or inaccurate statements and discussion. This paper needs substantial revision before it could be published. The comments given below show the specific areas of the paper that are problematic.

p. 20161, l. 8. The reference should be Bodeker et al 2002. Please check all the places you reference Bodeker 2005 because I think some of the others also should be 2002 (e.g., p. 20163, l. 3).

p. 20161, lines 8-11. Vincent and Tranchant 1999 is referenced for the statement that 550K is chosen because it is near the maximum ozone number density. (This statement is repeated on p. 20169, lines17-18.) This reference actually states '...we have mapped the TOMS data on the 520K [not 550K] isentropic surface (i.e., p˜50 mb) because it is near the maximum of ozone mixing ratio [not number density] and hence near the largest contribution of column ozone.'; This does not support your use of the 550K surface (although I suspect you can get away with using k on 550K because the vortex shape does not vary too greatly with height between 450-550K .) I have looked at assimilated Antarctic temperatures and for Sep/Oct I find that the 550 K surface occurs

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in the 25-30 hPa range which is well above the O3 number density max. Looking at realistic (well-evaluated) model results, I find that at 80S on Sep 1, before most loss has occurred, the max number density is near 60 hPa (440K or below). By the end of the month when major loss has occurred, number densities have sharply declined from 20-200 hPa. The max number density on Oct 1 occurs near 90 hPa (~400K). The majority of the O3 loss occurs below 550K; it seems that k on a lower surface would be more appropriate.

p. 20161, l. 22. These models have been 'evaluated' against measurements. 'Validated' is inappropriate because it implies the models have been demonstrated to be valid. This comment applied to p. 20174, lines 18 and 21.

p. 20163. Equivalent latitude mapping will be slightly different on each theta surface, so the equivalent latitude of a column quantity is not really well-defined. This should be pointed out.

p. 20163. It is well known that 2002 was a very anomalous year in the southern hemisphere. It is the year of the only observed sudden warming. It is misleading to include it in the 5-year averages. You cannot sensibly talk about trends when including this year.

p. 20164. It's interesting that observed k is increasing from 1980-2000. k is proportional to the PV gradient with latitude and the horizontal wind. Could you discuss which of these components is increasing? What might be causing the increase - radiation/ dynamics feed back due to decreased O3 inside the vortex?

p. 20164, lines 7-15. The Butchart 2006 paper referenced here is a model study and doesn't show an observed polar warming trend. Wouldn't one expect decreased vortex O3 to lead to less radiative heating in spring and hence reduced temperatures? Fig. 1c is 'demonstrating' this because 2002 is included in the 5-yr averages. That is an absurd basis for a trend! I doubt that this figure with 2002 removed would support this conclusion. And, you would first need to demonstrate a warming trend before you could begin to attempt to attribute the trend. Several papers by Ramaswamy (2001, 2006) which discuss lower stratospheric cooling trends.

p. 20164, lines 21-26. Newman et al 2004 show that once Cly reaches ˜1990 levels, the year to year variation in the size of the ozone hole (defined by the 220 DU contour) is driven by interannual temperature variations in the collar (near vortex edge) region.

p. 20165, lines 1-9. The conclusions drawn here are not new; they are from Newman et al 2004 and Bodeker et al 2002. These papers show that the size of the ozone hole is not correlated with the vortex size. Ozone hole size depends on Cly and temperature in the collar. Vortex size is dependent on wave forcing and varies interannually with dynamical forcing.

p. 20165, lines 13-15. Same point as above. Newman 2004 showed the O3 losses are determined by temperature once you've hit a certain EESC threshold (which was reached around 1990). You could state this more specifically rather than saying the losses are driven by 'dynamical variability and ...coupling'.

p. 20166, line 1-2. E39C and LMDZ are in 'good agreement' with reanalysis results only because they have been fitted to a gaussian (what's the justification for this?) which shifts their peaks closer to the reanalysis peak. The black lines (model means) in Figure 3 do not show good agreement with the reanalysis for E39, LMDZrepro, or umetrac.

p. 20166 lines 7-10. This short paragraph is basically repeated two paragraphs further down. I suggest omitting it here; it makes more sense on line 18.

p. 20166, lines 25-28. Why not generalize this statement to include umetrac and the E39 models? All produce a barrier that is too wide and too close to the pole.

p. 20167, lines 1-11. This paragraph is about WACCM results and does not fit here. The information is tangential. The reader is asked to 'note shapes' from figures not in this paper.

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p. 20168, line 21. Just because there is a large gradient in ozone doesn't mean this is a mixing region. Strahan and Douglass (2004) used HALOE CH4 in the vortex to show that transport across this edge is largely one way (vortex to midlatitudes) and occurs through erosion of the vortex in spring.

p. 20169, lines 12-16. Toward the end of Section 4, it was stated that the ozone hole is constrained by the size of the dynamical vortex. Newman 2004 clearly showed that once in the high EESC range (1990 and beyond), year-to-year variations in ozone hole size come from temperature variations near the edge of the dynamical vortex. I don't like that this paragraph refers to a correlation between k and the ozone gradient. There is a relationship between them, and to-date the ozone gradient is always found at least slightly poleward of k (i.e., the vortex edge) but because k and the gradient are controlled by different processes, I don't think they should be labeled 'correlated' just because they are nearly co-located. I also do not see that 'the correlation between k and the gradient in ozone is good in all [model] cases' in Figure 5. Fig. 5a (obs) shows that the k peak is at slightly lower latitude than the O3 gradient peak. This relationship is also seen in maecham and socol, but for e39c, lmdz, and umetrac the order of the peaks is reversed. Exploring why these models reverse the location of the peaks could be interesting.

p. 20169, lines 19-21. While O3 at 550K is an important part of the column, the O3 number density peaks much lower (˜400K) so I don't think it's accurate to say that the column is 'strongly weighted' to ozone at 550K.

p. 20170, lines 8-15. Since the existence of a strong tracer gradient does not constitute evidence of mixing, it seems ill-posed to suggest that k and the total column O3 might be used to quantify ozone transport across the vortex edge.

Section 5.3. These two paragraphs (and Figure 6) say only very general things that are already well known about vortex temperature, O3, CH4, and ClOx. It is well known that there is interplay between the chemistry, temperature, radiation, and dynamics that

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causes these species to show specific relationships. It is the details of these interactions that are interesting but what is said here is that the details of these relationships 'remain open questions'. This is saying nothing.

p. 20171, last paragraph. In Section 5.4 the 'inner vortex edge', defined as the 2nd derivative of k, is now assumed to represent the actual vortex barrier, that is, the latitude of 'containment' of the ozone hole. Why is the 2nd derivative rather than the peak of k now the preferred latitude for containment? Where does this idea come from and what supports it?

p. 20172. Figure 5 showed that some models have O3 biases. Knowing that, what is the point of constructing Figure 7a (which uses the models' 220 DU location as the ozone hole edge regardless of model bias) when it's already clear this will produce meaningless results? Skip this and go straight to correcting for bias, then show Figure 7b instead.

p. 20172, lines 25-26. It is clear from Fig. 7b that the observed relative ozone hole area is nearly constant from 1990 onward, but it is not clear from this figure that the same can be said for all the models. This figure has a jumble of lines so it is hard to see their slopes and when they flatten out. But if you look at their slopes in Fig. 7a (where they are better separated), you do not see a flattening out after 1990. In fact, the slopes from these models between 1980-2000 are fairly steady. This figure does not lead me to conclude that the models simulate a 'reasonable onset, development, and containment...'. All the models have increasing CFCs as part of their boundary condition and the increasing ozone hole size seen is simply a response to increasing Cl.

p. 20173, lines 5-7. What is the basis for this statement? This seems to come from nowhere. There is no model output to 2020 shown in this paper, and why is maechem being singled out here as a model to be used for predictions?

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 20155, 2008.