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Interactive comment on "2002–2003 Arctic ozone loss deduced from POAM III satellite observations and the SLIMCAT chemical transport model" *by* C. S. Singleton et al.

Anonymous Referee #1

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Review of "2002-2003 Arctic ozone loss deduced from POAM III satellite observations and the SLIMCAT chemical transport model," by C.S. Singleton et al.

This work yields estimates of ozone loss in the Arctic polar vortex during winter of 2002-2003. I think it is a very thoughtful piece of work and would be a useful addition to the literature. My comments are relatively minor, and mainly concern points of clarification. However, I would like to see each of them addressed.

Specific comments: p. 7015, line 6: I think the neglect of mixing of air parcels with their surroundings is probably just as important, if not a dominant, limitation of the Match

technique, as the accuracy of trajectory calculations. However, this is not the subject of the current paper.

p.7015, line 16: I disagree that the vortex average method necessarily assumes uniform descent in the polar vortex. It simply yields average estimates for the polar vortex.

p.7015, line 18: By “This” I presume you mean the mixing between vortex and ex-vortex air.

p.7018, line 28: Can you clarify your argument explaining why vortex and ex-vortex O3 are nearly identical at 500K? The “competing forces” explanation would appear to argue for how vortex O3 changes with time at 500K, rather than why vortex and ex-vortex values are “nearly identical.” [Since the vertical range of O3 (over theta) for vortex air lies within the vertical range of O3 for ex-vortex air, at some theta the two must coincide].

The paper could benefit from some zonal cross-sections of ozone from the POAM data (potential temperature vs. equivalent latitude) that would more clearly illustrate the ozone distribution inside and outside the polar vortex.

p.7020: ref the ozone initialization. Sounds like only the ozone fields are reinitialized, but no other fields, e.g. NOy, Cly. Could the authors comment on consequences of potential inconsistencies in the initial fields for the chemically active SLIMCAT simulations?

p. 7021, line 18: SLIMCAT is driven by analyzed winds, but the “active” ozone is described as coupled to the heating rate calculation. How do you guarantee mass continuity between the prescribed horizontal wind field, and the diabatic component which depends on the evolving O3 distribution? Can you demonstrate that the global average heating rate on a pressure surface is zero?

p. 7021, line 25 to p. 7022, line 18: This is a very useful discussion of the potential caveats in interpretation of the model/observation comparison. The inclusion of

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the “pseudo-passive” calculation certainly helps focus on the impact of heterogeneous processes alone. However, I think that assessing the net change in ozone (production - loss) is of equal interest.

p. 7022, lines 25-28. I don’t understand this argument about “reverse” chemistry. The differences are still negative above 600K in March, indicating net chemical loss, even if there is some impact here from unaccounted tropical O3 production (passive run).

p. 7024, line 28: At 450K the POAM data appears to show continued (if less dramatic) decline from end January through mid March (rather than constant). What about diabatic descent of air from above that has experienced heterogeneous loss?

p.7025, line 6 and Figure 9 (also Fig.8): A pedantic point perhaps, but the “losses” you show are presumably the negative values. At least for the left panel of Fig. 9, you are showing net production minus loss, right? Can you comment on the positive values in March above 650K in the middle panel? It seems to me that one possibility is that the residual circulation in the model may be too weak? See above question on mass continuity.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 7011, 2004.