

Interactive comment on “The 1985 southern hemisphere mid-latitude total column ozone anomaly” by G. E. Bodeker et al.

Anonymous Referee #2

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The authors describe many aspects of the low ozone values observed in 1985 over the southern hemisphere. They demonstrate that the residual circulation associated with the QBO, its particular phase relative to the annual cycle, and effects of the QBO associated wind-fields on wave-breaking can explain the observed low ozone values in 1985 and some other years. The topic is well within the scope of ACP, and the paper presents several interesting (although not entirely new) aspects. The presentation is generally clear, sometimes, however, too focused on the details. The presented evidence supports the conclusions of the paper qualitatively, but not always quantitatively. I think this is a good paper on a secondary effect and certainly deserves publication in ACP.

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1 Major comments

I think the paper would benefit greatly from a schematic diagram (along the lines of the famous diagram from Holton et al. Rev. Geophys., 1995) that shows, in a latitude altitude cross-section, the main features of the wind-field (e.g. by shading), the QBO associated residual circulation/ozone fluxes (e.g. as arrows), and the wave-mixing of ozone. The schematic would probably need two or three panels, that show the progress over the year, and clearly indicate why low ozone resulted in 1985. I think this would help a lot to understand the basic concepts. Currently, because of all the trees, it is difficult to see the forest. I strongly suggest the addition of such a schematic.

For completeness, it would also be good to have a plot that shows the vertical structure of the 1985 ozone anomaly (e.g. anomaly profile from ozone-sondes and E39C for November 1985).

My other point is, that it would be good to have some numbers (with justification) on the magnitudes of the effects. How many Dobson Units ozone reduction can be attributed to the different mechanisms? Does that add up to the observed anomaly?

2 Minor comments

page 7138, lines 3–5: The authors have never shown that state-of-the-art models cannot reproduce the 1985 anomaly (and this is not important either). I suggest deleting this sentence from the abstract.

page 7138, line 12: What is a “local” reduction by the solar cycle. Does the solar-cycle not induced large-scale variations? Maybe omit “local”.

page 7129 line 1: Are the CTMs from WMO 2003 still “state-of-the-art” in 2007?

page 7129 line 6: Do the authors mean the change of the residual circulation that is regularly associated with the QBO. If so, please make this clear, and give e.g. Baldwin et al., Rev. Geophys., 2001, as a reference.

page 7143 lines 16–28: I don't see the point of Fig. 3 and its discussion. There are many years with similar structure: 1990, 1993, 1995, 2000, 2002, What does Fig. 3 clarify? I would suggest to omit Fig. 3 and its discussion.

page 7144 lines 2–28: As mentioned by the authors, using the annual mean is problematic, because TOMS does not have observations around the pole in winter. An annual mean would also smear out the QBO related effects. Would it not be better to just plot the result for the month/ season where the maximum effect is observed? I suggest to change the plot and show the month/ season with the largest effect, presumably southern spring, September to November, where TOMS data should be available!

page 7144, Figure 5: The same applies to Fig. 5. Also, please justify at some point why 1984 is used as the reference, and not e.g. a longer-term mean.

page 7145, 7146: I was surprised that the PDFs in Figs. 6 and 7 show the largest effect in September and October, but that the principal component analysis/ Fig. 8 show the largest effect in November. Is there an explanation for that? I think the authors should at least comment on this.

page 7147: Are the NCEP/NCAR wind fields from the NCEP reanalysis, or the “normal” operational forecasts. Please specify. Also, please change the labeling in the left panels of Fig. 9. “Meas” should be changed to “NCEP”, or “NCEP/NCAR”. A data assimilation/ reanalysis is not a measurement!

page 7148: Fig. 9 is the only plot with height resolved information. I think it would be good to also have some information on the vertical structure of the 1985 anomaly, e.g. from ozone-sondes. Is there a relation between the ozone anomaly profile and the mixing profile (low mixing at 450 and 550 K in NCEP, low mixing at 550 and 650 K in

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E39C)?

page 7149: What is the take home message from Fig. 10? I would have expected low wave amplitudes, and thus low mixing. However, I don't really see that, except maybe for wave 2. Is there something in the summed amplitude of the waves (summing with or without accounting for the phase)? Maybe the summed amplitude should be added to the plot. Or is the reduced wave 2 amplitude the crucial point? Why would that relate most to mixing? Please make clearer what the message from Fig. 10 is.

page 7150/7151: This is where a conceptual picture would really help a lot. Also some quantification (in DU) of the effects is very desirable.

page 7152, lines 14–15: I would omit the last sentence. Any variation in the strength of the various effects, or in their relative phase could cause a difference, not just the long-term trend.

Overall, a nice paper, which can hopefully be made even better.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 7137, 2007.

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