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Atmospheric Chemistry and Physics Discussions

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

G. E. Bodeker et al.

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Response to reviewers comments on "The 1985 southern hemisphere mid-latitude total column ozone anomaly"

First, we would like to thank the reviewers for taking the time to review our paper. Responses to the comments are detailed below.

Reviewer 1

The authors provide an interesting analysis of the causes of the significant ozone reduction observed at southern mid latitudes during 1985, and to a lesser extent 1997. The paper contributes to our understanding of the ozone layer interannual variability mechanisms. The paper is well written and contains interesting analysis. However there is a need to brush up the statistical analyses and to decide whether or not to *include a discussion about 1997 and 2006 in the paper.*

We agree that inclusion of 1997 and 2006 is an issue. The analysis presented in the paper was initiated to better understand the causes of the 1985 downward step in southern mid-latitude total column ozone. We started this analysis early in 2006 before we had the 2006 measurements. We subsequently found that the mechanisms responsible for the 1985 event also occurred in 1997 and in 2006 and that annual mean total column ozone in these two years was also suppressed. So while the focus is on 1985, as reflected in the title of the paper, the findings are also relevant to 1997 and 2006. We would therefore like to retain the discussion of 1997 and 2006. We believe that showing that the same mechanisms responsible for the 1985 event also occurred in 1997 and 2006, and suppressed ozone then, increases our confidence that it was this mechanism that suppressed ozone in 1985. We have modified the manuscript to better reflect this intention and to clarify why we have included discussion of 1997 and 2006.

Clearly the paper aims to understand the 1985 event but there are rather random comments on the situation during these two years, in particular for the 2006, including some of the figures which can be confusing. In consequence the authors are encouraged to introduce the changes and comments to improve the discussion where needed, as suggested below, as well as to standardize the period sampled in all figures. After these changes are introduced the paper is worthy of publication in ACP.

Major Comments

There is a need to standardize the period being considered. Some plots include 2006 (figs. 2, 3 and 8), other only span up to 2005. This may appear as a minor issue but it can suggests, together with some other minor items to correct, that the paper was hastily completed.

It is certainly not the case that the paper was hastily completed. We included data for 2006 where they were available. In many cases the data for 2006 were simply

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not available at the time that we conducted the analyses and completed the paper. Perhaps it would have been better to be consistent and include data only to 2005 but when it became apparent that 2006 experienced the same anomalous mixing as 1985 and 1997, we decided that, where possible, we should include these data. We have reconsidered all figures in the paper and updated them through to the end of 2006 where possible, specifically.:

Figure 1: fully updated.

Figure 2: there are still insufficient data in the WOUDC to calculate a valid annual mean for Melbourne for 2006. The data for the other 3 locations run to 2006.

Figure 3: no update required.

Figure 4: completely restructured and updated.

Figure 5: completely restructured and updated.

Figure 6: no update required.

Figure 7: no update required.

Figure 8: completely restructured and updated.

Figure 9: no update required.

Figure 10: fully updated.

Figure 11: these data come from Garny et al. [2007] which provide analyses only to the end of 2005 and therefore we cannot update this plot to include 2006 data. However, this plot does not show a time series but rather correlations over the period 1979-2005. The addition of one more year of data (2006) is unlikely to affect these correlations.

Figure 12: This is a new figure that uses data from Randel and Wu. Their data set only goes to the end of 2005 and therefore we could only use data to the end of 2005. Again, this is unlikely to be an issue at all since Figure 12 shows deviations for 1984 and 1985 from a regression against EESC (described further below) and the addition of one more year of data to that regression analysis is unlikely to affect those deviations for 1984 and 1985.

The authors are well known for their excellent work and this ought to be brushed up.

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Done and we hope that this raises the paper back into the ‘excellent’ category.

The authors need to decide, within this context, what they want to do with the other two events mentioned in different sections of the paper, i.e., 1997 and 2006. 2006 does not have a full complement of observations since as the authors state only Lauder and Buenos Aires provide data for that year.

We can now add Perth to that list and with updates to the other figures (see above) we now feel that inclusion of 2006 is warranted.

Indeed as the paper stands now, it may appear that the mechanisms explained in the paper apply directly to these other two years. Is this so or is it just a perception of this reviewer?

Yes, the mechanisms explained in the paper apply directly to these other two years and we have made that much more clear in the paper.

Finally there is a need to improve the statistics of the paper. Assertions are made along the text but no information is provided regarding significance tests, etc. Specific comments can be found below.

We have addressed the statistical significance of the results throughout the paper as detailed below.

Specific Comments

Page 7141 The authors point out the fact that the product of the normalized F10.7 solar cycle index and the QBO index, introduced in Fig. 1 yields minima in 1985/1986, 1997 as well as for 1995 and 2001 (nothing can be said of 2006 since the result is not shown).

We now include data for 2006 which shows an absolute minimum for 2006 in the product of QBO and solar signals.

Why did the authors choose to use a product and not some other way to combine the

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indeces, say for example, a weighted sum of both?

Consider the use of the QBO and solar cycle signals in a regression analysis of total column ozone time series. The regression would include specific terms for the dependence of ozone on the QBO and solar cycle, e.g.:

$$O_3 = E \times \mathrm{QBO} + F \times \mathrm{Solar} + \ldots \tag{1}$$

Now consider some non-linear dependence of ozone on the QBO and solar cycle i.e. the E and F coefficients in the equation above depend themselves on the solar cycle and QBO respectively. This might happen because e.g. in the QBO westerly phase a solar cycle induced anomaly in ozone in the tropical upper stratosphere is more readily transported to lower altitudes where the ozone lifetime is longer. So, to make E depend on the solar cycle, and, similarly to make F depend on the QBO, we might expand these as:

$$E = E_0 + E_1 \times \mathrm{Solar} + \ldots \tag{2}$$

and

$$F = F_0 + F_1 \times \mathrm{QBO} + \ldots \tag{3}$$

Inserting these expansions into the original regression equation and regrouping leads to the term $(E_1 + F_1) \times \mathrm{QBO} \times \mathrm{Solar}$. This cross-product term results from 'cross talk' between the QBO and solar cycle drivers of ozone whereby the effect of the QBO on ozone depends on the solar cycle and/or the effect of the solar cycle on ozone depends on the phase of the QBO. We have added a sentence to the text to clarify this choice.

What is the physical rationale behind the chosen criterion for combining the solar cycle and QBO in this single new index?

There is no physical rationale behind using the product of solar and QBO signals and this is now clarified in the paper.

Page 7144 ..The authors show difference fields for observed ozone (fig 4) and geopotential height and temperature from NCEP/NCAR reanalysis products (fig. 5) as well

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as model outputs in both. Why did the authors choose to compare the 1985 fields with 1984 rather than with the mean field obtained from, say the TOMS Nimbus data set?

Good point and we have now totally revamped Figures 4 and 5 to address a number of concerns of this reviewer and the second reviewer.

If this choice has to do with the difference in phase of the QBO please point it out explicitly. Furthermore rather than using annual means why not show specific relevant months for the discussion?

We have now done that by showing the anomalies for 4 seasons, viz.: December to February, March to May, June to August, and September to November. Showing all months would make the individual plots too small. This seasonal disaggregation shows the build-up of the anomaly through the southern winter of 1985.

Another very important issue is the need to include in the plots the areas where the difference fields are statistically significant, for the TOMS observations, model outputs and reanalysis data products.

We have now done that for both Figure 4 and Figure 5. Regions where the differences are within 1 standard deviation of the variability over the period 1980 to 1999 (selected to be consistent between the observations and the model) are hatched to highlight only those regions where the differences are statistically significant.

The authors are requested to include this information in the plots and invited to include specific months rather than annual mean fields. If the latter is not feasible please justify.

We have done as requested.

Page 7145 Please be more explicit regarding the comment 'This indicates that the wave 1 pattern seen in Fig. 4 results from and underlying change in synoptic scale wave structure.'

We have changed the text to make this more explicit.

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Page 7145; .Why did the authors limit the baseline for the PDF study to the period 1980-1989. Would it not be reasonable to consider at least the period 1980-1993, i.e., TOMS Nimbus dataset to improve the statistics or even the complete TOMS V8 set?

We could have pushed the statistics for the PDFs to 1999 (no further than 1999 to be consistent between observations and model). However, including data through the 1990s, when there was large scale southern mid-latitude ozone depletion, would bias the statistics. To primarily highlight the dynamically induced differences in the PDFs, we decided to compare the PDFs against a 10 year climatology centred on 1985 which would then minimize biasing due to chemically induced ozone depletion over mid-latitudes.

Page 7146 Why is the PC analyses for November used rather than September or October, months which show the largest differences between the 1985 PDFs and the mean PDFs?

Good point and we have now repeated the EOF analyses on seasonally averaged data and show the EOF patterns and PC time series for the September to November mean to match the bottom panels in the new Figure 4.

The authors do argue that November is the month with the largest interhemispheric difference, which, as argued later in the discussion section, makes sense from the perspective of meridional transport modulation by the QBO. However this does not appear to be consistent with the PDF plots. Please elaborate or introduce the PC analyses for September or October.

We have now included September and October in the EOF analyses.

Also please discuss the explained variances of at least the first 3 EOFS, for the reanalysis and the model outputs and clearly justify why it is possible to compare, despite the very large explained variance differences, the reanalysis EOF 3 (explained variance 3%) and the model EOF 2 (explained 12%): the current justification is rather vague.

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We have now clarified this in the paper.

Page 7148 Regarding the Liapunov indices, which are much weaker for the model than for the reanalyses data products, the authors argue that the differences are due to the coarser model resolution. However no explanation is provided to justify why the 450K values have a very different annual evolution with respect to the reanalysis product estimates.

This is true and at present we have no explanation for why the 450 K values have a very different annual evolution compared to those obtained from the NCEP/NCAR reanalyses. We are in the process of investigating this further, in particular examining whether changing the advection scheme within E39C from semi-Lagrangian to fully Lagrangian may improve this comparison. A full discussion of these differences is beyond the scope of the current paper however.

Page 7149 The section on planetary waves focuses exclusively in the 1985 event. The behaviour during 1997 of planetary wave 2 appears to be the opposite however (fig. 10). In this analysis would it not make more sense to study the evolution of the ratios of the energies wave1/wave2 and wave2/wave3? Such an analysis would probably complement the current analysis and improve the insights derived there. Please compare the 1997 event with 1985.

We have expanded the analysis of the planetary wave amplitudes in the paper. We have also extended the analysis to include 2006.

Page 7151 Please provide significance levels for the correlations shown in fig. 11. The phrase 'the correlation is significant at most latitudes equatorward of 30oS' is rather vague.

Figure 11 has been modified to better show the significance levels for the correlations.

Discussion Section. An analysis of the height latitude evolution would have been a very valuable contribution to this interesting paper.

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We have now included this as an additional figure (Figure 12).

However there is a lack of data over the Southern Hemisphere due to the limited and sparse distribution of ozonesondes and ground-based profiling facilities.

True, but we were able to use the data base of Randel and Wu which combines satellite and ozonesonde data to provide the best coverage possible.

However satellite profiles, if available, would provide interesting insights during the 1997 event. It is for this reason that a more detailed comparison of 1997 with the 1985 event would be of great value throughout the paper, since it may then be possible to use the latitude-height observations during 1997 to carry out such a latitude height study and complement the current effort.

We have restricted Figure 12 to the 1985 event, the main focus of this paper. We could produce the same figure for the 1997 event but including both figures as new figures in the paper would make the paper unnecessarily long. We feel that the new Figure 12 addresses this comment.

Minor Comments

Note that the latitude of Buenos Aires given in Fig. 2 should 34.5oS

Thank you for pointing out this error. We have made the correction.

Please highlight in all relevant plots the years 1985 and 1997 (2006 if finally considered in all plots) since for example in fig. 10 it is not simple to determine what is happening with the wave amplitudes.

We have done as requested.

The authors are encouraged to refer to reanalysis information as data products rather than data since they are derived from model.

We have done as requested.

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Reviewer 2

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.

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.

We have attempted to include all of these mechanisms in a new figure (Figure 12) which provides a synthesis of the results and discussion presented in the paper.

The schematic would probably need two or three panels, that show the progress over the year,

We ended up including 6 panels in Figure 12, showing data every second month from December 1984 to October 1985, and the evolution of the ozone anomaly in latitude/altitude space over this period.

and clearly indicate why low ozone resulted in 1985. I think this would help a lot to understand the basic concepts.

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We agree and thank you for this suggestion.

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).

We have now included the vertical structure of the 1985 ozone anomaly in Figure 12.

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?

The only way we could envisage doing this would be through the application of a full regression model to the ozone observations. The issue then becomes which basis functions to include in the regression over an above the standard ones used (QBO, solar cycle, ENSO etc.) to capture some of these secondary effects such as the latitudinal movement of the region of planetary wave breaking induced mixing of ozone from the tropical source region out to mid-latitudes. These basis functions would also need to vary with altitude. This would be a significant piece of work requiring at least 6 months and would make the paper substantially longer. Rather, what we have done to address this comment is to produce a significantly improved version of Figure 4. Rather than just differencing 1984 and 1985, we have regressed seasonally averaged ozone (DJF, MAM, JJA, SON) time series in each grid box against equivalent effective stratospheric chlorine and considered the residuals from that regression. This effectively shows the magnitude of the 1985 anomaly from what would be expected from changes in halogen chemistry alone i.e. it now provides a quantitative measure of the anomaly produced by the combination of QBO and solar cycle. We have added some discussion to the paper on the contribution of the solar cycle to these anomalies (based on previous studies) and hence can derive a quantitative value for the anomaly induced by the QBO related processes i.e. direct modulation of the strength of the Brewer-Dobson circulation and the QBO influence on the location of wave breaking and hence on mixing.

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.

True. In an earlier version of the paper we had included a plot that was essentially the same as the bottom panel of Figure 4-33 from the 2002 WMO/UNEP ozone assessment. To reduce the number of figures we later removed that and simply referred to the assessment. We agree that making this statement in the abstract is not warranted and we have now changed that sentence to 'This event remains unexplained and a detailed investigation of the mechanisms responsible for the event has not previously been undertaken.'.

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".

We have made the requested change.

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

Yes and they are still unable to capture the 1985 downward step in mid-latitude total column ozone. Consider Figures 3-20 and 3-21, and especially Figure 3-25, from the 2006 assessment. We have made changes to our paper to refer to the model results from the 2006 assessment rather than from the 2002 assessment.

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.

Yes, that's what we mean and we have added the reference as requested.

page 7143 lines 16-28: I don't see the point of Fig. 3 and its discussion. There are

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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.

Figure 3 compliments and reinforces the conclusions drawn from Figure 8. We agree that there are other years that show a transfer of ozone from the southern to the northern hemisphere (as listed above) but the point of Figure 3 is to show that the three years which have anomalously low ozone (1985, 1997 and 2006) are a subset of the years in which there was a transfer of ozone between the northern and southern hemispheres. We have clarified the discussion around Figure 3 to make this point clear.

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!

We have changed Figure 4 to show results for all four seasons (DJF, MAM, JJA, SON) for both observations and E39C.

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.

Both Figure 4 and Figure 5 have been changed to address this comment.

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.

We have changed the PDF analyses to show results for the SON season to match the seasons shown in Figure 4.

page 7147: Are the NCEP/NCAR wind fields from the NCEP reanalysis, or the “normal”

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operational forecasts. Please specify.

They are the ‘normal’ NCEP/NCAR data and not from the operational forecasts. We have specified this in the text together with the Kalnay [1996] citation.

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!

Good point and we have made the necessary changes throughout.

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.

This has now been done in a new figure (Figure 12).

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 E39C)?

Vertically resolved ozone changes for E39C were not available and therefore this relationship could not be investigated. A more detailed analysis of the correlation between anomalies in mixing resolved by altitude, and anomalies in tropical and extra-tropical ozone (and their mutual connection to the QBO) is underway and will be published in a follow-up paper. Inclusion of this analysis is beyond the scope of the current paper.

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.

We have expanded the analysis and discussion of Figure 10.

page 7150/7151: This is where a conceptual picture would really help a lot. Also some

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quantification (in DU) of the effects is very desireable.

We have addressed both of these requests in the revised version of the paper. Figure 12 now provides a conceptual picture while the revised Figure 4 provides a quantification, in DU, of the effects.

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.

Good point and we have removed the last sentence as requested.

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

Thank you for the good suggestions on how to improve this paper.

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