

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

Anonymous Referee #1

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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. 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

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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 suggest, together with some other minor items to correct, that the paper was hastily completed. The authors are well known for their excellent work and this ought to be brushed up. 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. 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? 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.

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). Why did the authors choose to use a product and not some other way to combine the indices, say for example, a weighted sum of both? What is the physical rationale behind the chosen criterion for combining the solar cycle and QBO in this single new index?

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 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

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set? 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? 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. 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.

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 structureĚ.'

Page 7145ĚWhy 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?

Page 7146ĚWhy 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? The authors do argue that November is the month with the largest inter-hemispheric 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. 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.

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

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values have a very different annual evolution with respect to the reanalysis product estimates.

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.

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

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

Minor Comments

1. Note that the latitude of Buenos Aires given in Fig. 2 should 34.5°S. 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. 3. The authors are encouraged to refer to reanalysis information as data products rather than data since they are derived from mode

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