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Interactive comment on "Indicators of Antarctic ozone depletion" *by* G. E. Bodeker et al.

G. E. Bodeker et al.

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First we would like to thank the reviewers for taking the time to review our paper.

Response to Anonymous Referee #1

The reviewer states that 'The key metrics identified in this study will help to provide the opportunity for better evaluation of the performance of chemistry-climate models in simulating the Antarctic ozone hole. This would be a useful extension of the current study'. We agree with the reviewer and a paper on such an intercomparison will be written in collaboration with members if the international CCMVal activity.

The reviewer has requested the following technical corrections to the paper:

Page 3813, line 10: replace 'proneness', which is not an English word, with 'susceptibility': While my Longman Dictionary of the English Language does list the word 'prone-

ness', we acknowledge that this may be non-standard English and have therefore followed the reviewer's suggestion.

Page 3813, line 17: Antarctic ozone hole, not Antarctic hole: We have made this correction.

Page 3938, caption for Figure 8: the greyed area represents the maximum value during the interval 1979-99, not the mean. This is stated incorrectly in the caption: This correction has been made.

Response to Anonymous Referee #2

The reviewer states that 'Both parts are important and could be published as separate papers'. We agree that we could have split this into two separate papers but the first half, discussing the intercomparison of the different satellite and ground-based data sets, would have been rather boring as a stand alone paper. We felt it would make for a more interesting paper to primarily present the indicators of Antarctic ozone depletion, with a preamble showing how the underlying data base was constructed to give readers confidence in the results, and to show how the corrections made to the underlying satellite-based measurements might affect the results.

The reviewer states 'While the paper in its present form is acceptable for publication after minor revisions, more in-depth analysis in each of the two parts could make it even more interesting'. Yes certainly we could have presented more in-depth analysis of each section but this would have made the paper inordinately long. Our intention was rather to present a focussed paper with later follow-up papers e.g. including results from chemistry-climate models, presenting more in-depth analysis.

The reviewer states that 'A better understanding of these differences could allow in better correction of the data, although it is unlikely that better corrections would substantially change the main results of the paper'. A more detailed understanding and description of the sources of the differences between the different data sets used is

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presented in our earlier paper (Bodeker et al., 2001 a), and in a number of other papers to which we refer. It is beyond the scope of this paper to make corrections to the retrieval methods applied to the original raw satellite-based measurements to derive total column ozone amounts. Certainly we are aware of the solar zenith angle dependence in the differences in some of the satellite-based data sets and have crafted our correction functions to be able to account for this dependence e.g. by including multiple fourier components to capture the seasonal variability in the differences (see equation 1). The corrections we have derived *are* therefore based on a thorough understanding of the sources of the differences. We have not presented a detailed description of these sources here however since they are presented elsewhere (references cited), and would have made our paper excessively long.

The reviewer states 'It would be also interesting to see how the suggested indicators of Antarctic ozone depletion perform if the data from different sources are used without any corrections'. A detailed intercomparison of the assimilated data base with and without corrections applied was presented in an earlier paper (Bodeker et al., 2001 a) to which we refer. Therefore, in this paper, we only present the most relevant results on how corrections to the data affect the derived indicators (Figure 15 and associated discussion).

The reviewer states 'However, it is difficult to agree with the principal conclusion of the paper. The authors propose to use the ozone hole size in November, date of the hole disappearance, and the AVP mean ozone mass deficit for detecting the ozone recovery'. This is not the principal conclusion of our paper. In fact our paper goes to great lengths to point out that we are not attempting to detect the recovery of the Antarctic ozone hole. In particular we make no attempt at attribution of the changes observed in the indicators we have developed - a key requirement for detection of recovery. We agree completely with the reviewer that 'with these indicators, long-term changes in the dynamics that make the vortex less stable could be misinterpreted as ozone recovery'. In our paper we are careful to make no statements about attribution

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of changes observed in the indicators. Frequently in the paper we make the statement **whether or not this is a sign of statistically significant ozone recovery is beyond the scope of the paper**. The original conclusion section of our paper was:

> This analysis therefore suggests that indicators such as the AVP mean ozone mass deficit, the annual date of disappearance of ozone hole values, or the November means of the Antarctic ozone hole area may be more suitable indicators for detecting the recovery of the Antarctic ozone hole than e.g. the annual maximum area of the hole or the annual minimum ozone values over the Antarctic. Many of the indicators derived above show a change in behaviour in the past 4 or 5 years. Whether or not this change is indicative of a recovery in the Antarctic ozone hole is not yet clear and requires more detailed statistical investigation.

Therefore, whether or not the derived indicators are ultimately good indicators of Antarctic ozone hole recovery, relies on the additional step of attributing the observed changes in these indicators to changes in ozone depleting substances. This was not done in this paper. We have changed our conclusions section somewhat to better reflect this - specifically we have added the two sentences 'Whether or not they will ultimately be useful as indicators of Antarctic ozone hole recovery relies on the additional step of attributing the observed changes in the indicators to changes in ozone depleting substances. This is beyond the current scope of this paper.'

The reviewer states that 'As for recovery detection, it is easier to determine long-term changes in a data set with small year-to-year variations. From this point of view, the minimum ozone plot (Figure 14) looks more attractive'. We disagree. As pointed out by Weatherhead et al. (2000) the issue of statistical detection of ozone recovery, which then still requires attribution of the observed changes to changes in ozone depleting substances, is an issue of signal to noise ratio. Both the signal and the noise are important. The reviewer is correct in saying that the smaller the noise in the signal the

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better suited the indicator is likely to be to detection of recovery. However, if there is no signal e.g. if the indicator suffers from saturation, the indicator will be less suitable. This is why we did not judge the minimum Antarctic ozone levels to be a good likely indicator of recovery as they suffer from saturation.

The reviewer states that 'It would be interesting to see mean ozone hole size plots for September and October'. These plots were created as part of our analysis but because they did not show much change over recent years (as would be expected from Figure 8), they were not included in this paper. The paper already contains 15 figures and inclusion of two additional figures, which do not show anything of interest, nor support the focus of the paper, would have been wasteful and it is quite likely that reviewers would have suggested their removal.

The reviewer states that 'Linear interpolation of ozone values over the polar cap during the polar night in August - September may overestimate the ozone hole size during that period' and 'It would be interesting to compare the interpolation results with actual Dobson moon measurements and integrated ozonesonde profiles from the polar night area'. A thorough assessment of this approach, including comparing the interpolation results with integrated ozonesonde profiles, was presented in Bodeker et al. (2001 b). In that paper, the whole of section 2 entitled *Estimates of Ozone in Polar Darkness*, was dedicated to this. In this manuscript we refer explicitly to this paper in the sentence 'In this study the 'over the pole' linear interpolation method described and validated in Bodeker et al. (2001b)...'.

The reviewer states 'Instrumental errors are always present in the data. Some indicators are more sensitive to those errors and some are less sensitive. The authors may want to study how a small error would affect different indicators. For example, what would happen with the curves in Figure 9 if one adds or subtracts a small ozone amount (e.g., 5D U) to the actual data?'. Essentially this exactly what was done in Figure 15 where small (0-9 DU) additional corrections were made to the Earth Probe TOMS data since 1 January 2001 and the effects of these corrections on the AVP mean

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ozone mass deficit are shown. This does provide an indication of the sensitivity of this index to instrumental errors. We feel that from this result it is clear that indices that depend on the area of the ozone hole (ozone hole area itself and the AVP mean ozone mass deficit) will be more sensitive to instrumental errors than the minimum ozone value over Antarctic which will change linearly with any instrumental offset.

References

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Interactive comment on Atmos. Chem. Phys. Discuss., 5, 3811, 2005.