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Comment

Interactive comment on “Climatology and trends in the forcing of the stratospheric ozone transport” by E. Monier and B. C. Weare

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We appreciate the comments by the reviewer to which we respond point by point:

Major comment

The study is based on the ERA-40 reanalysis data set and uses dynamical quantities including the vertical velocity as well as the ozone data set of ERA-40. My main concern with the study is that it is known that there are large biases in particular in these quantities in ERA-40 (see e.g. Simmons et al., JGR, 2004 and DethofHolm, QJRMS, 2004). While the authors discuss some biases in the data description (Section 2.1), and correctly restrict the analyses to the satellite era, in the discussion of the results and the conclusions possible errors resulting from the data uncertainties are not mentioned. For example, how does the too

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weak ozone hole and the too strong BDC mentioned in Section 2.1 effect the results shown in the following? I suggest to state more clearly that while for the demonstration of the method it is valid to use these data, the results might be dependent on the biases and should be compared to results from other data sets.

The comments made by the reviewer about the potential impact of the bias in the dataset used in this study are definitely relevant and will be included in the revised article. We appreciate the suggestion to state that it is valid to use this dataset for the demonstration of the method, but that the results should be compared to other re-analysis products. In particular, we have addressed some of the uncertainty in the too strong BDC in a companion article that is currently published in the ACP discussion forum. We will make a reference to the article and discuss the uncertainty associated with the dataset.

Minor comments

Abstract, line 11: ‘.. is not directly. . ’. This sentence implies on first sight that the ozone hole is not DUE to chemical destruction, which is obviously not the case. I suggest to use ‘.. not solely. . ’

We will correct this sentence as suggested by the reviewer.

Abstract, line 17: The sentence starting with ‘This is primarily. . ’: As this sentence does not state a result of the study, but a possible explanation/interpretation of a result I recommend not putting it in the Abstract.

We will remove/rephrase that sentence.

Introduction: To highlight the need to separate transport and chemical effects on ozone trends in order to evaluate the influences of changing CFC amounts and the effects of climate change, I suggest discussing some of the new literature on the issue (see the WMO ozone assessment 2010 and references therein, e.g.

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Oman et al, JGR, 2010 or Eyring et al, ACP, 2010).

We will include a brief discussion and these citations in the introduction.

Page 3699, Line 8 and 9: Here and in the following: The term ‘production’ in the context of transport tendencies is misleading; ozone is not produced by transport but is re-distributed leading to a positive ozone tendency at a certain location. I suggest using ‘tendency’ instead.

We do not believe that the word ‘tendency’ is appropriate in this occurrence because it is used to describe the derivative of ozone with respect to time. Nonetheless, we will address the issue of the term ‘production’.

Section 3.1: The comparison of the chemical loss rates to other studies is essential to show the validity of the method. Here, only polar loss rates are compared though. Are there any estimates of tropical/mid-latitude net production that could be used?

This article mentions chemical production rates up to 30 ppbv day⁻¹ in the tropics centered at 30 hPa in DJF from the chemical transport model used in Miyazaki and Iwasaki (2005), which are similar to the rates found in this study.

Page 3703, Line 20: ‘In the tropics. . . In the inner tropics, the ozone tendency is positive and transport does not offset chemistry. I assume the authors refer to the subtropics (_30_), and should state so.

This will be corrected.

Page 3704, Section 3.3: Is the ‘ozone streamfunction’ basically the streamfunction of the first two ‘Mean transport’ terms in Equ. (1)? It could be helpful to state this.

The ‘ozone streamfunction associated with the Brewer-Dobson circulation’ is simply the ozone density weighted Brewer-Dobson streamfunction.

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Page 3705, section 4.1/ Fig. 5+6: At the beginning of the section and in the Figure captions it should be clarified again over which time period the trend is calculated.

This will be clarified.

Page 3706: Comparison of ozone trends: How do the trends derived in Randel&Wu, 2007, compare quantitatively to the results here?

It is difficult to directly compare the ozone trends presented in this study with that of Randel&Wu (2007) who show net ozone changes from 1979 to 2005 in integrated ozone column (in DU). However, we recalculated the trends based on integrated ozone column, from 1980 to 2001 (the extend of the dataset used in our study). The trends are overall similar, though they are more strongly confined to winter and spring in the SH and to spring in the NH than in Randel&Wu. Also, the maximum trends in our study are stronger than in Randel&Wu by about 10-20

Page 3706: Title of Section 4.2: rather ‘Wave forcing of ozone transport changes’ or something along those lines.

We will make the suggested modification.

Page 3706, line 28: ‘. . .largest trends occur from September to December in the chemical term. . .’

This sentence will be rewritten.

Page 3708, line 2: ‘not directly related’: see comment for Abstract

Same as for the abstract comment.

Page 3709, line 1: I don’t understand why the change in the chemical term is expected.

This sentence will be removed.

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Page 3709, line 2: True, the increased chemical ozone loss in December in the polar region is hard to understand as the polar night inhibits most chemical reactions. I would suspect that this is not a real trend, but an artefact of either the data set (see above) or the trend estimation over a rather short period for the dynamically variable NH. I suggest mentioning that this might likely be an artefact.

As suggested by the reviewer, we will mention that the trend in the chemical loss in December in the NH polar region may be an artifact, of either the dataset or the time period over which the trends are calculated.

Page 3712, line 14: ‘This study also shows. . .’. As stated above, the change in the ozone eddy transport only occurs because of the stronger ozone gradient, i.e. because of the ozone hole. So without an ozone hole, there would have been no change in the transport, and the change in transport is induced by the decrease in ozone. Furthermore, if no transport of ozone from lower latitudes would occur, the trend in the chemical tendency would most likely not be as large, as virtually all available ozone would be depleted. Therefore, this sentence read by its own might imply that an independent process masked the ozone decline, but this is not the case as the changes in chemical and transport tendencies are closely linked; Unluckily the cause-effect relationships are not easy to untangle. I suggest some more discussion on this issue.

We will provide a better discussion on the relationship between the trends in eddy transport and the ozone depletion, as suggested by this comment.

Typos/ technical corrections

All the typos are corrected. The authors appreciate the care with which this reviewer has reviewed the article.

Page 3696, Line 24: ‘a useful diagnostic..’ (without s)

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Page 3702, Line 20: missing blank after 'Fig. 1'

Page 3702, Line 22: missing 'hand' after 'On the other'

Page 3708, Line 16: This result . . . (without s)

Figs. 2 and 4: The top left panel in Fig. 4 should be identical with the 3rd panel in Fig. 2, correct? Even though in both Figures it is stated that the contour spacing is the same (10 ppbv/ day), the contours are different. Please clarify.

Yes, the reviewer is right. There is a mistake in the contour spacing, and the graph was corrected. Thank you.

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