

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

Anonymous Referee #2

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Review of ‘Climatology and trends in the forcing of the stratospheric ozone transport’ by E. Monier and B. C. Weare

This study analyses the stratospheric ozone budget and its contributions of transport and chemistry, in particular the transport by the mean residual circulation and by eddy fluxes. The climatology shows the importance of transport and in particular of eddy fluxes in the SH, and it is shown that the results are consistent with earlier work. The trend analyses over 22 years of data is primarily performed for the SH polar regions, where strongest trends occur over this period, and it is shown that the chemical decrease in ozone is in large parts balanced by increased eddy flux transport of ozone to high latitudes. This increased transport is in turn driven by the stronger gradient in ozone implied by the polar chemical ozone loss. The well-written paper undergoes the

C693

important task to separate transport effects on ozone trends from observational data and nicely presents a valid method with potential to be used for various data sets. My main concern with the paper is the data basis, the ERA-40 reanalysis data (see below). After addressing the concerns stated below I recommend the paper to be published in ACP.

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 Dethof&Holm, QJRM, 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 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.

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...’

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.

Introduction: To highlight the need to separate transport and chemical effects on ozone

C694

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

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.

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?

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 ($\sim 30^\circ$), and should state so.

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.

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.

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

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

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

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

C695

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

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.

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.

Typos/ technical corrections

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

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.

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C696