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Atmospheric Chemistry and Physics Discussions

Interactive comment on “The Brewer-Dobson circulation and total ozone from seasonal to decadal time scales” *by* M. Weber et al.

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1 Reply to Reviewer #1

The authors thank Reviewer #1 for his comments.

Reviewer #1 (Sandip Dhomse): 1. For me stratospheric circulation is a combination of two parts. a) A residual circulation in the middle upper stratosphere on longer time scale. b) Mid-lower stratospheric transport due to eddies on shorter time scale. I think this transport is more relevant for this study. As shown in Weber et al, 2003 and present study (Fig. 4), increase in mid-high latitude ozone is quite fast in Sep 2002 (with in few days after wave breaking event). And as authors correctly point out (Page 8 –> line

8 and Page 14 line 16), lower stratospheric ozone is the main contributor for the total ozone column. I feel authors should include this discussion in the introduction.

We agree with Dr. Dhomse’s statement. There are both fast and slow effects from the wave driving. Adiabatic compression (expansion) in the polar (tropical) region as a result of an impulse from waves is fast and leads to an immediate temperature response (warming in the polar region, e.g. SSW, and cooling in the tropics). This adiabatic compression is responsible for horizontal mixing/stirring leading to fast column ozone changes. The relaxation back to equilibrium temperature is then the slow response of the atmosphere to these bursts and this is normally associated with the residual circulation, also called the diabatic circulation that determines the actual transport time scales. The accumulating effect from these wave driven bursts leads to the cumulative built-up of ozone in the polar region as shown in Fig. 4 of our paper. We believe this is sufficiently explained in the Introduction and beginning of Section 3.

Reviewer #1 (Sandip Dhomse): 2. Page 11 –> line 11- 14, "The turnaround latitudes" –> Again I think it is more about continuity equation in the eddy transport (more or less balance between transport from the tropics to the high latitudes).

Continuity equation or mass balance requires both an ascending (tropics) and descending branch (high latitude region). These turn around latitude roughly separates both regions.

All minor comments are changed as recommended.

2 Reply to Reviewer #2

The authors thank Reviewer #2 for his positive review and constructive criticism.

Reviewer #2: The study is based on two main variables, total ozone and eddy heat flux, taken from various satellite and re-analyses products respectively, but it uses them se-

lectively (and in different periods) in the figures. This needs to be justified (or modified, if possible):

a) For the inter-annual 1980-2009 time-series in figures 2 and 3 the MOD, GSG and OMI datasets are used for the March (N.H.) and October (S.H.) ozone and the ERA-40, ERA-Interim and NCEP re-analyses for EP-flux. In figure 4 the linear relationship between ozone and BDC strength is populated with the years from 1996 to 2010 using only the GSG ozone and the ERA-Interim heat flux. I understand that 1996-2010 is the period of the GSG ozone data availability which the main author has excellently validated and exploited in the past (Weber et al., 2003;2005) and it starts late enough to include the large N.H. ozone losses. I am wondering if it would be worth including in this figure data points prior to 1996 by using MOD ozone and ERA-40 heat flux. The inconsistency of the suggested dataset blend should be small (as figures 2 and 3 already reveal) and it would be more than compensated by the confirmation of the compact link of ozone vs heat flux for years with smaller halogen loading and effect on ozone, especially in the Northern Hemisphere. It would be very interesting to see how this relation holds then. Ideally I would like to see a new figure 4 (and then figures 9 and 10 could be easily modified too with more observational points) or if not possible at least discussed and justified the reason it's not done.

One major reason why only the late period after 1995 (coinciding with the GSG data set) is shown in most figures is that the linear eddy heat flux - ozone relationship works remarkably well in this recent period. The same is true when using the MOD V8 data set. It is also evident in Figs. 2 and 3 that the correlation is somewhat weaker before the 1990s. There is of course the possibility that ozone measurements were less mature then, but more likely the issue are the reanalysis data. In the 1980s satellite data started to be assimilated into the met analyses. The integration of satellite data posed some problems in the beginning, however, improved with time (Simmmons, personal communication). Showing MOD V8 data along with the GSG data set would make Figure 4 (and other figures) very crowded. Nevertheless with respect to Point 2 raised

by reviewer 2, we added a new Figure 12 (similar to Fig. 4) that show the decadal evolution of this linear relationship from the 1980s to 2000s based upon the MOD V8 data (see discussion further down).

Since the MOD V8 data has been updated to end of 2010, we have removed the OMI data from the figures. Figures 4, 5, 8, and 9 have been updated since we accidentially omitted the year 1995/1996 for which data from the GSG data set were available.

Reviewer #2: b) Similar considerations hold for figure 11 with the EP-flux centennial time-series. The use of NCEP only, needs to be justified. Also the choice of the periods for the regression "trend" lines should be clarified, what is the context that the 1980-2010 period serves (and the comparison with the 1960-2050 model ones)? A blend of ERA- 40, Era-Interim and NCEP can cover a much longer period (1960-2010, = 50 years) already looked at in figures 9 and 10 (with the 1960-1985 vs the other two periods). Then, a 50-year regression line could be also fit in the model output to test the CCM simulations. The model runs include all known external forcings and, ideally, should replicate the past observed long-term trend in the BDC strength. Trend estimations are very sensitive to the number and choice of years. If the authors would like to to keep the 1980-2010 period for the observed heat flux (then they should argue about it and the use of a particular re-analysis must be justified) they should also do the same for the modeled ones (perhaps with two periods, one to coincide with any past/present period until 2010 from the observations and one thereafter for the future from now).

Showing long-term trends in the winter eddy heat flux (ERA-40, NCEP) can be done but interpretation of the same is difficult. Before the satellite era (before 1978) the calculation are based on more sparse radiosonde data (in particular the SH) and the eddy heat flux shows for this reason a much smoother behavior and less variability than later. The important point to be made in Fig. 11 is that the apparent decadal trend seen in the reanalysis during the satellite era (on which this study focuses) may not be maintained in coming decades but are part of the decadal variability as suggested by the CCMs. Nevertheless we added ERA-40 (1980s) and ERA Interim data (1990s, 2000s) to the figure.

Reviewer #2: 2. I would disagree with the two statements in: Section 6: "... it is clear that the 30-yr satellite data record is still comparatively short for a complete disentangling of long-term changes in ozone columns due to atmospheric dynamics and halogen changes" and Conclusion: "When attributing processes to the current total ozone trends, it is difficult to clearly separate chemical and dynamical contributions (Kiesewetter et al., 2010b), which is understandable from the fact that both processes are closely coupled as shown in this study" The recent study "Salby, M., E. Titova, and L. Deschamps (2011), Rebound of Antarctic ozone, Geophys. Res. Lett., 38, L09702, doi:10.1029/2011GL047266", using the BDC concept, has simply and neatly separated the contribution of dynamics and quantified that of halogens for the Antarctic ozone depletion and recovery. The above two statements must take into account this Salby et al. (2011) work. In addition, regarding the shortness of the 30-year period, there is also the 45-year long WOUDC dataset (already used in figure 1 for the N.H. mid-latitude ozone time-series).

We added a new Figure 12 showing the evolution of the winter eddy heat flux - ozone relationship from the 1980s to the 2000s using the MOD V8 merged data set. This figure clearly reveals a larger change from the 1980s to the 1990s (strong positive trend in halogens) and virtually no changes from the 1990s to 2000s (during which the halogen load reached maximum and started slowly to decline). With regard to this we removed our statement and we state that despite the large inter-annual variability in ozone, the compact liner relationship as shown in Fig. 12 indicates the onset of recovery. The new figure also shows that the Pinatubo years showed much lower ozone ratios (for a given winter eddy heat flux) indicating enhanced aerosol related ozone loss. We added references to Salby et al. (2011) and Maeder et al. (2010) as first studies showing evidences for the onset of (anthropogenic) ozone recovery.

Reviewer #2: 3. Section 5, page 13839, line 28 onwards: "A possible explanation is the

breakup of the polar vortex and the removal of the polar transport barrier that mixes or dilutes polar air into middle latitudes and to the subtropics (e.g., Knudsen and Grooß, 2000; Ajtic et al., 2004; Fioletov and Shepherd, 2005)". The possible cause should be the one mentioned here although it should be true nearer the middle latitudes rather than the subtropics; all the cited work does not look of this effect at latitudes lower than 30 degrees. Another relevant reference here is the "Hadjinicolaou, P., Pyle, J.A., 2004, The impact of arctic ozone depletion on northern middle latitudes: Interannual variability and dynamical control. Journal of Atmospheric Chemistry 47(1), 25-43 " which explicitly connects polar dilution to EP-flux (their figure 8) and demonstrates the effect of dilution on the subtropics, with ozone depletion reaching beyond 30 degrees, especially in the S.H. (their figure 1).

We added a reference to Hadjinicolaou and Pyle (2004).

Reviewer #2: 7. Page 13841: overall the EMAC-FUB captures better the seasonal variation of the BDC strength than DLR-E39C-A, which implies that, given that both models have similar physics/climate core (ECHAM 5 and 4), the higher top (0.01 hPa) of EMAC-FUB might contribute to this improved representation compared to the lower top (10hPa) of DLR-E39C-A. Is this a useful comment to add?

This could possibly explain but is too speculative here. The differences between models (see SPARC-CCMVal, 2010) is quite substantial even among models with a high top, so there is no clear evidence for holding up such a statement.

Reviewer #2: 9. Page 13842, line 5: Clarify what is meant by "under identical meteorological/ dynamical conditions" during 1985-2010, figures 2 and 3 show that dynamical conditions, represented by the eddy heat flux, vary substantially in this period.

It simply means that by looking at Figs. 9 and 10 that for similar winter eddy heat fluxes, ozone ratios were in the past (before 1985) and will be higher (after 2010) in the models consistent with changes in the halogen load.

Reviewer #2: 11. Page 13842, line 18: Regarding "The largest contribution to the strong increase in NH total ozone in the extratropics since the middle 1990s is due to changes in the stratospheric circulation (Dhomse et al., 2006; Wohltmann et al., 2007; Harris et al., 2008)". The most relevant work to this statement, which chronologically precedes the above studies, is the "Hadjinicolaou, P., J. A. Pyle, and N. R. P. Harris (2005), The recent turnaround in stratospheric ozone over northern middle latitudes: A dynamical modeling perspective, Geophys. Res. Lett., 32, L12821, doi:10.1029/2005GL022476".

The references listed here were studies based upon observations alone, while Hadjinicolaou et al. (2005) was a model study. All these papers were part of the EU Candidoz project (Harris et al., 2008). Nevertheless, this reference will be added as suggested.

All other suggestions for minor changes were made.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 13829, 2011.

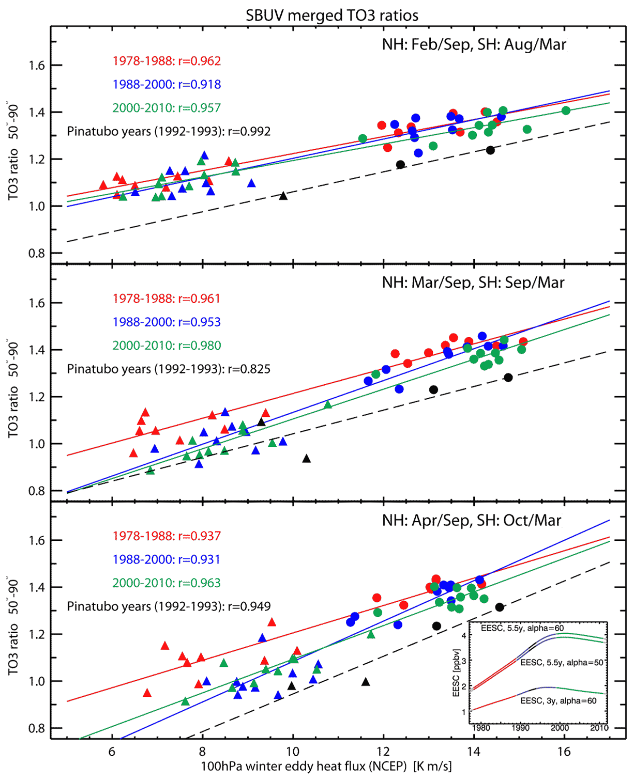

Fig. 1. new Figure 12 (see revised manuscript for a more detailed caption)