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Comment

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

Anonymous Referee #2

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This paper investigates the past and future control of the winter Brewer-Dobson circulation (BDC) on the seasonal and decadal ozone (TO₃) changes in the stratosphere in both hemispheres with a combination of satellite observations and output from two chemistry climate models (CCM). The authors update previous work which established a compact linear relationship between the extra-tropical eddy heat flux (a measure of the vertical propagation of planetary waves from the troposphere) and the winter build-up of extra-tropical column ozone (represented by the ratio of the spring-to-fall total ozone at latitudes greater than 50 degrees). This link is an important manifestation of the coupling between stratospheric chemistry and dynamics, where the strength of the BDC during winter regulates both temperatures that affect heterogeneous ozone depletion and ozone transport. The current work includes measurements of recent years which, taken all together for Northern and Southern hemisphere cases, reveal an im-

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pressive consistency in this relationship. The authors also look at the seasonality of the correlation between the ozone build-up in various latitude bands and the extra-tropical EP-flux change which points to a transition zone at 40-50 degrees for the ozone variation shaped by the tropical upwelling and poleward downwelling. Finally this study shows the ability of two chemistry climate models to reproduce the above connection and how it is projected by mid-21st century under increased green-house gas (GHG) emissions and expected halogen loading. An upward shift in the heat flux vs ozone build-up regression line is foreseen by both models, indicating an ozone recovery towards 2050. Interestingly, the simulations show an overall positive tendency in the heat flux (also observed in the last 2 decades), implying higher ozone abundances, although the modelled inter-annual variability of the BDC is large, suggesting periods of severe polar ozone losses.

This knowledge of the EP-flux and stratospheric ozone link is not new as it is being studied for the last decade, chiefly through the ground-breaking work of Salby and co-workers (cited in the manuscript). The current study complements nicely the existing knowledge by updating and invigorating this linear connection and revealing aspects of the chemical dynamical coupling and its possible future pathway. The manuscript is clearly written, the analysis is solid and the presentation satisfactory. It deserves publication to ACP given that the following specific comments are dealt with.

Specific Comments

1. 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 selectively (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

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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.

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 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 modelled 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).

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

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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 (Kieseewetter 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).

3. Section 5, page 13839, line 28 onwards: “A possible explanation is the break-up 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).

Other minor/technical comments

4. page 13831, lines 4-5: could remove comma after “meteorology” and insert comma

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after “recently” and “changes”.

5. Page 13835, line 24: insert “realistic” before “aerosols”?

6. Page 13835, line 25: are prescribed SST’s in the 20th century realistic?

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?

8. Page 13842, line 2: replace “coincides” with “coincide”

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.

10. Page 13842, line 9: the clarification “,back to the pre-ozone hole period 1960-1985,” can be added after “curves”

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”.

12. Page 13843, line 1: “... have not been seen in observations during the satellite era (since 1978)”. This is not deduced from figures 9 and 10 which include only observed data since 1996.

13. Page 13844, line 17: insert “in 2002” after “SH ozone hole”.

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14. Page 13845, line 8: remove “latitudes” after “35 degrees”.

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