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Atmospheric Chemistry and Physics Discussions

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

E. Monier and B. C. Weare

emonier@mit.edu

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We appreciate the comments by the referee 1. We respond point by point to the comments.

General comments:

1. Separation between stationary and transient eddies. In view of the results presented here, I find this effort particularly uninteresting as it does not clarify anything, or raises new points for further investigation. Moreover, as an additional point for the authors, the discussion of the transient eddies should include also the mean transport of the time varying ozone due to the transient circulation.

The decomposition in stationary and transient components demonstrates that calculating the various flux terms using monthly data (i.e. considering only stationary processes) does not provide the full picture since the contribution from transient processes is considerable. We will emphasize this finding in the article. Furthermore, the fact that the structure of the climatology and trends of both transient and stationary terms can be reasonably explained (presence of storm tracks and presence of topography and sea/ice heating contrasts) provides further confidence in the analysis. This will be mentioned more clearly in the revised manuscript.

2. Section 3. This Section deals with the mean climatological behavior. It should be emphasized that single year analysis can produce substantially different behavior, as the analyzed time period spans 20 years.

Absolutely, we will mention it in the updated section.

3. In the Conclusions it is stated that "a weaker intensification of ozone chemical destruction occurs in August". As explained below in the detailed comments, I don't believe this statement to be correct. On the other hand, I think the authors are complicating the issue unnecessarily. The intensification of the ozone hole IS primarily due to chemistry. Dynamics intervenes to mitigate those effects like in 2002. Is the goal of this study to discuss a trend of the eddy (or mean) transport between 1980 and 2000?

Indeed, the intensification of the ozone hole is primarily due to chemistry. The aim of this study is to examine the role of the dynamical transport of ozone in the stratospheric ozone budget, and as such the trends in the eddy and mean transport, and their contributions to the long-term changes in ozone. We will be careful to mention the complex interaction between dynamics and ozone hole: the ozone transport impacts on how the ozone hole develops and dynamical changes are also at least partly due to ozone depletion. One finding of this article is that large trends (statistically significant) in the eddy ozone transport largely balance the intensification of the chemical destruction of ozone, from October to December.

ACPD

11, C2787–C2794, 2011

Interactive Comment

4. Throughout the manuscript there is some confusion (at least on the part of this reader, for which I must apologize) between the trend of the field (ozone) and the trend of the tendency of the field which is a higher order and intrinsically less reliable measure. In the detailed comments below, I make some suggestions on how to improve the statistics, but a point needs to be clarified: the conclusions presented here on the trend of the tendencies are compounded with - and may be contaminated by – the trend of the chemical tendency, particularly in the last years as the ozone hole recovers. Since there are two trends involved here (the planetary eddies and the ozone) the conclusions seem to this reader a little confusing.

It is true that there are several trends involved (chemistry, ozone transport by the Brewer-Dobson circulation and eddy ozone transport) that combine to drive long-term changes in ozone. Furthermore, these processes are not independent of each other (the chemical destruction impacts the meridional distribution of ozone, which in turn impacts the eddy transport; or the planetary wave activity drives the Brewer-Dobson circulation and is also the source of eddies that contribute to the eddy transport of ozone). However, the authors want to make clear that any trends in the ozone tendency cannot be 'contaminated' by the trends in the chemical destruction since the latter is calculated as a residual from the other terms of Eq. 1. We will make sure that the notation concerning tendencies and trends are perfectly clear and consistently used.

5. In terms of benefits that this study seems to bring is that care must be taken in modeling studies of future ozone recovery if the planetary eddies (and thus the eddy transport) are not simulated realistically leading to the well known cold biases which can produce false statements about ozone recovery. This seems to me the most important message and should be emphasized, de-emphasizing the somewhat obscure (to me) statements on the trends of the tendencies.

As suggested by the reviewer, we will emphasize the importance of realistically sim-

ACPD

11, C2787–C2794, 2011

ulating planetary eddies (and thus the eddy transport) in modeling studies of future ozone recovery. This is indeed a very important result of this study.

Detailed comments:

1. Figure 1. The caption states that the global mean is subtracted out. Is that the time averaged/global mean? It should be stated. A conclusion that can be evinced from Figure 1 is that the ozone budget results from the balance between two large terms, the advection and the chemical terms, with the eddy transport playing by and large a secondary role except during specific times of the year.

By global mean, the authors refer to the annual global mean based on the 22-year period. This will be corrected in the caption of the article. The overall conclusion from Figure 1 will be better stated.

2. Page 8. The eddy transport is by and large negligible in the Tropics.

This will be underlined in the article.

3. Sections 3.3 and 4.1. Not a very illuminating part of the manuscript. Are they worth extra figures?

Section 3.3 is a short paragraph to identify the mean ozone transport, and its seasonality. The figure, though not necessary, is helpful to visualize the mean ozone transport. Section 4.1 is important because it investigates the trends in the ozone, and more particularly the seasonality of the trends. This is necessary in order to reconcile the trends in the ozone budget equation.

4. Figure 4. It has taken me a while to understand this figure. First, the components (M('),M(z)) are not vectors themselves, they are the components of a vector. More to the point, I had some difficulties in reconciling the plotted divergence with the fluxes. I realize that my difficulty arises solely by the non-conventional form of the eddy fluxes, Eqs (4) and (5). See Andrews-Holton-Leovy (1987; Appendix to Chapter 9) for a more conventional form. The problem is that

ACPD

11, C2787–C2794, 2011

the authors have moved the sign of the fluxes to the divergence. While this is not a problem in general, it creates some difficulties when one tries to reconcile visually the gradient of the fluxes with the plotted divergence. As minor this might appear to the authors, there is also a real problem that results from this convention. On page 12, it is stated that "the vertical eddy ozone flux is upward in the midlatitudes" consistently with a positive region of the vertical flux of M. This implies to me that there is an eddy source of ozone, as in that case eddies would bring anomalous ozone upward. How can that be, if there is so little ozone below 100 hPa? If the authors had used the conventional form, the fluxes would have been negative in the same region, indicating an upward flux of anomalously poor ozone air, which then would be consistent with the idea that there is no ozone to transport upward from the troposphere. This doesn't make any difference for the conclusions of this manuscript, nor the for the divergences.

The reviewer is correct to point out that the components (M('),M(z)) are not vectors themselves, and this will be corrected. As for the sign convention, it is taken from Garcia and Solomon (1983), as cited in the article. Figure 4 in this manuscript is very similar to Figure 4 in Miyazaki and Iwasaki (2005), which is cited. For example, the two figures show the same poleward and downward eddy transport in the Southern Hemisphere polar region, between 70 hPa and 10 hPa (consistent with the mixing near the edge of the polar vortex between ozone-rich and ozone-poor regions); or the same upward and equatorward transport between 150 hPa and 70 hPa in the midlatitudes that the reviewer is referring to. The sign convention in Andrews-Holton-Leovy stems from an analogy of the eddy flux vector with the Eliassen-Palm flux vector (which represents the flux of wave-energy), and does not actually represent the eddy flux of ozone. However, we understand the need to make the sign convention clear and will improve that in the revised manuscript.

5. Figure 6: a. Page 14. The authors don't show that this dip in November is associated with a weakening of the planetary waves and the cited paper is not

ACPD

11, C2787–C2794, 2011

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published yet, so there is no way for the reviewer to confirm this statement.

The cited paper is now a companion paper to this article and is published in the ACP discussion forum. The reference will be updated accordingly.

b. Again on Page 14 the manuscripts states "the negative trend in the chemical destruction of ozone in August". What chemistry are the authors referring to? As far as I know, there is no heterogenous chemistry going on at this time of the year, as the vortex is still in the dark: only when the Sun rises above the horizon, the chemistry is kicked in gear. I think this (as the following discussion of the negative chemical trend in the NH-December) are the result of unresolved dynamics mis-assigned to the residual term as chemistry. Even if gravity waves are negligible (maybe!), planetary scale wave breaking which tends to occur in wintertime will produce dynamical effects that are not resolved by the model ozone budget the authors use.

Figure 6 displays the trends in the ozone transport, chemistry and tendency averaged between 60-85°. There can be chemical destruction between 60-85°S pretty much all year long, although the chemical ozone destruction is weak in June and July. In particular, the polar night in August is located south of 75°S. Furthermore, Lamago et al. (2003) found that when taking into account large solar zenith angles, ozone concentrations are significantly affected from June to August south of 60°S because of photolysis rates of Cl_2O_2. While unresolved processes, such as gravity waves, contribute to the chemical term, it is unclear why they would experience negative trends resulting in an intensification of ozone destruction. Finally, planetary wave breaking is included in the TEM formulation of the eddy ozone transport, as well as in the residual mean meridional circulation. We will include this discussion in the article and add the citation as well.

c. Page 15 top. First I don't believe that there is any chemical loss in August as stated above. Secondly I find odd that there is no statistically significant value

ACPD

11, C2787–C2794, 2011

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of the trend of the tendency during austral spring. Is this because the authors have used the entire record, which contains a leveling off of the ozone depletion approaching 2000? What conclusions would be drawn if the authors had used the record up to the early 1990s? Another point is about the statistical test: are the authors calculating the test using sub-daily or monthly data? I suspect they used sub-daily data from which they get an enormous amount of noise and consequently little statistical confidence. If they had calculated the trends from monthly data, the variance would be reduced and the statistics improved.

See the point above for a discussion on the ozone chemical loss in August in the Southern Hemisphere. About the second point, there is a significant trend in October and December. The lack of trend in November is attributed to the dip in the ozone mean and eddy transport due to a long-term decrease in planetary wave forcing. As for the calculations of the trends, they are based on the monthly means of the various terms, not on sub-daily data.

6. Figure 7. Is the positive trend in the tendency the result of a recovering ozone hole? Also based on this figure, I would conclude that the main balance is between the eddy transport and the chemical term. It should be pointed out that this balance applies only to October-November-December time. In fact, Figure 1 it shows the main balance is between the mean transport and the chemical source term throughout the year, except during OND.

The positive trend demonstrates the strengthening of the recovery of the ozone hole in late spring. The points made by the reviewers will be underlined in the discussion of Figure 7.

Citation: Lamago, D., Dameris, M., Schnadt, C., Eyring, V., and Brühl, C.: Impact of large solar zenith angles on lower stratospheric dynamical and chemical processes in a coupled chemistry-climate model, Atmos. Chem. Phys., 3, 1981-1990, doi:10.5194/acp-3-1981-2003, 2003.

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11, C2787–C2794, 2011

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11, C2787–C2794, 2011

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