

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

### **Anonymous Referee #1**

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The authors use ERA40 analysis between 1980 and 2001 to produce an ozone budget using a transformed eulerian mean (TEM) formulation. The ozone budget is exemplified by the continuity equation for the tracer where the resolved mean and eddy transport are explicitly calculated along with the tendency and the residual is assigned to the chemical source/sink term. The authors acknowledge that unresolved dynamics is not represented with this formulation, but they deem this a small error. The authors attempts also a separation between stationary and transient eddies to determine their relative importance. The main conclusion is that the eddy transport is not a negligible contributor to the ozone budget and its trend is significant enough that it is important for modeling studies to represent realistically.

This study has lots of potential but needs some work in particular in the interpretation

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of the results. I will list general concerns in no specific order of importance, followed by detailed 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.

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.

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?

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.

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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.

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.

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

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

4. Figure 4. It has taken me a while to understand this figure. First, the components ( $M(\varphi)$ ,  $M(z)$ ) 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 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

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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.

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

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.

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 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

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had calculated the trends from monthly data, the variance would be reduced and the statistics improved.

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.

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