

## ***Interactive comment on “Characteristics of lower stratospheric transport as inferred from the age of air spectrum” by F. Ploeger and T. Birner***

### **Anonymous Referee #3**

Received and published: 19 March 2016

This manuscript presents calculations of the seasonal and inter annual variations in the stratospheric age spectrum obtained from the CLaMS model driven by ERA-Interim winds. The manuscript contains some interesting and new results that certainly warrant publication. However, the manuscript is not very well focused, does a poor job of discussing previous studies, and some of the major conclusions (as stated in abstract or conclusions) are not new results. I think the manuscript needs to be revised to be more focused on the new aspects of their calculations (seasonal and inter annual variability, including QBO) and to put these in context of previous studies.

### **MAJOR COMMENTS**

1. The manuscript is not very focused, and the new results are not clearly presented. This lack of focus can be seen from the title, which is very vague (and doesn't match

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much of the manuscript). In my opinion the relatively new and important aspect of the study is looking at seasonality and inter annual variability of the age spectrum, which has only been done in a few previous studies, and none of the previous studies have included the QBO (as the authors highlight in the Introduction). However there is actually relatively little discussion of these aspects, and there is just as much (or even more) discussion is on the age spectrum - mean age relationship, the approximation of  $G$  by inverse Gaussian, and comparisons of residual circulation with modal age. All of these latter issues could be examined using steady flows, and much of what is discussed is already known. My recommendation is to focus on the seasonal and inter annual variations (and QBO) and to minimize the discussion of the other issues.

2. While there are references to previous studies that examined similar aspects of the age spectrum in the Introduction there is very little discussion of these when interpreting the calculations presented here. Because of this it is not clear to me how much of the results are new and how much are just reproducing earlier results with Lagrangian model driven by reanalyses.

The clearest examples of lack of discussion of previous studies are Sections 3 (seasonality) and 5 (inter annual variability) where there is not a single mention of the Li et al. 2012a,b studies which examined exactly these issues. How do the results presented compare with these previous studies? What is new in what is presented (other than a slightly different approach)?

3. Some of the conclusions (as stated in the abstract) are well known facts. This paper may be providing more support, but as written it appears these are new results.

One example is the statement in the abstract that “Interpretation of the age spectrum in terms of transport contributions due to the residual circulation and mixing is generally not straightforward.” This is well known and not sure this counts as a significant enough statement for an abstract.

Another example are the statements towards the end of both the abstract and conclu-

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sions regarding benefits of age spectrum calculations and need for inclusion in model inter comparisons. Again not new, and in fact age spectrum calculations were actually included in model inter-comparisons in late 1990s (Hall et al. 1999). Is this really the major take home message from this manuscript (ending abstract and conclusions with this gives this impression)?

4. There are multiple places where I think previous papers or the current understanding are misrepresented. Examples:

Pg 1, Line 24: As written it suggests that the Hall and Plumb 1994 and Waugh and Hall 2002 papers did not appreciate that there was a range of pathways and an age spectrum, whereas the opposite is true and Hall and Plumb focused on this fact.

Pg 2, Line 6: I don't think it is correct to indicate that the apparent disagreement between observed and models changes in transport is due to age spectrum versus mean age differences. This disagreement occurs if you compare mean age from observations with mean age from models, so not a residual circulation vrs mean age issue.

Pg 2, line 28: Is it a common view that modal age can be related to residual mean mass circulation? Maybe in the tropical lower stratosphere, but I don't it is common to think such a relationship extends beyond this region.

Pg 13, line 19: It is not correct to say that "Hall and Plumb (1994) argued that the stratospheric age spectrum may be well approximated by the Green's function for a one-dimensional diffusion process". They used the 1 diffusion model to illustrate aspects of the age spectrum not to model the actual stratospheric age spectrum (they are explicit about this: "our goal at this point is not to quantify stratospheric transport, but rather to illustrate the points of the previous discussion").

Pg 14, line 7: "This process can evidently not be described by simple one-dimensional diffusion." I think this well known, not only from looking at  $G(t)$  from other three-dimensional models, but also from the tropical leaky pipe model where expressions

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for the age spectrum have been derived (Hall 2000) (see also Waugh and Hall (2002) review).

## MINOR COMMENTS

Pg 6, line 15-19: How long was the clock tracer run for; is it in (quasi-)steady state? It is stated that the agree is good but in polar regions the clock tracer is younger by over 0.5 yrs. Note, the paper of Hall and Haine (2002) might be relevant here. They derive the relationship between “ideal age” (which is an alternate clock tracer) and “mean of age spectrum”. In their example the clock tracer converges quicker than the mean of the age spectrum, which appears opposite to your result. However, this may because you have run calculations for different length.

Eq 7: Why reference Bonisch et al 2009? As you have just mentioned the  $G(t)$  for 1D advection diffusion was used in Hall and Plumb, and this same expression was given in the Waugh and Hall (2002) review paper. Furthermore, this is not a parameterization, this is the exact  $G(t)$  just expressed in terms of the mean and width (rather than flow velocity and diffusion).

Section 5: Is it possible to make composites on east and west phase of the QBO?

Pg 11, line 16 -: Are the results in Orbe et al. (2014) relevant for this discussion?

## REFERENCES

Hall, T. M., Path histories and timescales in stratospheric transport: Analysis of an idealized model, *J. Geophys. Res.*, 105, 22,811–22,823, 2000.

Hall, T. M., and T. W. N. Haine, A note on ocean transport diagnostics: Ideal age and the age spectrum, *J. Phys. Oceanogr.*, 32, 1987–1991, 2002.

Orbe et al. Seasonal ventilation of the stratosphere: Robust diagnostics from one-way flux distribution, *JGR*, 119, 2014.

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