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Interactive comment

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

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We thank all three reviewers for their careful considerations of the manuscript and their well thought-out comments. These certainly helped to significantly improve the paper. In the following, we address all comments and questions raised (Reviewer's comments in italics). Text changes in the manuscript are highlighted in color (except minor wording changes). The main concerns of the reviewers were:
(i) 'The manuscript is not very focused.' (Reviewer 3)
(ii) 'The paper is a bit long.' (Reviewer 2)
(iii) 'The discussion is too terse in places.' (Reviewer 1)
(iv) 'The manuscript does a poor job of discussing previous studies.' (Reviewer 3)
We have taken this criticism seriously and applied several changes to the manuscript. To intensify the focus, the paper now concentrates clearly on the main question regard-

ing the variability of lower stratospheric age spectra on seasonal to inter-annual time scales (as suggested by Reviewer 1), and on the additional aspect regarding the effects of residual circulation and mixing on the spectrum from a global perspective. We removed 3 figures (Figs. 5, 10, 15 in the submitted version), two subsections of the discussion (Sects. 6.2, 6.3 in the submitted version), and the discussion of ENSO-related variability in Sect. 5 (suggested by Reviewer 1). The parts from the old Sect. 6.2, concerning variability of the fraction of young air, are now included in Sect. 3, which presents the results regarding seasonality. The revised discussion therefore clearly focusses on the seasonal and inter-annual variability in the age spectrum and how this variability generates the multiple peaks in the spectrum. Regarding inter-annual variability, we only discuss the QBO, as this is the dominant mode of inter-annual variability in the tropical lower stratosphere and its effects on the age spectrum have not been studied in detail, so far. The discussion of QBO-related effects in Sect. 5 is enhanced (as suggested by Reviewer 2) and an additional figure (new Fig. 12) is used, presenting easterly and westerly QBO composites, to further illustrate these effects (as suggested by Reviewer 3). A by-product of these changes is that the revised manuscript now is substantially shorter (as requested by Reviewer 2).

We changed large parts of the text and included 'strategically placed clarifying phrases', as required by Reviewer 1, in order to enhance clarity of the discussion. A new figure (Fig. 2) is introduced, showing the dispersal of the winter/summer tracer pulses, and is referred to in Sect. 2 and in the discussion to illustrate the transport processes discussed. In particular, we thank Reviewer 3 to point out certain passages in the text where the submitted manuscript was not precise about the existing literature. It was definitely not our intention to mix up results of this paper with what is already known. Hence, we carefully revised the manuscript in order to cite existing literature correctly and to clearly state what is known already and what is not.

General comment:

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.

We appreciate the constructive criticism. As explained in our 'General comment' above, we significantly changed the paper (mainly introduction, Sects. 3, 5 and discussion) in order to better focus on seasonal and inter-annual variability, and in order to correctly cite the existing literature and to state what is known and what is new (see reply to the detailed concerns below).

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 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.*

The manuscript is significantly changed to focus more clearly on seasonality and inter annual variability of the age spectrum, as suggested (see also our 'General comment'). We also changed the title to be more precise about that ('Seasonal and interannual variability of lower stratospheric age of air spectra'). The discussion of the approximation of G by an inverse Gaussian has been removed, and the main message condensed into the last sentence of the discussion. The discussion subsection on the age spectrum - mean age relationship has also been removed. The seasonality of young air fractions (which was previously included in this discussion part) is now presented in the seasonality section 3.

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)?*

Thank you for the constructive criticism! We have tried to improve the discussion of previous literature throughout the text. Examples are:
(i) The introduction, where we now explain in detail that most existing age spectrum studies are based on the assumption of a stationary flow, and that only a few recent ones considered time-dependent age spectra (e.g., Reithmeier et al., 2007; Diallo et al., 2012; Li et al., 2012a/b). We frequently refer to these studies in the introduction, in Sect. 2 and in the discussion. Furthermore, in the revised version we also explain at the beginning of Sect. 3 that the occurrence of multiple peaks in the age spectrum has already been pointed out in recent publications, and we discuss probable causes in the discussion (Sect. 6) in relation to the literature (e.g., Reithmeier et al., 2007;

Diallo et al., 2012; Li et al., 2012a/b). In the discussion, we refer first (P13, L9ff) to the discussions of the peaks in these papers, explain that there is no common understanding, and finally discuss the CLaMS results in relation to them. The fact that in the reanalysis-driven CLaMS simulation multiple spectrum peaks emerge not only at high latitudes but almost globally is clearly different to previous studies, and thoroughly discussed now. As further discussed, we think that it is neither of the recently proposed mechanisms which causes these peaks, but a combination of them (see discussion).

In particular, our analysis is the first full transport model analysis on age spectra based on reanalysis winds including a QBO. Such inter-annual variability can be easily investigated using our modified BIER approach, as it is based on a transient multi-year simulation (Li et al., 2012a/b for instance considered only time-slice experiments). The clear effect of the QBO in modifying and even generating age spectrum peaks is clearly a new result.

Furthermore, in the introduction we now explain precisely (P3, L12ff) that in the tropics the modal age is known to be closely related to the residual circulation, but that no global investigations exist on that, to our knowledge. Hence, our global analysis on regions and seasons where modal age is controlled by the residual circulation (the tropics and the wintertime extratropical stratosphere above about 500 K) is, in our opinion, also a new result.

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 conclusions 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)?*

As stated above (see our ‘General comments’), we tried to better focus the paper on seasonal and inter-annual age spectrum variations. We also removed two sentences from the abstract to be more focussed here. We kept the last part of the conclusions, although it is indeed not new that considering the age spectrum is beneficial compared to mean age (the full skewed spectrum always includes more information than just its first moment). We included citations of two existing examplary papers on that (Hall, 1999; Waugh and Hall, 2002). However, time-dependent age spectrum diagnostics have not been implemented in global models as standard transport diagnostics, hitherto. While advantages of considering the full age spectrum have certainly been stressed many times before, most studies in the last ∼10 years focus still on mean age, which is a quantity very dangerous to interprete. We therefore feel that it can’t hurt to (re-)stress the benefits of age spectra analyses, and to conclude the paper with a related (slightly modified) paragraph.

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

P. 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.*

This misrepresentation was certainly not intended - we fully agree about the merits of these previous studies. We placed the respective citation to a different place not to cause confusion.

P. 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.*

Agreed, although the representation of mixing, and in particular corresponding long-term changes, is likely less robust across models. We have restructured this paragraph somewhat, which should emphasize the structural BDC changes more.

P. 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.*

We admit that we were imprecise with this statement. In the tropics this relation between the mode and residual circulation is known. However, globally no study on this relation exists to our knowledge. Therefore, our results confirm the common view in the tropics and further show that a similar relation holds in the wintertime extratropical stratosphere above about 500 K, whereas particularly in the summertime lower stratosphere mixing effects are important. We changed the text at several places in order to be precise (e.g., P3, L12ff).

P. 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").*

Thanks for pointing the confusing statement out! We entirely agree with the Reviewer's view and changed the text accordingly (corresponding text changes at two places in the revised manuscript: introduction/L20ff, and first paragraph of Sect. 3).

P. 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 threedimensional models, but also from the tropical leaky pipe model where expressions for the age spectrum have been derived (Hall 2000) (see also Waugh and Hall (2002) review).*

We agree with the Reviewer. However, the entire discussion part about age spectrum fits has been removed from the revised manuscript, in order to better focus the paper (as suggested by the Reviewer, see also out 'General comments').

Minor comments:

P. 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.*

Thanks for pointing out this lack of clarity. Indeed, the calculations for the two quantities effectively have a different length. The clock-tracer was subject to a 10 year spin-up (repeating 1979 conditions) and another 10 years of transient simulation (1979-1988) before taking into consideration, whereas the age spectrum has explicitly been calculated over 10 years of transit time (1979-1988 simulation) but with the tail fitted to infinite transit times. Hence, the effective calculation length is longer for the corrected age spectrum than for the 'clock-tracer', and there is no contradiction to the results of (Hall and Holzer, 2002). This is explained in detail now in a new paragraph at the end of the appendix (see also our reply to Reviewer 1).

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).*

We changed the text according to the Reviewer's suggestion, and refer to Waugh and

Hall (2002) in the revised paper.

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

Thanks for this suggestion. We present composites for easterly and westerly QBO phases now in the new Fig. 12 at the beginning of Sect. 5 (inter-annual variability). These composites clearly illustrate how the fraction of young air is increased during QBO easterly phase, due to anomalously strong and fast tropical upwelling, and how the spectrum tail becomes more pronounced during QBO westerly phase. Remakably, secondary peaks develop in the tropical age spectrum at older transit times during QBO westerly phase, indicating an increased impact of in-mixing of old extratropical air on tropical composition. We discuss these findings in Sect. 5, and enhanced the discussion about the QBO-related age spectrum variability.

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

Indeed, the fact that pulses released during summer are more efficiently dispersed meridionally before they reach the tropical pipe and therefore are less likely to undergo recirculation in the stratosphere would be consistent with a stronger return flux into the troposphere for air entering the stratosphere in July compared to January, as found by Orbe et al. (2014). We mention this in the revised discussion version now. However, it is not clear how much this comparison really tells because of the different pulse tracer settings (this paper: released at the surface every other month; Orbe et al.: released at the thermal tropopause in January and July)

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