

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

F. Ploeger and T. Birner

f.ploeger@fz-juelich.de

Received and published: 31 May 2016

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-

Printer-friendly version

Discussion paper



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:

[Printer-friendly version](#)

[Discussion paper](#)



This paper is very well written, enlightening, and concerns an important and timely aspect of atmospheric transport. My primary criticism is that the discussion is too terse in places. I suggest the addition of strategically placed clarifying phrases will help the reader understand the logic of the arguments. In the specific comments, I have noted a few examples where I had a particularly difficult time following the logic. In addition, the discussion of the relationship between ENSO and inter-annual variations of the age spectrum (Figure 10 and the last paragraph of Sec. 5) is not convincing. It should either be improved with some quantitative measures of the information content of that relationship or removed.

Thank you for your encouraging comments. As mentioned in our ‘General comment’ above, we carefully revised the paper in order to enhance the clarity of the discussion (‘strategically placed phrases’, e.g. in introduction/paragraphs 4-5, discussion Sect. 6, text referring to new Fig. 2 in Sect. 2/6). To better focus the paper, the discussion of ENSO-related variability has been removed, as suggested.

Specific comments:

P. 2, line 17: The discussion in this paragraph could greatly enhanced if you briefly explained (1) the motivation for representing transport as a diffusion process and that for representing the age spectrum by a Green’s function, (2) the assumption implicit in those representations, and (3) why the assumptions needed for such a representation are not met.

We rephrased the respective paragraph to be more precise about these points.

P. 2, line 34: Changing ‘using multiple pulses’ to ‘using multiple tracer pulses’ will help clarify what you are doing.

Done.

P. 6, line 16–19: Perhaps I am confused here. Shouldn’t the mean age calculated

Printer-friendly version

Discussion paper



from the age spectrum be identical to the value calculated from a perfectly linear clock tracer? Why is there a (small) discrepancy between the two calculations? Is it because, for ages approaching 10 years, the clock tracer does not capture the full range of source concentrations (i.e., the clock does not start early enough)?

Thanks for pointing out this lack of clarity. Indeed, if the time span for convergence for both the clock-tracer and the age spectrum would be infinity both should be exactly equal. However, the calculations here effectively are of different lengths. We explain this point in detail in a new paragraph at the end of the appendix now (see also our reply to the ‘Minor comment’ of Reviewer 3).

P. 6, line 26–27: It took me a while to decipher the meaning of the phrase ‘coinciding with the youngest peak from spring to fall and the second youngest peak in winter’. Perhaps it would help to be more explicit, for example, ‘the modal age as determined by the apex of the largest peak coincides with the youngest peak during spring, summer and fall and with the second peak during winter.’

The respective sentence has been rephrased according to the Reviewer’s suggestion.

P. 7, line 9–12: The description of the propagation of the peak and the argument that the peaks are due to the fact that the most efficient transport from the boundary layer to the stratosphere is too terse. Perhaps another sentence or two will clarify the argument. For example, I can understand why efficient transport during winter leads to a peak in the summertime age spectrum at 6 months. However, I don’t understand why the subsequent wintertime peaks are also linked to transport efficiency during winter. It seems to me that once the air is in the stratosphere (i.e., after 6 months) then an additional boundary layer to stratosphere transport boost the following winter is irrelevant; unless an important fraction of the air released during a specified winter remains in the troposphere until the following winter – when the troposphere-to-stratosphere ‘transport window’ re-opens.

We admit that the wording was not clear. We changed the paragraph, and strongly

[Printer-friendly version](#)[Discussion paper](#)

advise the reader of the discussion (Sect. 6), which has also been changed to enhance clarity (see our 'General comment' above).

P. 7, line 24–25: *Vertical velocity near the tropopause is also slower during summer than during winter (W. Randel and co-authors have written papers on this) and could be important.*

Indeed, the vertical velocity near the tropopause is slower during summer. However, particularly the reduced young air fractions in the summer hemisphere (Fig. 7 in the revised version) indicate that mixing plays an important role. Likewise, enhanced fractions of air older than 2 years directly above the tropopause (Fig. 7 c/d) can only be caused by in-mixing of old extratropical air. However, in order to better focus the manuscript we removed the respective figure (old Fig. 5) and discuss the related issues now with reference to Fig. 7, in a revised paragraph.

P. 10, line 8–20: *The connection between ENSO and inter-annual variations of the age spectra is, perhaps, believable, but is not convincingly demonstrated in Fig. 10. This could be due to the fact that, while equatorial convection patterns shift substantially as sea surface temperature patterns shift, variations of the average strength of tropical convection have relatively weak connections to ENSO. It seems the authors are trying to see make too much of patterns that appear in Fig. 10 – a problem that can arise when analysis rely too heavily on a qualitative comparison and the ability of the human eye to recognize patterns in a chaotic system (whether or not the patterns are meaningful). At the very least, Fig. 10 should be changed to make it easier to discern how well the age spectra are related to ENSO. It would be better to make the analysis more quantitative. For example, how much to the inter-annual variance of the age spectra is explained by a lag-relationship with ENSO variability? It might be best to simply remove Fig. 10 and the last paragraph of Sect. 5. Fig. 11: Did you create this plot for the global release experiment? (as opposed to 15S-15N) Does it look the same for both experiments? If so, or even if not, that is an important comparison to make.*

We admit that the presented relation between age spectrum and ENSO variability was too vague. We did some further analyses on that, like investigating the global release experiment which yields very similar results to the presented reference. However, in order to shorten and better focus the paper (as suggested by all Reviewers) we followed the advise of Reviewer 1 and removed the figure and the related text. The discussion of inter-annual variability now focusses on the QBO, as the main mode of inter-annual variability in the tropical lower stratosphere (see also our ‘General comment’).

P. 11, line 11–12: *Does the distribution in Fig. 11b mean that the particle pulses do not always have the same mass? That the rate of particle release varies? If so, how is this justified physically? Regardless, please explain this figure a bit more.*

The pulse release rate at the surface is not varying over the year. The figure shows the distribution of source times of the air sampled in the stratosphere. This distribution shows a strong peak at NH winter source times, although the release rate at the surface was the same as in summer. We changed the respective paragraph in the discussion to be clearer about this point in the revised version.

Sect. 6.1, last 3 paragraphs: *This discussion could use elaboration. Please be sure that, each time the effect of some phenomenon on peaks is mentioned, the explanation for how (or why) the effect is carried out is clear.*

We revised parts of the discussion to enhance clarity (see also our ‘General comment’). Furthermore, reference to the new Fig. 2, showing the dispersal of winter/summer pulses explicitly, should help illustrating the processes described in the text.

P. 12, line 26–7: *Regarding ‘This flushing . . . has implications for the chemical composition . . .’. Can you give an example?*

The flushing of the summertime lowermost stratosphere with young air is e.g. important for short-lived species and pollutants. However, in order to better focus and shorten the paper (suggested mainly by Reviewer 3) we removed this part of the

[Printer-friendly version](#)[Discussion paper](#)

discussion (see also our 'General comment')..

Technical details:

P. 2 L. 16: *'Many studies of stratospheric age . . .'*

The wording has been changed.

P. 3 L. 2: *Change 'spectra from seasonal . . .' to 'spectra on seasonal . . .'*

Done.

P. 8 L. 19: *'plays a dominant role' is an over-statement without further analysis. Change 'dominant' to 'important'. Better yet, use 'plays a more important role during this season than during NH winter'.*

Done.

P. 8 L. 21: *Change 'The isolation of tropical air through the subtropical transport barriers' to 'imposed by the subtropical transport barriers' (or some analogous change) to avoid the contradictory imagery invoked by the words 'isolation' and 'through'.*

The entire paragraph is changed in the revised version, and in particular the wording mentioned above.

P. 9 L. 3: *Change 'transit times above' to 'transit times longer than'.*

Done.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-135, 2016.