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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Deriving stratospheric age of air spectra using chemically active trace gases" *by* Marius Hauck et al.

Anonymous Referee #2

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General comment:

The paper by Hauck et al. presents a novel method to infer stratospheric age of air spectra from chemically active trace gases. This inversion method resolves seasonality in stratospheric transport and provides multimodal age spectra. As it is based on chemically active tracers it has the potential to be applied to atmospheric measurement data. Here, the method is applied and validated using a suite of trace gas species with uniform "radioactive" decay from model simulations with the EMAC model. Comparison to model pulse spectra shows very good agreement throughout most regions of the stratosphere.

The stratospheric circulation is not well constrained in current climate models, in particular regarding its variability and trends. Hence, new circulation diagnostics and con-

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Discussion paper

straints from measurements are urgently needed and the topic of the paper is highly relevant for a wide field in atmospheric science. Hence, the paper is without any doubt within the scope of ACP. The paper is well written, the results are very clearly presented and discussed within the context of existing literature in a balanced way. Problems of applying the new method, as in regions close to the tropopause (e.g., multiple stratospheric entry points) and to "real" atmospheric measurements, are critically discussed - something not all authors do so comprehensively.

Finally, I want to state clearly that I don't agree with the other Reviewer's comments which doubt the additional benefit of the paper due to being solely based on model data. Of course, deriving age spectra from "real" observations would be highly beneficial for advancing our understanding of stratospheric transport and for model validation. But this is, indeed, not a simple task. Hence, if a new method has the potential to be applied to measurement data, in my opinion the best first test is to validate the method in a controllable environment. Such a "proof of concept" is presented here based on EMAC model data, and this is also stated clearly several times (e.g., P1, L15; P4, L4).

There is clearly nothing wrong about the presented analysis. The main criticism of the other comments (applicability to measurement data) is actually already addressed and critically discussed in the discussion part. If uncertainty about such applicability to existing observations was an argument for rejecting the paper, most modelling studies would have no right to exist - and in particular many of the already published papers about stratospheric circulation and age of air. Furthermore, even if this novel method turns out not to be applicable to existing observations, with future measurements of better quality and more species it could become applicable and very relevant.

Overall, the paper is one of the better ones that I have read and I do like this validation of the age spectrum inversion method within a model environment very much. The next logical step would be the application to real measurements - but to me this is clearly a new project. Therefore, I do highly recommend publication of this study in ACP.

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Only a few specific minor comments as listed below. But I leave it to the authors to decide whether they help improving the paper.

Specific comments:

P4, L17: I'm wondering whether the assumption of a constant annual mean entry mixing ratio is really necessary. If the seasonality is known it could just be included on the right-hand-side of Eq. 6 and the parameter estimation should still be possible. Maybe I miss something here - it's just a thought...

P5, L9ff: I suggest to discuss the application of the specific spectrum shape (Eq. 3) a bit more critically. The 1D diffusion approximation holds for stratospheric transport only for a stationary state global average, and on tracer coordinates (Plumb and Ko, 1992). Hence, the "diffusivity" K is not representing small-scale diffusion but including the net transport across tracer isopleths (also diffusive processes, but mainly large-scale advection).

P7, L20ff: I didn't find the explanation of the scaling factor here easy to follow and I think the explanation paragraph here could be improved. After having reached Eq. 8, and then even more the discussion of Fig. 1 (P8, L17ff), I finally understood the scaling - but it would be better to directly do so.

P10, L6: Why is the pulse reset done in October of year 9? Therefore, the spectrum is only 9 years and 9 months long. Resetting to zero in January of year ten (directly before the new pulsing) would provide exactly 10 year long spectra. I don't expect any change of the results and conlusions due to that - I'm just wondering if there was some specific argument for that...

P10, L27ff: I fully agree with the authors argumentation that the method is likely biased in the extratropical lowermost stratosphere (exLMS), due to multiple stratospheric entry points. However, the peaks in Fig. 3 above the extratropical tropopause could also be a fact and just be consistent with main entry into the exLMS via the tropical tropopause

and tropically controlled transition region in reality. The relevance of this pathway for exLMS composition has already been shown by Rosenlof et a. (1997). Further evidence, however in combination with model data, can be found in the recent study by Krause et al. (2018).

P13, L16ff: The weaker oscillations in the pulse spectra compared to the inversion results could also just be related to the coarse 3-months timing of the pulses which likely dampens the variability in the spectrum.

P19, L15: I'm wondering how critical uncertainties in the ratio of moments are for the derivation of mean age from observations. Is it possible to give a simple example here, i.e. 20% uncertainty in the ratio of moments translate into a particular uncertainty in mean age.

Technical comments:

P2, L12: Change "It" into "Mean age" P2, L23: I find "in contrast" to strong and would prefer "even". P5, L5: better say "...are then defined as (Hall and Plumb)" P7, L9: I would also cite Reithmeier et al. (2008) here for the seasonal spectrum peaks. P10, L17 (Eq. 12): There is a typo in the equation (and actually also in the same equation in Ploeger and Birner, 2016). The argument of the exponential should correctly be t'-10a. P20, L13: "...that it is a..."

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