Reply to reviewer #2
Estimating Brewer-Dobson circulation trends from changes in stratospheric water vapour and methane

We thank the referee for the detailed review and for the helpful comments and suggestions. We give a point-by-point reply below, where the reviewer comments are repeated in black. The replies to the reviewer's comments are in blue text color. The revised text is given in italics and in quotation marks, with the positions of the corrected sentences in the revised version noted in brackets.

General remarks

This is an interesting and well written paper, but overly detailed. I recommend that the authors take some time to significantly reduce the content to the salient points, briefly summarizing the experiments and driving toward the main conclusions (which are a little nebulous). Concensing and consolidating the paper would improve the focus and make it more accessible to the reader.

We thank the reviewer for the encouraging comments and the advice for improvement. The suggestions have been taken into account in the revised version. As the text had already been consolidated amongst all authors in many iterations, a further shortening indeed proved to be a challenge. However, as we agree that a better focus would make the paper more accessible to readers, we have further worked on the text throughout the entire paper, improved formulations and wording and tried to shorten and focus – taking into account the reviewers comments.

Specific Comments to the Summary

1. "The basic idea, as I understand it, is that the authors want to use measurements of water vapor and methane to determine trends in the BDC. . . . No actual observations (except boundary forcing of methane and water) are used in this paper."

   Thank you for the comment.
   The goal of the paper is to present a proof-of-concept study within an idealized model environment. We aim to give a simple and practical advice for obtaining more reliable AoA trends estimation from the observed H_2O and CH_4, in comparison to broadly used approaches. Therefore, we test several methods within the "model world" and state the conclusions on the possible improvements to the standard approximation. We agree that this idea was not clearly stated at the beginning of the paper. We rephrased the abstract in the revised version of the paper (p1, L8).
   "In this work, we explore how mean age of air trends can be estimated from the combination of stratospheric H_2O and CH_4 data, by carrying out a proof-of-concept within the model environment of the Chemical Lagrangian Model of the Stratosphere (CLaMS).

In particular, we assess the methodological uncertainties related to the two commonly-used approximations of (i) instantaneous stratospheric entry mixing ratio propagation, and (ii) constant correlation between mean age and the fractional release factor of CH_4. By carrying out different sensitivity studies with CLaMS, we test different methods of the mean age of air trend estimation, and we aim to give a simple and practical advice on the adjustment of the used approximations for obtaining more reliable mean age of air trend from the measurements of H_2O and CH_4."

2. The authors assess various methods of using model water vapor and methane to determine changes in the BDC, or basically AoA trends and associated errors. I liked the evaluations they produce and an analysis of various errors (Fig. 5), but I think there is WAY too much detail, and the paper could use more of a reminder of the goals in the results section. For example, near the end perhaps you should show only 3 cases – True, Full and Improve Approx. Discussion of the other cases can be put in an Appendix since the average reader will give up while wading through this material. I think about 30% of this paper could be deleted with no loss of information content.

   We agree that the paper includes many technical details. We have made many internal iterations, but since the subject is a comparison of methods where there is no generally accepted "standard method", one cannot assume the reader will be able to understand what exactly is done without detailed instructions. So, it is important to provide enough details such that the results are reproducible by others. We took into account the above suggestions in the revised version and, overall, we reduced the text of the manuscript (please, see the difference .pdf file between the submitted and updated version of the manuscript).

3. It was interesting that if you assume a simple age spectrum (Eq. 7) rather than try and reconstruct it, the methodology might work (Fig. 8) pretty well. I look forward to the authors applying this technique to real data, and I wonder how observational uncertainty will impact the results given the size of the existing errors.

   We agree that the application of the improved approximation method to real measurements seems promising. The present manuscript should be seen as a first step into that direction by assessing the potential of the methods within the "model world". The application of the methods laid out in this paper to real observational data will be the focus of the future work.

4. As an aside, the authors mentioned a number of times that their analysis won't work in the polar regions, yet they show these regions in the figures which is distracting. Perhaps cutting the figures at $\pm 50^\circ$ might be reasonable.

   Thank you for this comment. We certainly discussed and considered this comment. Although the reconstruction methods does not work at SH polar regions (because of dehydration by sedimentationing ice crystals), we think it is important to keep these regions in the plots. On the one hand this makes the problem evident to all readers directly at first glance. Also, cutting figures only at the SH would lead to a non-centered equator,

what could potentially confuse readers, as there is little dehydration in the polar Arctic stratosphere in winter in the NH.