Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-974-RC1, 2019

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Sensitivity of Age of Air Trends on the derivation method for non-linear increasing tracers" *by* Frauke Fritsch et al.

Anonymous Referee #1

Received and published: 21 December 2019

This paper examines the uncertainties involved in estimating the mean age of stratospheric air (AoA) and its trend from observations of non linearly increasing tracers (here, mainly SF6). A large part of our knowledge of the stratospheric circulation comes from observational estimates of AoA. In this study, the authors evaluate the uncertainties of the AoA estimation method from measurements (Engel et al., 2009, 2017) by applying it to model SF6 data and comparing the obtained estimate to the actual model AoA trend. The paper's topic is of interest to ACP's readership and the analyses presented are thorough. The paper is definitely worth publishing. However, I believe the presentation could be made more pedagogic and would like the authors to carefully consider my comments below. For this reason I recommend major revisions.

Printer-friendly version

Discussion paper

Main comments

1) Presentation: The uncertainties discussed by the authors stem from the non linearity of SF6 increase at the boundary, which is illustrated in Fig.1. This non linearity is small in recent years but larger at earlier times when the time series is more convex (Fig. 10). As a consequence, the small sensitivity of the AoA retrieval on the chosen parameters for the 2010s becomes much larger for the 1980s when there is a kink in the time series, as illustrated in Fig. 9 12.

While this is clear in the manuscript, I think it should be stated earlier in the text. I would recommend the authors to first present the uncertainties in age estimates from SF6 at different times due to the shape of the curve (Fig. 9 to 12) before discussing the impact on the trend, and the improvements obtained by taking variable ratio of moments from the model (Fig. 5 to 8). Figure 10 in particular comes exclusively from the analysis of the time series and should be presented earlier on, to explain what might be expected.

-Implication of non-linearity of SF6 for the uncertainties in AoA estimates

-Quantification of uncertainties and improvements obtained by constraining the ratio of moments

2) In the abstract p1 line 22 the authors mention both SF6 and CO2 observations being used to derive AoA, but in the model they only examine SF6. A nice addition would be to also discuss CO2, but it might be beyond the scope of this study.

3) One of the messages I take from this paper is that mean age of air derived from observations may not be the optimal quantity to compare the model with. A better approach would be to introduce the tracers (CO2, SF6) in the model and to compare the age estimates obtained applying the same method to modeled and observed tracers. The authors may want to put that forward in their discussion and evtl. abstract.

Specific comments and typos

title: “non-linearly increasing inert tracers”

p1 l 22: If possible, it would be interesting to quantify the uncertainty of the method for CO2.

P2 : in the introduction you first present the concept of age spectrum and then the mean age which is its first moment. I would do the same in Sect. 3 p3, i.e. put 2.1.1 and Eq. (2) first and then define mathematically the first moment of the spectrum and its relation to a linearly increasing tracer (Eq. 1.)

p2 l 27-31: In this paragraph you mention two sources of uncertainty for deriving AoA from measurements (non linearity and non inertness). As far as I understand you only address the non linearity, and your EMAC simulation does not have the SF6 mesospheric sink. Is that correct? It should be stated here (and in Sect. 2.2 that your EMAC SF6 is inert).

P3 l 1: maybe replace “the non-linearity of the SF6 tracer boundary condition” ?

P3 Sect. 3 : See above: I would first introduce the age spectrum and its relation to the boundary condition of an inert surface-emitted tracer. Then introduce mean AoA.

P3 l20: the reference location is the surface here, correct?

p4 l1 :”quadraic” → Quadratic

P4 Eq. (3): please define t’ and t0

p4 Eq (4): reference for this equation (Volk et al., 1997 ?)

p4 line 11: please provide a formula to define gamma and delta mathematically. Also note that delta**2 is not the second moment strictly speaking (there is a factor of 1/2 involved here)

p4 lines 13-14 : “approximating the reference time series by a second order function”: a second order polynomial (defined in Eq. 1).

p4 Eq. 5 : Introduce a symbol, e.g. F for the fraction of input

p4 line 24: Convolution Method “Numerical convolution method”. The other method also implies an analytical convolution with the fit polynomial

p 5 line 2-3: “this approach requires only an assumption about the ratio of moments”: this is not true, there is an additional assumption: that the spectrum is an inverse Gaussian. Although the fit method also uses that assumption to estimate t_{fit} for a given fraction of input F, once the quadratic fit is chosen Eq (4) is exact whatever the shape of the age distribution might be. Actually, you might say that the numerical convolution and the quadratic fit both use an analytical approximation, but to different functions: one approximates the time series by a second order polynomial, the other approximates the spectrum by an inverse Gaussian. The uncertainty related to the fit method is arguably easier to constrain. Anyway, combining the two methods might provide a better idea about the AoA uncertainty, as you show in the discussion.

P5 : How long are the simulations? Are you confident that the early part of the simulation is not affected by spin-up? I would already raise that issue here.

P6 l2-3 : well-constrained: What do you mean exactly? Is it just that time series shifted?

P6 line 4: interpolated : extrapolated ?

P6 line 6: “released at the surface”: globally?

P7 l1: “it is fairly easy”: “it is straightforward (Eq. 1)”

p7 l3: How different is the mean AoA between RC1. . . and transpulse?

P7 l 10 : “spectra resolution” : “transit time resolution”

p8: legend of fig. 3: are you doing a fit or of eq 6 ? Or using the width and mean age

estimated from the spectrum?

P8 line 12 :"we can expect" : "we expect"

p9 l3: "selection": "section"

p10 line 1: "it is clearly evident" : "it is evident"

p 11: Fig. 6 the different subfigures need to be explained in the legend

p 11 l 8: $F = 98$

p12 Fig. 7: I agree this biennal and 11 year cycle in the ratio is interesting and deserves further investigation

p12 Fig. 8 : This is the main figure of the paper,

p12 legend of Figure 8: which confidence test are you using ?

P13 l6: "In such cases a reference location closer to the troposphere should be used.": What do you mean?

P14, Figure 10: it would be interesting to see the time series together with exemplary fits

p14 l 20: As I mentioned above, I am not at all convinced that the numerical convolution method is better

p16 Figure 13 and p11 Figure 6: Is the mean AoA displayed here a time average? Over what period?

P16 line 16: spin-up: I think this information should be given earlier, in the model description.

P17 l 8-9: please consider my comment above regarding the advantages of the convolution method. Again, I prefer the term "numerical convolution".

P17 l 10: 'it is rather certain, that larger ratio of moments are more realistic." : You

should specify according to the model (Figure 4).

p19 line 19: “the larger the ratio of moments...”: I would state here again that the retrieved AoA is older in the 1980s with larger ratio of moments while it is left unchanged in the 2010s, so that larger AoA imply more negative trends

p19 line 30: “the parameter selection helps to resolve ...”: Please rephrase, for instance: “Hence, the discrepancy between model results and the Engel et al; observations may be partly explained by the choice of parameters for the retrieval in Engel et al.”

As far as I understand, the discrepancy is not completely resolved since the recalculation in section 4.2 still doesn’t agree with the model. This should also be clearly stated.

p20: One of the messages I get from your study is that, to evaluate transport in models and compare them to observations, it would be preferable and more direct to implement SF6 like tracers (rather than using estimated and modeled AoA). Would you agree? This could be put in the conclusion.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-974, 2019.

Printer-friendly version

Discussion paper