Responses to the editors comments on "Simulating age of air and distribution of SF_6 in the stratosphere with SILAM model"

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Your further changes and responses seem generally adequate to me and I think that the paper makes some interesting points. I understand that you do not wish to address all of Referee 3's comments by making changes to the paper – and, in the end, you as authors, have to take responsibility for what is in the paper. On the other hand I do feel that some of Referee 3's comments remain valid and that they can be addressed by simple further clarifications.

Please provided a revised version of the paper which makes the following minor changes. I regard these as straightforward, but if you really do not want to make one or two of them then provide clear arguments why this is the case. After these changes have been made I will accept the paper.

[The next comment is quite long – but the summary is that I am requesting a clearer specification of the model and a clearer argument as to why it is fit for purpose.]

l98-117: Referee 3 requested evidence that the SILAM model was fit for purpose. You have cited the Sofiev et al (2015) paper. That demonstrates that the SILAM model performs well on a number of basic tests, but it does not demonstrate that, when driven by e.g. ERA-I velocity fields, it provides a good simulation of stratospheric tracers. Please take a little more time to explain the logic here.

Sofiev et al (2015) show that the SILAM numerical model performs well in a range of problems in simulating tracer advection by a specified wind field.

Others, e.g. Diallo et al (2012) have shown that using other numerical approaches that ERA-I winds give a good (to the extent that can be assessed) simulation of stratospheric tracers. Therefore on this basis you consider that the SILAM model driven by ERA-I winds will also give a good simulation of stratospheric tracers.

But this logic ignores the point that the vertical winds are calculated in SILAM using a certain method – can you be sure that this method works for ERA-I wind fields? Have you for example, done any tests of prediction of age-of-air or some other artificial tracer vs the prediction of Lagrangian methods such as those used by Diallo et al?

Furthermore the procedure for calculating vertical transport in given in 3.5 is quite complicated and I am a bit confused whether this is the procedure described in Sofiev et al (2015), or whether it is a new procedure – please clarify.

You should also make it clear that the model is 3-dimensional and give details of the resolution. You have said that the velocities are retrieved on a 500x250 lat-lon grid – I think actually you mean lon-lat grid – but you haven't said explicitly that

this is the resolution of the numerical model. Again re vertical levels you have given details of the levels on which the velocities are provided, but have not confirmed explicitly that these are the levels in the SILAM model. (The velocities could have been interpolated to some other grid.) Actually I now see that some of this information is in Section 3.5 – and the information on the resolution of the velocity data is provided again here – that is a bit confusing. At the very least the term 'setup' used in Section 2.1 needs to be more explicit and mention grid resolution etc.

This then returns to Referee 3's overall point – can you provide some concrete evidence that the SILAM model gives similar results to other approaches for calculating stratospheric tracers from ERA-I winds or similar. If you cannot then you should say explicitly that whilst the SILAM model has been carefully constructed it is not yet know how it performs against, say, Lagrangian calculations using diabatic vertical velocities and any results are therefore subject to this uncertainty.

Response:

Thank you for the detailed suggestion! We followed the recommendation and expanded the "SILAM…" section (Sec. 2.1) by collecting the setup information from other sections and adding the missing data. Details of the meteorological setup are now all collected to the section . Finally, the chemical setup of the run is in the unified sections 3.1 and 3.4, which is now the section 3.3. We did not put it into the section 2 because the list of tracers was dictated by the physical processes considered in the study - and they are described in sections 3.x.

The validity of the meteorological data processing restoring the global mass conservation of the fields is an important question, which however has been addressed by the authors of the methodology. We highlighted it in the updated section 2.2. The second confirmation indeed comes from the comparison with the Lagrangian experiments of Diallo et al. (2012), the current study includes this very comparison in the section 5.2 - the corresponding sentence is now extended. We have also added a reference to that comparison in the section 2.2.

l449: Give the difference from Kovacs et al in the same units as are used in the plots (and elsewhere), i.e. pmol/mol rather than pptv. Also include a following sentence 'Note that whilst we regard this newer version of MIPAS SF6 data as an improvement, it has not yet been reported in a publication, and on that basis is subject to uncertainty.'

Figure 6: include information on meaning of dashed lines in the caption.

l471: 'non-monotonous' > 'non-monotonic'

l500: 'overstating' – not the right word – I suggest 'overestimating' is better:

l540: as above 'over-stated/overstating' should be 'over-estimated/over-estimating'

l755: 'and the SF6 observations are potentially a good means to evaluate it'

l758: 'downdraft' > 'descent'

l769: 'braking' > 'breaking'

Response: Done. Thank you!

References

Diallo, M., Legras, B., and Chédin, A.: Age of stratospheric air in the ERA-Interim, Atmos. Chem. Phys., 12, 12 133–12 154, https://doi.org/10.5194/acp-12-12133-2012, 2012.