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Interactive comment on “Using beryllium-7 to assess cross-tropopause transport in global models” by H. Liu et al.

Anonymous Referee #1

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GENERAL COMMENTS:

Liu et al. study the impact of various meteorological data sets used in the Global Modeling Initiative (GMI) chemical transport model (CTM). In particular, they use a beryllium like aerosol tracer to assess stratosphere-troposphere exchange (STE) in the different meteorological fields. Since it is well known that all data fields have issues in realistically representing this process, constraints on STE in the model were made based on surface concentration measurements as well as on observed deposition fluxes of beryllium 7 (7Be). Moreover, the model results were compared with airborne measurements of 7Be and ozone in the upper troposphere and lower stratosphere. The observational constraints were further applied to other model simulations with the GFDL AM2 and GEOS-Chem, finally leading to the conclusion that these constraints result in a more

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reliable STE flux assessment.

Although research is conducted for a long time, STE is still a topic of intense debate. To assess the strength and location of STE properly is important for both air pollution studies as well as climate projections. Comparing available meteorological fields and assessing the strength of the errors of STE as well as the physical reason for these errors is a valuable contribution to further improve the quality of meteorological data sets. The paper is generally written in an understandable way, however, sometimes with the tendency to be a bit too descriptive. This finally results in a relatively long paper. The figures and tables are generally of good quality and presented along the discussion in the text. The applied methods are sound and the conclusions follow the discussion in the sections before. Overall, I support the publication of this study in ACP, after the following comments are addressed.

DETAILED COMMENTS:

1) The introduction could benefit from some updated references. For instance, the paper from Riese et al. (2012), JGR, which shows the importance of assessing the concentrations of certain trace species in the UTLS (page 26134, line 8). The study of Skerlak et al (2014) presents climatology of STE and numbers on STE mass fluxes over the entire ERA-INTERIM time period (see paragraph on STE starting on page 26136, line 21). Maybe recent model comparison (from CCMI, ACCMIP, CMIP5) could also give further numbers.

2) Regarding the model (section 2.1), what does "nearly full chemical treatment" mean? Could you specify the chemical mechanism a bit more in detail (NO_x-VOC-O₃, sulfur?). Furthermore, why are the aerosol and chemistry simulations conducted independently?

3) The horizontal resolution of the model simulation with 4°x5° is very coarse (section 2.1), especially since it is known that STE is very sensitive to the model resolution both in the vertical and horizontal. Is it possible to include at least one further simulation

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with one meteorological data set with an increased resolution and to discuss the differences? Is STE still overestimated in a high-resolution simulation? And consequently, is there a "threshold" resolution at which the observations are met satisfactory without applying further constraints. Also in section 6, wouldn't it be more sensible to use a higher vertical resolution in the tropopause region in the AM2 simulations? Moreover, did I understand it right that always only full levels are either stratospheric or tropospheric or is there also an interpolation applied between two model (interface) layers?

4) Could you please mention which advection scheme you are using and provide a reference? (page 26140, line 5)

5) Beryllium attaches to the ambient aerosol and then it is treated as the aerosol. In the model the bulk mass is tracked. Is there an aerosol climatology provided to the model to assess the ambient aerosol. Or is the beryllium treated as a quasi-passive tracer with a fixed modal representation? If so, then it would be sensitive to give some additional meta information, such as molar mass (important for the dry deposition) and radius and sigma of the aerosol (usually important for the scavenging efficiency). Sub-micron particles can still have quite different scavenging efficiencies.

6) In section 2.2, it is mentioned that clouds, precipitation as well as convective transport are mainly responsible to obtain a good distribution of ^7Be . However, for STE often the strength and location of the subtropical jet is also crucial where quasi-isentropic exchange between tropospheric and stratospheric air masses can occur. Maybe it is worth thinking about to include a comparison of the location and strength of the jets between the different meteorological fields. The comparison in section 4 would also benefit from such a discussion. Is it possible to assess the error in the model deposition fluxes resulting from the over- and underestimation of precipitation? (page 26141, line 17 - 20)

7) In section 2.3: Following comment 5), how is the beryllium introduced into the model? Is it distributed evenly over the globe? Are there hot spots? Does the strato-

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spheric aerosol have different parameters (size, sigma, etc.) than the aerosols in the upper troposphere? Moreover, and please correct me if I am wrong, but do I get it right that you use a constant ^{7}Be production rate, which is, however, not so constant in reality (since it depends on the solar cycle). Thus, the constraints from the observations are based on a variable source strength, however, the model results are based on a constant source strength. Is it somehow possible to discuss this potential error between observations and model results a bit more in detail?

8) Section 2.4 is difficult to read. However, the main points are not that difficult to understand. Maybe think about reducing the current content slightly and highlight the main points a bit more.

9) In section 5 a comparison with other modeling studies is presented. It would also be helpful to provide some further numbers from studies of STE. Potentially, studies analyzing reanalysis data sets could be included, such as Skerlak et al. (2014). However, this could also be part of the introduction (see first comment).

10) At the end of section 6 changes between results from model simulation are discussed. The differences are attributed to the various GEOS-DAS versions. Could you potentially list the major changes between the various data sets. Is this related to finer native resolution, different physical parameterizations, etc.? This might further help to understand the changes in the beryllium results. Why is cross tropopause transport slower in GEOS3-DAS? Is the jet structure different? Does the location of the tropopause vary strongly? Have there been significantly changes in the assimilation cycle?

11) Why is the thermal tropopause used to calculate the tropospheric TOC in section 7? Would it not be more sensible to use the dynamical tropopause (based on a potential vorticity threshold, eg. 2 pvu). The temperature lapse rate tropopause is often not assumed to be the best marker between stratosphere and troposphere, since there are situations where the thermal tropopause is not so well defined.

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12) The discussion has often a quite descriptive character. An example is the second and third paragraph in section 4. STE is compared based on surface measurements of ^{7}Be . It is mainly stated that there are differences between the model simulations but not what causes the differences, which are the main driving processes that cause the differences. I think the manuscript would benefit significantly, if such additional information could be provided directly.

13) What is meant with satisfactory representation in line 7 on page 26157? Is this based on a certain (small) deviation from the expectation?

TECHNICAL COMMENTS:

- 1) page 26142, line 6 : LP67 is not specified explicitly
- 2) page 26146, line 5 : tropospheric → tropospheric
- 3) page 26147, line 16: please define once SCM, either in the text or in a figure caption
- 4) page 26150, line 19: what is meant with standard simulation?
- 5) page 26161, line 16: "to" is missing → helping "to" reduce ?
- 6) page 26136, line 19: verus → versus
- 7) page 26150, line 10: middle latitudes → mid-latitudes
- 8) page 26158, line 4 : greatest → largest
- 9) Figure 2: maybe the convective mass fluxes could be compared to a high resolution convective mass flux of one reanalysis data set to see how large the difference are caused by the reduced resolution.
- 10) All colored latitude-height plots could benefit from a color bar (Figures 3, 6a, 7a, 9a, 10a, 11, 12)
- 11) Figures 11 and 12 could potentially be merged to one Figure 11 with two panels a,b, since they show the same content.

12) In all line plots: at least in the printed version, the blue and purple lines are difficult to distinguish.

13) Caption of Figure 7 says "Same as Fig. 4...", I think this should read "Same as Fig. 6...".

14) Figure 15: a) and b) are mentioned in the caption but are not assigned to the two panels. Moreover, is it possible to add the zonal variability to Δ TOC and Δ 7Be in panel a), maybe by using gray contours. And is it further possible to add error bars to the best fit line in panel b)?

References:

Riese, M., F. Ploeger, A. Rap, B. Vogel, P. Konopka, M. Dameris, and P. Forster (2012), Impact of uncertainties in atmospheric mixing on simulated UTLS composition and related radiative effects, *J. Geophys. Res.*, 117, D16305, doi:10.1029/2012JD017751

Škerlak, B., Sprenger, M., and Wernli, H.: A global climatology of stratosphere–troposphere exchange using the ERA-Interim data set from 1979 to 2011, *Atmos. Chem. Phys.*, 14, 913–937, doi:10.5194/acp-14-913-2014, 2014.

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 15, 26131, 2015.

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