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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

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

Anonymous Referee #2

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This manuscript uses the NASA Goddard Global Modeling Initiative (GMI) CTM, driven by 4 different meteorological data sets, to simulate the distribution of Be-7 in the atmosphere and its deposition to the surface. The meteorological data sets are known to differ significantly in their treatment of stratosphere to troposphere exchange STE (among other things); the authors hypothesize that Be-7 should provide a sensitive (and computationally inexpensive) test of how well STE is simulated in the models producing the meteorological field driving the CTM. A convincing case is made that compilations of observed Be-7 concentrations, coupled with prior work combining Be-7 and Sr-90, and long-term measurements of Be-7 deposition at a small number of mid latitude NH sites are adequate to assess how well the 4 different meteorological data sets implement STE within the GMI framework.

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In later sections, the authors apply the Be-7 tests described above to several additional models, with additional meteorological data sets to reinforce the utility of Be-7 as a routine first-order test of how well any global model is simulating cross tropopause transport. Similarly, they drive the full chemistry version of GMI with 3 of the meteorological data sets used for the Be-7 simulations and compare simulated ozone to observations, finding that problems with STE identified in the Be-7 tests impact the simulated ozone fields in similar ways. In my opinions, these latter sections (6 and 7) are presented "in a rush" and do not add tremendous value to the overall story. I urge the authors to consider whether section 6 (and figures 11 and 12) could be deleted, and if section 7 could not be distilled to a few sentences discussing figure 15.

Overall, this is a solid paper which does a thorough job making its main point, but feels too long. The motivation, approach, results and implications are clearly presented in most of the manuscript.

Specific comments.

Pg 7 line 31 and page 8 line 1. Given that previous GMI studies have used met fields from NCAR (CCM2 and CCM3) and found CCM2 to be best of one group tested, and CCM3 as good as its competition, why was the current version of CCM not included in this study?

In section 2.3, the authors should provide some rationale for the decision to use the LP67 Be-7 formation rates, which have the highest global mean column production rate of the 3 options listed in lines 2 and 3 on page 9. A few sentences later in the same paragraph the authors state that a more recent formulation of Be-7 production rates (Usokin and Kovaltsov, 2008) "broadly agree with those of LP67 with slightly (about 25%) lower global production rate." This would seem to imply that the global mean rate from Usokin and Kovaltsov is essentially identical to that suggested by Obrien et al., 1991, raising the question: if 2 approaches basically agree, why choose an older one with higher production rates? Later on in the paper there are several times that a

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positive bias is found when comparing model estimates to observed Be-7, which might partly be due to using too strong a source. For example, on page 13 lines 18-20 it is stated that the Usokin and Kovaltsov source would probably largely eliminate positive model bias in LS, presumably the Obrien source would also move things in the right direction, so why use LP67? Likewise on page 14 lines 2-5 and again in lines 19-22, it is stated that if observations had not been scaled down 28% the positive model biases would be much smaller, suggesting if the source in the model was 25% weaker the agreement would similarly improve.

In section 2.4, the discussion of equations 2 through 7 is confusing to me, even after reading it many times. Can this be made both more clear, and probably shorter since in the end it turns out that relatively little time is spent in the discussion section on the scaling factor.

Section 2.5 first sentence. While mathematically it is equivalent to either scale down long term averages of observed Be-7, or to scale up the production rate (by 28% in either case) to account for the fact that the production rates are produced for a year of solar maximum (production minimum) I have a philosophical preference for scaling the production rate up. As noted in section 2.3, there is significant disagreement between published estimates of the production rate (range is more than a factor of 2) so it would seem no one should object to adjusting these a little to facilitate model/data comparisons, while the data are the result of significant sustained effort to collect and analyze samples as accurately as possible.

Section 4, discussion of Fig 6 (mainly on page 15, but also comments/questions about the figure and caption) Seems that you need to comment about the fact that according to the contours much of the lower strat in all 4 models shows strat fraction of Be-7 significantly less than 100%. Is this related to different definitions of the tropopause, or to seasonal movement of the tropopause vertically muddling the annual averages? Does not seem plausible that trop to strat transport is bringing that much tropospheric Be-7 into the LS, given the steep vertical gradient in concentrations. A more minor

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point, but first sentence in the figure caption says the plot shows “strat fraction of zonal mean tropospheric Be-7 concentrations”, but it clearly shows strat fraction in the full depth of the model atmosphere.

Section 4, first paragraph on page 15, lines 9-15 and second paragraph lines 26-29. Here you strongly suggest that fvGCM and GEOS4 met fields are doing quite well with STE (clearly much better than the other 2). First paragraph ends by pointing out some very minor differences between the two “better” data sets, which are largely negated by the statement in the second paragraph. However later on you circle back and claim there are significant differences (e.g. pg 19, lines 8-11 and pg 20, lines 31-33), and claim that these were pointed out here in section 4. If you feel these differences need to be highlighted, make that point more strongly in this section.

Technical comments Pg 5 line 32 representations

Pg 8 line 1 delete “and”

Pg 8 line 12 Clouds and precipitation

Pg 8 lines 31-32 probably should note that Lal and Peters will be referred to as LP67 since you start doing that on page 9 (but not consistently). If you are going to use the acronym, probably should do it everywhere after pg 8.

Pg 14 line 6 deposition

Pg 15 lines 6-8 while it is true that GISS puts maximum strat fraction in the troposphere at high southern latitudes, both Fig 6a and 6b show that the fraction is nearly constant from just > 30 N all the way to the north pole

Pg 15 lines 16-17 and the caption for Fig 6b. I think you are talking about strat fraction both in surface air, and in deposition, but as written it is ambiguous whether the dashed lines shows the total deposition, or the stratospheric fraction of total deposition

In current draft, many of the figures are a little fuzzy. This is more distracting in line

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plots, but also seems to degrade many of the maps. Specific examples: Figs. 1, 3 (especially contour labels), 4, 5, 6, 7, 8, 10, 13, 14.

In caption for Fig 7. pretty sure it should be "Same as Fig 6" (not 4)

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