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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Influence of model resolution on the atmospheric transport of ^{10}Be" *by* U. Heikkilä and A. M. Smith

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General comments:

This study attempts to use observations of surface concentration and deposition flux of radioactive isoptopes of beryllium and lead to test the impact of model configuration on the simulated lifecycle of these compounds. However, since differences between the fields for the different model configurations are small, and the scatter in the measurements is large, the authors are not able to say whether the higher resolution simulations or those with a well-resolved stratosphere lead to better results or not.

There is already a fair amount of research on the impact of model resolution on model climate (e.g., Hack et al., 2006, Roeckner et al., 2006), middle atmosphere dynamics (e.g., Richter et al., 2008), tracer transport (e.g., Land et al., 2002, Aghedo et al., 2010), stratosphere-troposphere exchange (e.g., Gray, 2003) and also on the effects of low-

top vs. high-top model configurations. None of this work is referenced or referred to in the manuscript at present which must be corrected. Much of the results of the present study can be tied to prior results, for instance the fact that vertical resolution has an important impact on cross-tropopause transport. I strongly suggest that the introduction needs to include some theoretical explanation of why model resolution affects simulated tracer transport, and summarizing pertinent results of past studies. Also, the discussion of the results needs to be amended to include comparison with prior studies. Of primary importance here is comparison with the results of Aghedo et al., (2010), who also use the ECHAM5 GCM, and two model resolutions common to the present study. There are also obvious similarities on the diagnostics used in the Aghedo et al. (2010) study and the present study, these results should be directly linked.

The comparison of modeled surface fields with observations suffers from a lack of quantification. Many results of the study are presented as maps of modeled concentrations or deposition fluxes with observations overlayed as colored points. As a scientific study, some quantification needs to be made concerning the agreement between model results and observations. Scatter plots of measured vs. modeled quantities would be helpful, as would an overall metric of agreement, for instance a root-mean-square error for the different model configurations to be better able to quantify the effect, if any, of changing model configurations.

Finally, the central results of the study show the latitudinal distribution of the deposition of 10Be produced in different regions of the stratosphere and troposphere. These results are quite interesting, but there are some problems. Firstly, there is no explanation of how these results are obtained, as they cannot, as far as I know, be assessed based on normal monthly mean output fields of a typical model run. Secondly, almost all of the discussion of these results is very hard to follow, as sentences do not typically state clearly what they mean to say. Finally, the authors tend to exaggerate the interpretation of these results. The results link production and final deposition locations, but do

not directly say anything about atmospheric mixing, transport pathways, or location of cross-tropopause transport. While distributions of chemical tracers are used to indirectly quantify model transport processes (see Chapt. 5 in SPARC CCMVal, 2010), actual assessment of transport "pathways" or cross-tropopause "locations" relies upon detailed trajectory studies, or other more detailed analyses. This fact puts in question a number of statements in the abstract, results and conclusions, and even the title of the article.

Specific comments

Abstract

Line 6: There are no direct assessments of mixing or transport paths within the study – the comparison of model and observation surface fields is at best an indirect measure of such processes. The abstract needs to be clear in what was actually done, and that is compare surface fields.

Line 7 vs. line 15: There is a contradiction here, as the fact that a "full validation of the resolutions is not possible" makes it impossible for the study to permit one to choose an acceptable resolution, since one could argue that none of the resolutions produce physically accurate reconstructions. These sentences need to be rephrased.

Line 25: See comment on mixing and transport above. Having a realistic surface field does not validate the atmospheric mixing or even its transport pathway, it could well be that a number of model errors compensate for each other leading to a realistic surface field.

Introduction

Only 10Be is mentioned specifically in the introduction, but some mention needs to be made of 7Be, how its source and relevant properties might be different than 10Be. Important details of the lifecycle of 10Be, 7Be and 210Pb need to be introduced up front. It is stated that 210Pb, like the beryllium isotopes, "attaches to ambient aerosol

and undergoes the same transport and deposition". This point needs to be made earlier, which explains why HAM, an aerosol model, is used to model the transport of these isotopes.

Also: if the lifecycle of the beryllium isotopes is controlled by their attachment to ambient aerosols, and the transport of these aerosols, it seems important to examine, as a first step, the impact of model configuration on the background stratospheric aerosol layer. The vertical extent and resolution of the model could in theory have a large impact on the transport of sulfur into the stratosphere, which could have important impacts on the stratospheric sulfate aerosol burden.

Line 5 By what process are beryllium isotopes created in the stratosphere?

Line 10: Why would a version including the middle atmosphere be good to use? Because the dynamics of the stratosphere, and hence the transport of tracers through the Brewer Dobson circulation is more realistic in high-top models.

Pg 18534

Line 5: See general comments: why does model resolution matter?

Pg 18535

Line 5: this trade-off is only necessary when one limits the CPU time, i.e., the decrease in resolution in the lower atmosphere is not a fundamental result, but a practical one.

Line 16: again, "the limit" is too strong, since other groups at other computing centers may not have such an issue.

Line 19: New paragraph "In order. . ."

Line 19 – 26: It is not clear how separating the production rates of beryllium into zonal bands allows one to study the transport path.

Pg 18536

Line 23: the temporal coverage of the measurements needs more description, i.e, how long were the long-term and short-term measurements? Is there any evidence of trends or innterannual/decal variation in the measurements which would compromise the technique of lumping all measurements together irrespective of temporal overlap?

Pg 18537

Line 3-5: repetition in Figure 2 description.

Line 6-7: The agreement between model and observations in Figure 2 is not obviously "good", and subjective statements like this should be avoided. The model produces a clear meridional structure in 7Be values, with higher values in the tropics and smaller values in the high latitudes, but this is not consistent with the observations, which show (to my eyes) maximum values in the midlatitudes (North America, Europe and South American west coast) and the Antarctic coast. These points are described in the text below, but calling the agreement between model and observations "good" seems like a gross overestimate.

Line 8: If mountains are such an issue, then it may be misleading to plot all measurements against surface values from the model.

It appears by eye that of all the observed fields considered, 7Be surface air concentration is the only field that has noticeable differences between the model configurations. This is also the field which has the highest measurement quantity. It is stated that model-observation differences may be related to the fact that measurements are often taken on mountain-tops, and are compared here to surface model results. If the gradient of 7Be is truly so strong within the boundary layer, it would be worth interpolating the model results to the height of each measurement station, in order to get a better match. These results would obviously not be shown on a map, but could be shown in a scatter plot and used to compute whatever overall metric is used to gauge model measurement agreement.

Line 21: The most obvious difference between the model resolutions is that the global mean 7Be concentration seems to depend strongly on resolution. This should be stated here, perhaps with a reference to further discussion of quantities (e.g., tropospheric burden) in Table 2.

Page 18538

Line 11: How short are the measurements? Months?

Line 22: 5-year mean

Line 24: remove "of"

Line 25: Very subjective statement: some quantification of the model to observations differences is needed (see general comments).

Pg 18539

Line 18: Again, very subjective. One should quantitatively compare the model and observed fluxes in the locations available and compare to uncertainties in the source strength.

Pg 18540

Line 4: The first thing to address here is the >10% difference in production rates between the different resolutions shown in Table 2. Through what mechanism does this occur?

Line 6 vs. Table 2: Line 6 says globally integrated quantities, Table 2 caption says global mean. Since units of burdens are given in g (not g/mˆ2) it is probably the former.

Line 7: It is not clear how residence time is calculated. Are tracers in the model uniquely identified that one can know the production time and deposition time of individual atoms, and then average over large ensembles?

Line 14: residence time observation reference?

Line 21: By “stratospheric transport” you mean actually “stratosphere-to-troposphere transport”, or “stratosphere-troposphere exchange”.

Pg 18541

Line 9: Figure 9, burdens should be in atoms. Atoms/m2 would be a “column density”.

Line 13: largest amplitude variation/gradient over latitude.

Line 19: did not reveal significant difference in <something> between model resolutions.

Pg 18542

The description of Figure 10 is unclear. As I understand it, this is showing separately the latitudinal distribution of the deposition of 10Be produced for each of the 6 tropospheric zonal bands, as well as for that produced within the full stratosphere. Like the residence times commented on above, it is unclear how this is quantified from the model output, is it a trajectory type analysis, are particles tagged based on their production location?

Line 5: It is not clear how the latitude of cross-tropopause transport is deduced, as Figure 10 only shows the latitudinal distribution of deposition.

Line 7: this appears to be approximately true for the NH, but not so much for the SH.

Line 8: It is also important that the 60-90 latitude bin has the smallest area, so the total number of atoms produced there is small just for this reason?

Line 9: should be clear, you mean the 10Be *produced* in the mid-latitude and tropical troposphere.

Line 11: really all latitudes, or over full hemisphere?

Line 14-23: This paragraph is quite hard to understand. Sentences need to be made clearer, containing the quantities actually compared. For example, “The T42L31 and

T42L39 are similar except. . .” should at the very least be “Results from the T42L31 and T42L39 model configurations are similar, except. . .”, as long as whatever “results” are being considered are well described in the first sentence of the paragraph.

Line 24: Again, the description of the quantities plotted in figure 11 is unsatisfactory.

Line 26: Again, the models agree that the 10Be produced in the stratosphere is deposited between 30-50deg, but the actual locations of stratosphere-to-troposphere transport is only a guess.

Line 27: It is not clear what “modulated” is meant in this context, and also it seems that the portion of deposited 10Be produced in the NH polar region is comparable in magnitude to that produced in the 30-60 degree band, at least for T63L47, so it’s not clear why the polar fraction is referred to as “remarkably small”.

Pg 18544

Line 24: model resolution dependent.

Line 24: one can’t mix a production rate.

Line 26: “while the different model resolutions” or configurations

Line 27: “the number of observations is too limited to. . .” or else it sounds like the number of models is too limited.

Pg 18545

Line 19: mixing and transport paths are not assessed (see general comments). Rather, one could say you assessed the latitudinal distribution of the deposition of 10Be produced in different regions of the atmosphere.

Pg 18546 Like the text in the Results section, the summary of the “transport” results here in the conclusions is very hard to understand.

Line 8: Again, no diagnostic shown directly assesses atmospheric mixing.

Line 10: a very subjective and vague final statement – hopefully some quantitative diagnostics will help make some firmer conclusions!

References

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