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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Meridional transport and deposition of atmospheric ^{10}Be" *by* U. Heikkilä et al.

U. Heikkilä et al.

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We would like to thank the reviewers for many valuable comments which helped us to improve our manuscript. See below for more specific comments.

Anonymous Referee 1

1. A more detailed comparison of the modeled deposition fluxes with current data is missing. This is addressed in only one sentence on page 16831 but appears to be essential to see how representative the model results are for the real world. This has been addressed in a little more detail in a previous paper (Heikkilä et al., ACP, 2008) but I would urge the authors to include those and other data in this study (e.g. in Table 2 and Fig.2). Most important in this respect appear to be 10Be fluxes from ice core studies such as the data by Stanzick, 1996,

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cited in the precursor paper. Similar data also exist for Dronning Maud Land, Antarctica, which should be available to the authors.

In the previous paper (Heikkilä et al., ACP 2008) we compared the model results with all possible Be-7 data we were able to find (surface air concentrations, deposition fluxes and high altitude concentrations). Unfortunately not much Be-10 data exists which is why we compared modeled and measured Be-7. Since the run which was used for the comparison is the same which is used as "control run" in the present manuscript we could only repeat exactly the same comparison as in the previous paper. We will however discuss the model performance in more detail in the revised version of the manuscript.

Unfortunately, the Dronning Maud Land data starts only at 1800 AD and therefore is not suited for a comparison with a present day run. Comparison with the fluxes from ice core studies by Stanzick, 1996 as well as comparison of modeled and measured fluxes in GRIP ice core at Summit (Heikkilä et al., GRL, 2008) reveal a good agreement in Greenland. To our knowlegde there is not much present day Be-10 data available from Antarctica. We included a comparison with modeled Be-10 deposition flux (average 1986-1990: 150 at/m2/s) and fluxes approximated from measured snow pit concentrations at Law Dome (coarse approximation, average 2001: 100 at/m2/s), Antarctica (Pedro et al., JGR, 2006).

2. I personally miss a more detailed description of the aerosol deposition scheme in the model section as well as the potential influence of this scheme on the results. The relative contribution of wet versus dry deposition becomes essential for the interpretation of 10Be records in low precipitation areas such as the polar ice sheets. E.g. the differences of the results in this study and the study by Field et al., 2006 may be related to either atmospheric transport or wet and dry deposition en route. For the comparison between the two different runs within this paper this seems not so essential because the relative contribution of the two deposition effects remain the same for an unchanged climate. Maybe,

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this should be stated explicitly.

We did not describe the aerosol deposition scheme because it is explicitly described in the previous paper (Heikkilä et al., ACP, 2008) and has not changed. However we now discuss it shortly in relation with its possible influence on the results.

The issue of wet vs. dry deposition is an important one in dry regions. In Greenland the precipitation rate is relatively high and the model suggests that the fraction of wet deposition is > 90%. In some areas of Antarctica the precipitation rates are extremely low and the dry deposition process becomes more important. Studies suggest that in the driest areas the fraction of dry deposition is around 60% (Pourchet et al, 1983 and Raisbeck and Yiou, 1985). The model suggests that the fraction is between 40% and 65% which is slightly lower but still in agreement with the estimates.

However, because the dry deposition becomes important only at very high southern latitudes it cannot be the reason for the differences between the GISS ModelE (Field et al. 2006) and ECHAM5-HAM (this study) because Field et al, 2006 find the polar enhancement at latitudes higher than 40-50 degrees in both hemispheres. Therefore the difference between the models must be related to atmospheric transport. We now include this discussion into the manuscript.

3. In addition, I think it would be instructive to plot the modeled latitudinal distribution of the stratosphere-troposphere 10Be flux together with the deposition at the surface to illustrate the effect of tropospheric transport more clearly. The spatial distribution of the stratosphere-troposphere exchange is shortly mentioned in the Introduction but not further quantified in the Results.

Plotting the latitudinal dependence of the stratosphere-troposphere flux is unfortunately not possible because it cannot be extracted from the model. We can only mark the Be-10 atoms produced in the stratosphere and follow where they are deposited. This is shown in Fig. 4 by the "stratosphere" flux. It can be seen that Be-10 originating from the stratosphere is mostly deposited between 30 and 60 degrees where the intrusions

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of stratospheric air are strongest. Figure 4 also sheds some light on the effect of tropospheric transport, see the fractions of Be-10 produced in the troposphere (0-30, 30-60 and 60-90). The effect of the tropospheric transport is small because the Be-10 atoms are mostly deposited at the same latitudes where they are produced. This can be explained by the relatively short tropospheric residence time of Be-10 atoms. Only Be-10 atoms produced in the tropics have a significant chance to be transported to higher latitudes (30-50 degrees), especially in the Laschamp experiment. The atoms which are produced above the cloud level most probably raise into the stratosphere by the Brewer-Dobson circulation and return into the troposphere by the stratosphere-troposphere exchange in the mid-latitudes.

Apart from these more structural comments, I disagree with the statement made in the paper that the stratosphere would be well mixed in this model. Table 2 shows that 10Be produced in the northern stratosphere essentially does not reach the southern polar region and vice versa. This suggests that the 10Be produced in the stratosphere may be well mixed within each hemisphere but that the interhemispheric stratospheric exchange appears to be slow (typically on the order of 4 years) compared to the stratospheric-tropospheric exchange in each hemisphere. This should be addressed in the revised version of the manuscript.

The referee is right that the Be-10 produced in the stratosphere is well mixed within each hemisphere but not interhemispherically. We apologise for misleading the readers. Because the production structure of Be-10 can be assumed to be symmetric within the hemispheres due to the dipolar structure of the geomagnetic field the mixing within one hemisphere is sufficient to mask the latitudinal dependence of the Be-10 production.

Specific comments

Model description Please add more details on the aerosol deposition scheme.

Included.

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Page 16824 line 18: are 5 years spin-up time really enough when the interhemispheric mixing time of the stratosphere is of similar length?

As mentioned above, we erroneously talk about interhemispheric mixing of the stratosphere although we mean mixing within each hemisphere. This is corrected in the revised version of the manuscript. 5 years seems to be enough for the model to spin up because the average residence time of Be-10 atoms in the stratosphere is in the order of 1-2 years.

Results Here a model-data comparison would be important to assess the credibility of the model results.

We included a comparison with Be-10 data from Law Dome, Antarctica. We were not able to find more present day Be-10 data.

Page 16826 line 17: is the higher change in polar latitudes something like a polar enhancement? Please discuss.

The higher change in polar latitudes cannot be connected to "polar enhancement" because the polar enhancement should be positive and visible at all latitudes from 60 to 90 degrees in both hemispheres. The difference found in this study is positive in the northern and negative in the southern hemisphere, shows no regular pattern similar to the production rate of Be-10, and exists only at some latitudes. If we compare the latitudes where the higher difference takes place (lowermost figure) with the deposition fluxes (two upper figures) we see that the difference in percent is higher where the absolute fluxes are very small. A fluctuation from an absolutely smaller value accounts for a greater difference in percent. We would connect these fluctuations to numerical fluctuations of the model.

This discussion is now included into the manuscript.

Page 16831: here a more detailed data-model comparison would be in place

We included a more detailed description of the model performance based on the pre-

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vious paper.

Page 16832 line 2 (and Table 2 and 3): why does the differentiation for production in different latitudes in the stratosphere only exist for the Laschamp run? If in any way possible this differentiation would be helpful for the control run too and could be added to Table 2 and 3.

This would indeed be very interesting and important. Unfortunately we used a "control run" made earlier and could not repeat it.

Page 16833 line 6: the statement that the stratosphere is well mixed (between hemispheres) seems to be in contradiction with Table 2 (see comment above). Is such a slow interstratospheric exchange a problem for the short spin-up time of the model?

We refer to the earlier comments on this issue. We mislead the readers by using an erroneous term "stratospheric mixing" instead of mixing within each hemisphere.

Interactive comment on "Meridional transport and deposition of atmospheric 10Be" by U. Heikkilä et al. Anonymous Referee 2

There are a number of results that suggest to be that the "weather" in the two models runs is not the same. For instance, the little downward spike in the trop/strat production ratios (60S) in Figure 1,

This is a misunderstanding. Those downward spikes in the figure are just lines indicating which curve corresponds to the "stratosphere" and which to the "troposphere". The smooth thicker lines show the real curves. We have removed these misleading lines.

the "noise" seen in fig 2c and the negative regions in Fig 3. These are almost certainly not the result of the smooth production changes imposed (which increase production everywhere!). Since 10Be is a passive tracer, the governing equations are simply a advection/mixing and removal - all of which are linear in the concentration. Therefore the net effect of all of these processes on the

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smooth change in production must necessarily be to smooth it further and not introduce any decreases. Therefore indications of un-smooth behaviour indicate that the weather component is different. For many results this is unimportant, but in determining the PEC or looking for systematic issues related solely to production change, it is important that the weather related variability (up to 25% according to the authors - though I'm not sure what this refers to exactly) is removed. I strongly recommend that the one or other (or both) of the experiments be rerun with the check that they do have exactly the same meteorological path. This should not be too onerous (these are just 10 year AGCM experiments), especially since I forewarned the authors that this would be necessary in my preliminary assessment of this paper. Without this step, the conclusions about the existence of the PEC or the influence of production changes will remain vague and unconvincing.

Both runs were performed using exactly the same sea surface temperatures and sea ice cover of the AMIP2 project to force the model which is a commonly used procedure. We compared temperatures and precipitation rates of these 2 model runs and they were different by less than 1%. A proof for the "constant" climate between the experiments is the fact that the deposition distribution of Be-10 remains the same. If the climate was different in both experiments the deposition should have changed significantly.

The noise seen in Figure 2c and negative regions in Figure 3 can be explained by the fact that the solar modulation function Phi was kept constant (700 MeV) in the Laschamp experiment but varied monthly according to the observed values in the control run (700 MeV in the average over 1986-1990). The difference is very small as can be seen in Figure 1, the production rate pole-wards of 60 deg is slightly higher in the Laschamp run but the difference is in the order of 5%. If we compare this with the global mean production raise of 100% the difference can be considered negligible.

In the abstract and the main text the authors claim that the stratospheric pro-

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duction is "well-mixed", by which they appear to mean that the local deposition change can be calculated assuming a stratospheric mean production change. This is simply incorrect as can be seen from the "stratospheric fraction" numbers in Table 2.

We have erroneously not made clear that we meant mixing within each hemisphere of the stratosphere instead of interhemispheric mixing. We apologise for this error and corrected it in the revised version of the manuscript.

If the stratosphere were well mixed and the production independent of latitude (as in the Laschamp experiment), then the fraction of 10Be originating from each latitudinal sector of the stratosphere would simply be proportional to the area of that sector. The areas of 90-60, 60-30 and 0-30 SH sectors are 6.7%, 18.4% and 25% of the global area (and similarly for the other hemisphere). I think that the even fractions shown in the last column of fig 8 may have confused the authors since these are not equal-area sectors. Indeed, the statement on p16833, para 1 implying that this result implies a "well-mixed" stratosphere is a fundamental error. Taking the South Pole case for an example, a truly well-mixed stratosphere (with 69% of the total production) would show stratospheric fractions of 5%, 13% 17%, 17%, 13%, 5% for each 30 deg sector respectively (north to south). Instead the values are 0, 2, 6, 17, 28, 16%. Very different! Even if one assumed that only each hemisphere was well mixed, distributing the 61% of the SH strat production, would give 31, 22 and 8% for 0-30 S, 30-60S and 60-90S, again, dissimilar to the actual results. In fact, contrary to the claims in the paper, the local deposition (at SP) is weighted by 0.55, 1.3 and 2.0 for each sector (thinking just about the hemispheric production) or 0.9, 1.2 and 1.7 (for the globe taking account of cross-hemispheric transport). That should allow the authors to estimate a PEC given the changes in 10Be by latitude for a solar or geomagnetic change (which won't be identically equal to 0).

We are aware of these latitudinal fractions and the model grid boxes were weighted by

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their area. However, we must not forget the fact that the height of the tropopause is not constant, but varies between 8-10 km in polar regions and 12-16 km in the tropics. This changes the fractions significantly and resolves the pretended discrepancy. The tropopause is very low in polar regions where the maximum of the Be-10 production takes place. Hence the large production changes at high latitudes take place mainly in the stratosphere with correspondingly long residence times.

It is a little odd that the authors positively state that their results are similar to Mazaud et al for Vostock (from which those authors derive a PEC) and then claim that there is no PEC in this work. I don't see how that can be squared. Since this is such a strong component of the paper's conclusion, and in fact the key bottom-line that will be useful to people trying to interpret 10Be records, I cannot recommend publication in the current form. A re-working is clearly necessary.

We come to the same conclusion as Mazaud et al. 1994 that 75% of Be-10 deposited at Vostok must come from lower latitudes (< 60 deg). They did not distinguish between the stratospheric and the tropospheric production and therefore derived the PEC.

technical comments:

p16825: "The fraction ...." - neither 1.7 nor 1.9 are the fraction of stratospheric production. If they are the ratios, then the fraction would be 63% and 66% respectively.

We agree and corrected it in the revised version of the manuscript.

table 2. It would be clearer if the latitudinal regions were ordered north-to-south (or vice versa).

We also corrected this in the revised version.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 16819, 2008.