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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.

**Anonymous Referee #2**

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This paper is focused on an interesting issue in the interpretation of 10Be records in ice cores. It follows in the footsteps of Field et al 2006 and the authors own previous paper in assessing the importance of climate/weather and production on measured ratios.

General comments:

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, 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



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concentration. Therefore the net effect of all of these processes on the 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.

In the abstract and the main text the authors claim that the stratospheric production 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. 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,



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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).

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.

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.

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

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