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Comment

Interactive comment on “Modeling cosmogenic radionuclides ^{10}Be and ^7Be during the Maunder Minimum using the ECHAM5-HAM General Circulation Model” by U. Heikkilä et al.

Anonymous Referee #1

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1. GENERAL COMMENTS

The Maunder Minimum (MM), 1645–1715, is the most pronounced of several cold spells between about 1450 and 1890 that are collectively known as the Little Ice Age (LIA). During the MM there was almost a complete absence of sunspots, suggesting solar variability as the climate forcing agent. Solar variability is independently suggested by an increase of about 30% in mean global production rate of ^{10}Be , derived from measurements of ^{10}Be concentrations in ice cores in Greenland and Antarctica. This paper reports the results of a modeling study that addresses to what extent the observed increases of ^{10}Be concentrations in the ice cores could be due to climate (transport)

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changes rather than production rate (solar) variations. This is a very interesting and important question to address. The paper is well written and I recommend it for publication in ACP after the following comments are addressed.

2. SPECIFIC COMMENTS

a. ABILITY OF THE MODEL TO REPRODUCE PRESENT-DAY (PD) REGIONAL CLIMATE.

- Since this paper concerns regional climate change, it is important to establish the capability of the model to reproduce regional climate realistically. For the PD, the comparison of the model with observation is shown in Figs.2-6. These comparisons show reasonable agreement in overall magnitudes and seasonal variations. However, since the size of the 10Be variation between the PD and MM is only 30%, some more detailed clarifications are required in the text. First, I accept that an overall normalisation discrepancy is not particularly important when considering *relative* changes of the MM with respect to PD (which could, for example, be simply accommodated by a 10Be production cross section error). However further comments/clarifications are required on the following aspects:

- In Figs.3 and 4, the impressive seasonal agreement between observation and model probably simply primarily reflects the seasonal variation of precipitation. If so, this should be pointed out.

- In Fig.5, the observed PD 10Be deposition flux in Greenland varies by up to a factor 1.5 with respect to latitudinal band. Why is this variation not reproduced by the model? If we are to trust the model to interpret a factor 1.3 variation (MM/PD) in 10Be deposition flux at a given Greenland ice core location, then we need to be convinced this could not in part be generated simply by a small latitudinal shift of the present-day pattern.

- Fig.6 shows the model underestimates 10Be stratospheric concentrations by up to a

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factor 5. Why, and what are the consequences?

b. ABILITY OF THE MODEL TO REPRODUCE MAUNDER MINIMUM (MM) REGIONAL CLIMATE

- Borehole temperature measurements suggest that the global average temperature during the MM may have been about 0.7°C cooler than the mid twentieth century, before appreciable anthropogenic CO₂ emissions. The simulation appears to insert a manual reduction of sea surface temperatures to simulate the MM, along with a reduction of the solar constant by 1.5 W/m². The latter corresponds to about 0.26 W/m² at the top of the atmosphere. Assuming a climate sensitivity of 0.7 K/Wm⁻², this would produce a temperature change of about 0.2°C, which is insufficient to generate the cool MM climate.

Does the GCM used in the present study show a 0.7°C cooler mean global climate during the MM? This number should be quoted in the paper. If the model produces only a modest cooling then how does this qualify the conclusions of the paper?

- In fact, current estimates of the reduction of solar irradiance during the MM are substantially (about a factor 3) smaller than those used in the present simulation. So this model - and moreover, no model, since the detailed climate forcing mechanism of the MM is unknown - does not include a realistic physical mechanism to generate the MM climate. This point should be made clear in the paper. If the forcing mechanism is not known then this fundamentally underpins the level of confidence attached to the conclusions.

- There is good evidence to suggest that the Inter Tropical Convergence Zone (ITCZ) shifted southwards during the LIA, on a global scale. This implies a significant movement of the atmospheric circulation cells in the tropics, which could easily have caused a corresponding rearrangement at higher latitudes, possibly affecting circulation patterns over Greenland. The flux of ¹⁰Be can vary by a factor 10 at a given latitude depending on the relative amount of wet and dry deposition. Since there is a large

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contrast of precipitation across PD Greenland (a factor 10 or so), it follows that a small change of circulation patterns could have a large effect (large compared with 30%) on the ^{10}Be deposition flux. Even layer-counting may not avoid the uncertainty in the wet/dry transport ratio, when converting ^{10}Be concentration into ^{10}Be flux, since there may be climatic variations in the rate of sublimation of snow after deposition. Does the GCM used in this study show such a shift of the ITCZ? What uncertainty does this introduce on the overall conclusion of the paper?

c. COMPARISON WITH FIELD ET AL. (JGR 111, D15107, 2006) AND THE ^{14}C RECORD

Despite the above reservations - which could apply to *any* GCM study of the MM climate - I would like to stress the importance of modeling studies such as the one reported here. These models provide important insights on the climate processes involved in transporting ^{10}Be and similar radionuclides to their archives. I expect the models will ultimately provide reliable results with a high level of confidence. However, for the reasons indicated above, I feel that this study (and that of Field et al.) should not be considered as definitive. Indeed, Field et al., using the ECHAM5-HAM GCM, reach the opposite conclusion of the present paper, which perhaps illustrates the uncertainties that exist in the conclusions of both papers. The limitations of the present study should be made more explicit in the paper in order to avoid a busy reader from drawing unreasonably firm conclusions.

So who is right: this paper, which finds a relatively small climatic influence on the ^{10}Be deposition fluxes during the MM, or else Field et al., which finds large climate effects - comparable to the production changes? The most convincing argument is the experimental comparison of ^{10}Be and ^{14}C production between the MM and PD, which supports the conclusions of the present paper. Carbon-14 has a completely different transport mechanism (it rapidly oxidises to $^{14}\text{CO}_2$ and then enters the well-mixed carbon cycle). The good agreement between the ^{10}Be -derived and ^{14}C -derived measurements of the production changes indicates that the ^{10}Be ice cores reliably measure

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GCR changes since the MM. The present authors have in previous papers stressed the good agreement between ^{10}Be and ^{14}C measurements during the Holocene, and this should be pointed out again in the present paper. This, finally, is the most convincing argument that there was a true production increase of around 30% during the MM.

3. TECHNICAL CORRECTIONS

a. Fig.1 y scale: The text defines the "Standardised units" of the y axis as "All records have been standardized to a mean value of 1." However, none of the records in Fig.1 has a mean of 1. Furthermore, the excursions of the curves span about 4 units range. Please clarify the y scale and origin of Fig.1.

b. Fig.6 x axis, ^{10}Be : Please replace the "500" at the x origin of the right hand plot with "0".

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 15341, 2007.

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