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Climate of the Past Discussions

Interactive comment on "High-resolution records of the beryllium-10 solar activity proxy in ice from Law Dome, East Antarctica: measurement, reproducibility and principal trends" *by* J. B. Pedro et al.

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Authors' Response to Referee #2 (W. Webber)

We are grateful to Prof. Webber for his careful and constructive review of the manuscript. Comments by the referee are labelled (R) and followed by the authors' response (A). Text that has been added to the manuscript in response to the referee comments is emphasised below using italic script. All citations here can be found in the references section of the manuscript.

R: With regard to the references: There is at least one work that also studies monthly averages of 10Be concentration over an earlier time period 10 years (Beer, et al., Atmospheric Environment, Vol. 25A, No. 5/6, pp. 899-904, 1991) that is not referenced but should be.

A: We now cite this work in the introduction of the revised manuscript. In addition, we include several new sentences in the Section 3.4.2 which compare our results with this work and another high resolution GRIP snow pit. The new sentences are as follows:

We are not aware of any monthly-resolved 10Be records from other Antarctic sites with which to compare the DSS composite. However, there are at least two such records available from Greenland. The first is from a snow pit spanning 1978 to 1983 from the Dye 3 site in south Greenland (Beer et al., 1991). The second is from a snow pit spanning 1986 to 1990 from the GRIP site in central Greenland (Heikkila et al 2008). Similarly to DSS, both Greenland sites experience short-lived concentration maxima. An important difference between the Antarctic and Greenland records is that the concentration maximum typically occurs during the (austral) summer to autumn in Antarctica whereas in Greenland there is suggestion of a bimodal pattern with a primary maximum in the (boreal) summer to autumn and a secondary maximum in the (boreal) spring.

R: It is important to recognize that the annual wave of 10Be concentrations as determined from the monthly averages factors into how the yearly averages are calculated for historical studies. And this annual wave is larger than the year to year changes, and is quite variable in its amplitude and time of maximum, all contributing to an uncertainty in the yearly averages.

A: This is a valid point; however we include a similar statement already in the manuscript (pp 696 line 20, copied below) and therefore do not alter the text.

The authors concluded that the that the sporadic peaks were likely caused by intrusions of 10Be rich air from the stratosphere, they suggested further that such a mechanism

may explain relative peaks in ice core 10Be records. Our results appear to support this view.

R: With regard to the Pearson correlation coefficient between 10Be concentration and N.M. data: Although it is rarely calculated by those obtaining the 10Be data and using it for historical studies, it has been calculated for several of the earlier 10Be concentration measurements for the last 50 years or so also using neutron monitor data as a reference for atmospheric production. These calculations give correlation coefficients 0.3 and should be referenced as a background to the values found in the current paper. This reference is Webber and Higbie, 2010, http://arxiv.org/abs/103.4989

A: The paper mentioned in this comment is not published in the peer reviewed literature and therefore, unless the editor advises otherwise, we are not comfortable to reference it in the manuscript.

R: Page 692, line 27: I think reproducibility is a better word than veracity.

A: In hindsight we agree. The term 'veracity' was used twice in the manuscript. In both instances it has been replaced it with 'reproducibility'.

R: Pages 694-695: The question of the time delay between 10Be production and its sequestration is important and is addressed by the data in this paper. All of the indications, as noted on these two pages, seem to favour a very short residence time of only a few months or less yet the authors seem unwilling to fully commit themselves to this possibility, e.g., stratospheric or tropospheric production.

A: Our interpretation of the data is that most 10Be deposited to DSS has been produced in the atmosphere within the previous 0 to 10 months. This interpretation is informed by the lag correlation analyses shown in Fig. 5c; the analyses calculates Pearson's correlation coefficient (r_{xy}) between the monthly-resolved neutron counting rate (which is proportional to atmospheric production) and the monthly-resolved ice core 10Be concentration. Our assumption is that the correlation coefficient will reach

a maximum when the lag approaches the average time between production of 10Be in the atmosphere and deposition to the ice sheet (i.e. when the lag approaches the atmospheric residence time). Fig. 5c shows that r_{xy} reaches a stable maximum for lags of between 0 and 10 months. Therefore, we cannot support short residence times 'of only a few months or less'; the 95% CIs around r_{xy} in Fig. 5c demonstrate that within the 0 to 10 month range the r_{xy} values for atmospheric residence times of 0 to 3 months are statistically indistinguishable from those for 4 to 10 months. In addition, and as noted in the manuscript (pp 695 lines 5-8) our value for the residence time is supported by general circulation models (Heikkilä et al., 2008a) and 10Be:7Be ratio-based measurement of air mass ages in the stratosphere and troposphere (Raisbeck et al., 1981 and Jordan et al., 2003). For these reasons we do not alter the text in response to this comment.

R: Pages 695-698: Another point that the authors say too little about is the fact that the NM intensity continues to increase throughout 2008-2009, whereas the 10Be concentration decreases. This lack of tracking means that the most important aspect of solar cycle #23, namely the unusually high cosmic ray intensities and low solar modulation, (e.g., McDonald, et al, 2010; Mewaldt, et al., 2010) has actually been missed to a large extent by the 10Be concentration measurements reported in this paper! This does not bode well for the use of 10Be measurements in a historical sense to study solar activity through the amount of cosmic ray modulation. Could it be that there are systematic biases in the extraction of the 10Be concentration from the most recent snow layers? It seems that some further comments are in order here.

A: This is indeed an important result in our work. We have not been able to pinpoint any single factor responsible for the divergence between neutron counting rates and 10Be concentrations after 2008. Webber questions whether there may be systematic biases in extraction of 10Be from the most recent snow layers. Since $\sim$97% of the total 10Be flux to Law Dome is wet deposited (Smith et al., 2000), the concentration in snow is essentially fixed at the time of deposition; i.e. it does not change with

depth. Furthermore, the technique for extraction of 10Be from meltwater is the same for samples from the surface layers and for samples from deeper layers. We cannot think of any reason why there would be a systematic bias.

We now include some new text (Sect. 3.4) that comments specifically on our assessment of the possible reasons for the divergence:

Since the procedure for extraction of 10Be from melt-water was identical for all of the records used in the composite, chemical processing factors do not explain the divergence. This leaves meteorological influences or local/in-situ effects (e.g. local variations in snow accumulation, snow surface topography, ablation and other forms of surface disturbance) as the most probable explanation.

There is some overlap in this comment with remarks from Reviewer 3, who asked whether relation with d18O may explain the divergence. We include some new text addressing this point (Sect. 3.7, see also response to Reviewer 3). In brief the relation with d18O cannot explain the divergence. We have also included some new text in the conclusion which to give greater recognition to this issue [inserted text in square brackets].

The atmospheric production rate is responsible for the principal trend on inter-annual timescales. [The relation appears to break down during 2008 to 2009 when neutron counting rates increase to unusually high levels whilst 10Be concentrations actually decrease. In our assessment this divergence must be caused by some unexplained meteorological influence or local/in-situ processes (e.g. local variations in snow accumulation, snow surface topography, ablation or other forms of surface disturbance).]

Currently we are undertaking research using the ECHAM5-HAM general circulation model (e.g. Heikkila et al., 2008a) and results from Langrangian particle dispersion model FLEXPART (Stohl and Sodemann, 2010) that we hope may shed light on this subject, however this next step is outside the scope of the present work.

Interactive comment on Clim. Past Discuss., 7, 677, 2011.