

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.

W. Webber (Referee)

bwebber@nmsu.edu

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This is basically a very good article. It is strong in two areas where previous articles presenting new ^{10}Be records (then used for historical projections of ^{10}Be concentrations and their relation to solar activity and cosmic ray intensity changes through the changes in the ^{10}Be atmospheric production function) are traditionally weak.

The first area is the analysis of the ^{10}Be data itself, the errors involved in the extraction, measurements, reproducibility, dating and indeed the quality of the ice-core site itself.

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These aspects are investigated more carefully and thoroughly than in most previous articles, so that one has a higher level of confidence in the accuracy and reproducibility of the final result.

The second area concerns the mathematically rigorous correlation of the measured ^{10}Be concentrations with well established direct records of atmospheric production of ^{10}Be such as neutron monitor data. Again previous papers presenting ^{10}Be concentration measurements which are then used for historical studies have almost uniformly tacitly “assumed” a “high” degree correlation, equivalent to a (1 to 1) linear correlation between ^{10}Be production and concentration.

The Pearson’s correlation coefficient of 0.64 that the authors find for their ^{10}Be data vs. N.M. is really not all that high but at least it is better than a correlation coefficient ≤ 0.50 which implies almost no correlation and that another parameter, different from production, is responsible for most of the ^{10}Be concentration changes.

I would recommend publication of this article in essentially the form it is in now. However, there are a few omissions (references) and other points that really need a response from the authors. I will leave it to the editor to decide if the responses are adequate.

With regard to the references: There is at least one work that also studies monthly averages of ^{10}Be 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. It is important to recognize that the annual wave of ^{10}Be 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.

With regard to the Pearson correlation coefficient between ^{10}Be concentration and N.M. data: Although it is rarely calculated by those obtaining the ^{10}Be data and using it for

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historical studies, it has been calculated for several of the earlier ^{10}Be 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>

Other comments:

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

Pages 694-695: The question of the time delay between ^{10}Be 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 favor 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.

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 ^{10}Be 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 ^{10}Be concentration measurements reported in this paper! This does not bode well for the use of ^{10}Be 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 ^{10}Be concentration from the most recent snow layers? It seems that some further comments are in order here.

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