

This paper is an extremely detailed look at annually resolved records of stable water isotopes and snow accumulation from the NEEM site. The authors compare the data with all manner of reconstructions, climate indices and model outputs. I am impressed with the very thorough job that has been done, and happy that the many uncertainties in the analysis are carefully explained. The downside of the thoroughness is that the paper is extremely hard to read – almost more like an uncensored thesis than a paper at times - and I wish the authors had been a little more willing to exclude material that was not central to their story. However, what is here is careful and provides the best assessment of the relationship of Greenland oxygen isotopes to climate that I have seen. The most important result is a potential recalibration of warm temperature isotopes at the NEEM site, which would imply a lower change in temperature in the last interglacial. I think some of the implications of that might have been expanded. I also suggest some material that I feel could be cut or at least moved to an appendix to make the paper more digestible. With these relatively minor changes the paper should certainly be published and will be an important contribution to the literature (indeed almost required reading for anyone impressed with the NEEM 2013 paper).

Detailed comments

In a few places the English will benefit from proofreading at the TC stage. I don't list all instances of awkward phrasings but as examples, page 658 line 18 "constraints" not "constraint", and line 19 "the NEEM last interglacial" (missing "the").

Section 2.2. Page 662, line 23. Do the Box reconstructions include NEEM ice core data? If so, isn't this comparison circular? If not (which at least may be the case for his 2009 paper) then you should clarify this.

Page 663, line 26 "again"? Do you mean "against"?

Page 664, line 2: MARv3.4/ERA is repeated twice.

Section 3.1, page 665. Here you show that there are significant differences between the 4 cores (R^2 only 0.31 for $\delta^{18}O$ between cores, and similar for accumulation rate). The noise is reduced by averaging 4 cores. Later on when you do regressions of NEEM $\delta^{18}O$ (or accumulation) against other climate indices and model outputs, it's obvious that even if the NEEM region could perfectly record one of the indices, one would not get an R^2 of 1 because of the remaining noise. It would be helpful context if you could tell us what the "ideal" R^2 would be, ie what is the theoretical best R^2 one could obtain from a signal with the amount of noise in the 4-core average added to it.

Same section: you show here that the deuterium excess signal at annual level is insufficiently precise. I therefore question the value of describing the results of comparisons with other measures in Table 4, Figure 10 and section 4.2. At the very least these comparisons need to come with a very strong health warning that good correlations cannot be expected as the averaged d-xs record has little signal content at the annual level (I am not sure if you are saying that the S/N is 0.4 for an individual or for the averaged signal, but either way S/N is less than 1). I realise that the authors do not want to abandon dxs, but it would make the paper easier to read if it was put to one side and discussed in a brief section separate from the more robust signals, and with many caveats.

Page 667, line 2. I note that the authors used R^2 until now, but in most of the rest of the paper use R . I request that they point this out, because readers may fail to appreciate how little of the variance is explained for R values of 0.4 and below.

Page 667, line 4 “possibly...accumulation”. I don’t follow this statement because you can easily test this statement by presenting the slope for other time periods.

Page 671, section 3.4, 1st para. Please rephrase: I don’t understand what point you are making in lines 10 and 11.

Section 3.4 (but this is a common problem throughout the paper): you are giving lists of correlation coefficients (eg with AMO). These are already listed in the tables, and reading the text becomes a bit like reading a telephone directory. Please rely more on the table and on statements about what exhibits a strong correlation, and be a bit more restrained about citing all the numbers again in the text. That way the important science conclusion will be clearer to the reader.

Section 4.1. Please refer to Table 2 earlier in the paragraph.

Page 67, line 9, should be Table 4 not 3 However I think this is overinterpreting the noisy dxs data and should probably be reduced and toned down.

Section 4.3. As far as I can tell, you compare model and composite temperatures against NEEM delta. However I don’t see the comparison of model temperature against model del18O which would set a limit on what can be expected, and is an obvious comparison to make. Please include this.

I find it interesting and surprising that the R value for modelT vs NEEMdel18O is similar to that of model-del18O vs NEEM del18O. I’d have expected a weaker relationship for temperature.
Comment?

Section 4.5.2. I recommend removing this entire section. It is out of place in the paper, and ascribes too much weight to single years of data that may be extreme because of noise rather than signal. The end of the section presumably brings in Fig S2 (though does not refer to it) and again seems completely out of place here. I think if you want to follow the effect of volcanoes or these apparently extreme years it is a different paper. Here it just comes as a surprise and a distraction from the main themes.

Page 684, line 28. This is incorrect. At a site with quite high accumulation rate, most of the aerosol will be wet deposited and therefore the dependence of concentration on accumulation rate will be quite weak (if 70% is wet deposited, which might be the correct order, then a 30% increase in accumulation rate would decrease chemical concentrations by only 7%, which would be hard to pick against other effects in a world with a different climate and therefore perhaps different atmospheric circulation). I doubt it will be strong enough to diagnose rates.

Table 6, please be clear what “NEEM temperature reconstruction” is. Does that mean the “Box” reconstruction at the location of NEEM?

Fig 3. For the power spectra please help the reader by showing the periods as well as the frequencies. Also the caption should refer to (b)-(d) not just (b).

Fig 4. I assume you mean top and bottom, not left and right?

Fig. 6. The order of b and c seems to be different from what the caption says.

Fig. 12: I think these are differences between 1979 and 2011, rather than trends (which should be per year or per decade). Please clarify.

Fig. 14 caption refers to “a function of the month” but this seems to be missing from the figure.

Many figures are missing units on the axes. In most cases this is dealt with by putting them in the caption (not very nice, but acceptable), but even this is missing in Fig 8, please add.

For all the figures, please remember that most readers will read them on a printout of the paper. Please try to persuade the typesetters to make some of the figures larger (eg 3b-d, 6) as they are unuseable as they print at present. It would also be nice to see larger axis labels in many cases.

Section 4.6. This is a really important part of the paper. I think I agree with your best estimate of the temperature difference at the Eemian based on your paper (though I am a little unsure whether we also need to use a higher ΔT slope for the upstream corrections made in the NEEM paper). In any case, I think it would be valuable to say a little more about the implications. In particular something like: “Ice sheet modelling experiments constrained by evidence for the existence of Eemian ice have suggested that Greenland contributed 1.4-4.3 m sea level equivalent, with the implication that this was the Greenland retreat expected for an 8 degree warming. This would be hard to reconcile with the finding that the threshold for irreversible loss of all or part of the Greenland ice sheet is well below 8 degrees for Greenland temperature. If the actual temperature change in Greenland during the Eemian was only 4 degrees, these results are reconciled, and the response of Greenland to higher temperatures expected under some scenarios was not tested at that time.” (Of course you will choose your own words, but I do feel you need to comment more).