Clim. Past Discuss.,
doi:10.5194/cp-2016-28-RC2, 2016

Climate of the Past Discussions
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EGU

Interactive comment on "How warm was Greenland during the last interglacial period?" *by* Amaelle Landais et al.

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Received and published: 8 May 2016

The suggestion that Greenland could have been 8 degrees warmer in the last interglacial, and yet the ice sheet survived, is indeed a paradox. It is really important to find other ways to tackle this. A recent paper (Masson-Delmotte et al., 2015) involving some of the same authors as this one concluded that the temperature change may only have been about half that, illustrating that it is very hard to pin down the magnitude of the temperature change based on the water isotopes. It is therefore very important to find alternative ways to assess the temperature change that occurred. This paper takes a new approach, of using 15N data, with the assumption that these tell us the thickness of the firn at the time, and that this is controlled by temperature and snow accumulation rate. By making a range of assumptions about the accumulation rate (either as a function of temperature, or just as an independent adjustable), the authors

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Discussion paper

attempt to put limits on the possible temperature rise.

This is clearly an important problem and it is very worthwhile to try this approach. However, the authors reach a conclusion that I don't think their data justify, especially if one includes a further estimate of the accumulation rate, which I would rate as being at least as valid (perhaps more so) compared to the ones they choose. The critical part of the paper is Figure 3. I would make a first comment that, given how crucial this figure is, it's extremely hard to follow. I'll follow this up later. If I accept all the accumulation estimates shown (but I don't, see below), I reach the conclusion of the authors in section 3.2.3, but not the overall conclusion of the paper. This section seems to conclude:

For Summit, the data can probably not be interpreted as a pure thermal signal; For NEEM, you seem to choose a range between the +20% accumulation (4.5 degrees warming), and the "M-D" approach (∼8 degrees warming). The authors don't explicitly state how they corrected for the upstream issue but I assume from Table 2 that they added 2.5 degrees. Thus the range they estimate is 7-10.5 degrees. For NGRIP, the best estimate is 3.2+/-0.7 degrees, with no significant upstream correction, and a possible extra 0.5 degrees for the warmest part of the LIG.

So taken at face value, one would conclude that one site gives 7-10.5 degrees and the other gives 2.5-4 degrees, which does not seem like a basis for asserting that the climate was 8 degrees warmer and that the NEEM paradox is confirmed.

However, what Figure 3 really tells us is that there is no good way of estimating the accumulation rate in the LIG, and therefore in the end we can't constrain the temperature this way. I can suggest another very valid way to estimate the accumulation rate. In Kapsner et al (1995), which the authors cite, an accumulation rate-temperature estimate for the early Holocene (pre-Boreal, which seems the most relevant) is made from correlation of annual layer thicknesses with oxygen isotope ratio measurements. They find a rather shallow slope within climate periods such as the pre-Boreal. Because they actually did a regression of accumulation against oxygen isotopes, we can in fact estimate directly (without going through temperature) what should be the acc rate change for an oxygen isotope change of 3.5 permil (Table 2, change at deposition site). Given that they report a slope of 0.9%/K, and they used a calibration of 0.53 permil/K, their acc-isotope slope must have been 1.7%/permil, and a 3.6 permil change would correspond to a 6% change in accumulation rate. This would suggest a 3.5 degree warmer firn column, which adding the 2.5 degrees upstream correction, implies the NEEM region was 6 degrees warmer than present. Of course there are many reasons why this estimate may also be wrong. However, as a temporal slope, it is probably the most evidence-based estimate of all those presented, and should quite definitely be part of the range that is considered. Taking it together with the 4 degree estimate for NGRIP (which would also be reduced by assuming only a 6% change in accumulation, presumably to about 3 degrees), it does not suggest that the 8 degree conclusion is most likely right, but rather that it is probably too high, with values of 3-6 degrees more likely, pending more definitive estimates of accumulation rate. I know that I am playing Devil's advocate for a low value here, because really the evidence about acc rate is weak, but anyway I do not think the current conclusion of the paper (that it reinforces the paradox) is sustainable.

In the rest of this review I will go through the paper in more detail, and comment some more on Figure 3. However my overall suggestion is that, taking into account another realistic way of calculating accumulation rate, and the NGRIP result, the conclusions and abstract should be much more balanced and should not claim to be confirming the very high estimate of NEEM Project Members.

The abstract needs recasting in light of the NGRIP result and the more considered estimate of the NEEM accumulation rate that I am proposing.

Page 3, line 5 "rules out stratigraphic disturbance within the segment" – add this because clearly there is disturbance in the core as a whole.

Page 4, line 13. Is it worth here pointing out that the relationship most likely depends on changes in sea ice among other factors?

Page 5, line 27. You don't actually say it but I assume there really is a visible ice layer at this depth? I am not sure I see exactly how an event of the sort you imagine would affect 15N – after all a significant shrinkage of the firn must affect significantly the porosity, so it is not really clear what the net effect on the ability of 15N to diffuse should be, but since this is not crucial to the paper, I think there is no need to go further.

Section 3.2. While I think it is reasonable to assume that, in Greenland, 15N data under interglacial conditions should conform to a firn model with specified temperature and accumulation rate, one should add the small caveat that, for central East Antarctic sites, it has been rather conclusively shown that the firn models don't work correctly.

Section 3.2.1. As I already suggested, you really only show in this section that we have no good basis for estimating the accumulation rate that applies to this warm interglacial. Here I just comment on some of the estimates you give: i) The Kapsner paper quite conclusively shows that the thermodynamic approach is not really applicable to a situation like that in Greenland, where most of the precipitation is related to cyclones and storm activity. It's fine to mention it as an option, but it is clearly flawed. When it comes to figure 3, I really don't understand what the grey shaded area is meant to represent, so if you leave it, it needs a better explanation. ii) It seems as if this Benson approach is actually an empirical spatial version of the thermodynamic approach? However, while I could almost justify this approach for single storms tracked (and their integrated effect over a year) spatially across Greenland, there seems no basis for translating that into the temporal domain, as you acknowledge on Page 8, line 22. Note also that Benson 1962 seems missing from the reference list. iii) Same applies for the Buchhardt approach, but since the blue lines are missing from the plot I cannot assess it. I understand why you prefer the sensitivity Buchardt derived for the NW region but don't understand why you give a range for Greenland of 6.7-9.6%/K, when Buchardt's table gives a value of only 1.5%/K (with an error bar encompassing zero) for the nearby NE region, and even a negative slope for SW Greenland. I do not think you have summarised the Buchhardt outcome fairly; in my opinion it gives weak evidence for a 6.7%/K slope, but with an uncertainty that encompasses a zero slope. Note also that it is unnecessary to draw a curve to represent Buchardt; he actually derived an acc rate-del18O slope, so for a measured change in del18O, you can pinpoint precisely the range of the change in acc rate and draw them as horizontal lines . iv) I have not checked the MD approach specifically, but note that this is the same paper that derived a much larger del18O-temperature slope and therefore a smaller temperature change than NEEM Project Members, so it seems inconsistent to use the same model simulations to derive the opposite result. v) I have not checked the 10Be estimates, but note only that because deposition in Greenland under interglacial conditions is completely dominated by wet deposition, it seems very unlikely that one can derive accumulation rates from 10Be. I agree with your implication that the change in overall scavenging would have to be accounted for in a sophisticated model, and I don't believe that local accumulation rate would be the dominant control on 10Be concentration or flux. For the same reason, I do not understand at all your statement on Page 9, lines 22-24 that changes in aerosol rule out no change in accumulation: if you want to make such a statement it needs considerably more explanation and analysis, as there seems no basis to estimate the changes in sources well enough to make such a statement. vi) It seems that GCMs agree with a small change in accumulation rate.

Taking all these estimates together with the one I derived from Kapsner et al, and noting that they all have significant weaknesses, I see no reason to rule out a very small change in accumulation rate, which is suggested by Kapsner et al, approach vi, and the lower range of the Buchardt approach when you include their NE data. It's unfortunate but it means you simply can't constrain accumulation rate this way.

Page 14, line 28. I don't see the relevance of this: at current mean summer temperatures we know we can get melt layers (as we did this year). You certainly don't need the summer mean to be 5 degrees warmer than present to expect significant melt every year.

Fig 1. Please remove the age scale for the shaded portion beyond 128 ka, as we have no basis for it. Summer insolation should also be cut at 128 ka as it cannot be compared to the climate curves below it beyond that age.

Figure 3. Please make this figure clearer in the caption. I suggest something like: “The black circles. . ..NEEM. The light curves are contours of accumulation rate-temperature combinations for a given value if del15N, with that value shown in the curve. The coloured examples correspond to the measured LIG del15N for GRIP (blue), NGRIP (purple) and NEEM (red). The horizontal lines and darker curves correspond to different estimates of the accumulation rate (for the curves, this is as a function of temperature).” Then having explained the overall point of the diagram, you can explain each curve, but of course I would hope that the Buchardt lines will be shown and will include a wider range than in the text, that the Kapsner line is added, and that the vertical arrows better reflect the range of credible estimates.

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