

Response to the reviewers

We thank both reviewers for their positive reviews and constructive comments. We have revised the manuscript in order to take their suggestions into account. Please find below detailed answers.

Reviewer 1 (Eric Wolff)

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.

We are sorry for this. The choice was to have a comprehensive analysis inside one single (but long) manuscript.

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.

We have expanded the implications of the results in this section.

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).

We have done our best to take these suggestions into account.

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").

Taken into account.

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.

The Box (2009) temperature reconstruction does not include ice core data (they are

used in the accumulation reconstruction). This has been clarified in the section where data are compared with the reconstruction.

Page 663, line 26 “again”? Do you mean “against”?

Corrected.

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

Corrected.

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.

As stressed by the reviewer, there is only 30% of variance in common to two records of oxygen 18 or accumulation from nearby ice cores, which may arise from noise due to the analyses and dating as well as noise from deposition and post-deposition processes.

The percentage of common variance between the 4-core stack and temperature reconstructions or simulations of temperature is quite similar (<50%). In this case, mismatches may arise from the signal to noise ratio in the 4-ice core stack, but it can also arise from the caveats and biases inherent to the atmospheric models (due to their resolution and their parameterizations) and the large scale wind patterns from reanalyses.

It is therefore not possible to answer to the question in a quantitative way with the available information.

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 δx_s 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 δx_s , 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.

We have taken this suggestion into account and added a strong warning about the validity of the ice core deuterium excess stack at each place.

In the data description part, we had already clearly written :

“The lack of strong signals in recent deuterium excess is surprising, as one could have expected a relationship with recent changes in Arctic sea ice cover (Kurita, 2011; Steen-Larsen et al., 2013). It could arise from the low signal to noise ratio.”

« In the subsequent parts of this manuscript, we will therefore be cautious not to over-interpret this NEEM deuterium-excess record”

We have added in the analysis of correlation with SST :

“Deuterium excess is negatively related to SST, with a weak correlation coefficient which may arise from the low signal to noise level in our dataset”

and in the discussion of the link with weather regimes :

“ despite its low signal to noise ratio, deuterium excess is significantly correlated...”.

We have also warning statements in the data-model comparison part, where we had already written:

“We have already stressed the weak signal-to-noise ratio within the individual NEEM shallow ice core records (with a core-to-core correlation of 0.25).”

and now add a final sentence in 4.2: “Our conclusions are limited by the large inter-core deviations and the low signal to noise ratio in the stack signal. »

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.

We have in fact used R^2 (determination coefficient) when reporting signal to noise ratios in section 3.1 (inter-core data) and 3.2 (comparison with other Greenland ice core records), but R (correlation coefficients) after section 3.3 (comparison with meteorological data, simulations, climate indices...) as we needed to report positive or negative correlation coefficients. The text and table captions are very clear about the metrics that are used. We agree that the percentage of variance explained is often small, and have added this precision at the beginning of section 3.3 :

« In this section and the following parts of this manuscript, we systematically report correlation coefficients (R) and not determination coefficients (R^2) as results of statistical analyses, to inform as well about the sign of the relationship ».

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.

Thank you for pointing this out. We have calculated the accumulation-180 slope for the whole record, prior to the recent increasing trend. The relationship is less strong

($R=0.5$) and with a smaller slope (1.4 cm cm per year per ‰) than for the last decades. We have therefore removed the last statement.

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

We have reformulated this sentence to clarify what we mean:

« The statistical relationship between the NEEM and South Greenland ice cores may therefore arise from this simultaneous impact of the NAO on both regions »

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.

In this section, we have removed the correlation coefficients to improve the readability of the text.

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

Taken into account.

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.

Taken into account. We have added several reminders about the noise level in the deuterium excess record.

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 del180 which would set a limit on what can be expected, and is an obvious comparison to make. Please include this.

This is a very good suggestion. Note that Table 6 already reported the 180-temperature relationship obtained from multi-decadal trends in the simulations.

At the inter-annual scale and for the simulations nudged to ERA, ECHAM produces a slope of 0.8 ‰ per °C, and a correlation coefficient of 0.79; LMDZ produces a slope of 0.5‰ per °C, and a correlation coefficient of 0.59 (mostly due to differences in extreme years for each parameter).

For 1979-2007, it is also illustrative to compare the two simulations (ECHAM-ERA and LMDZ-ERA). Their annual mean results are closely correlated for temperature (correlation coefficient of 0.95) and slightly less for 180 (correlation coefficient of 0.83).

One paragraph about these results has been added in the revised manuscript.

I find it interesting and surprising that the R value for modelT vs NEEMdel180 is similar to that of model-del180 vs NEEM del180. I'd have expected a weaker relationship for temperature. Comment?

We have highlighted this result in the revised manuscript. This probably arises from the relationship between precipitation isotopic composition and surface air temperature in the simulations themselves.

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.

We have better structured this section into 3 different parts (extreme years, cold-dry decades, and reponse to volcanic events) to clarify its purpose. We think that these three aspects are relevant for the implication of the NEEM ice core records for recent climate variability in north west Greenland.

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.

Thank you. We have removed the statement.

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

This has been clarified in the figure caption.

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).

The caption has been modified. We have not added the periods which can easily be inferred from the frequencies as peaks coincide with periodicities of 0.05 (20 years), 0.2 (5 years) and are reported in the main text.

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

The caption has been modified (this arised from a re-arrangement of the panels in the edited version).

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

The caption has been modified (this arised from a re-arrangement of the panels in the edited version).

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

The panel display the temperature change from 1979 to 2011 (°C) calculated from a linear trend (not the difference between the start and end years). The caption has been corrected to clarify this.

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.

This comment is for Figure 13. In fact, the angle of the polar diagram of Panel (a) represents the month, and this is clearly labelled on the outside of the polar graph (Jan ... Dec).

We have added the unit (‰) in the caption of Figure 8. Only in the case of water stable isotopes (oxygen 18 or deuterium excess), we did not report the units on the axes.

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 fo 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.

We will ask the editor to improve the readability of the figures in the final proofs.

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 delta-T 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).

Thank you for this suggestion. While we initially tried to avoid speculation, we have added a paragraph to clarify the implications of a reduced last interglacial warming at NEEM.

Anonymous referee # 2

This is a high-quality analysis of some newly-available north-west Greenland ice-core and climate data, that overall builds significantly on previous work and which will be of interest to a wide readership. There are a few points of clarification and missing references. I recommend acceptance once the following points have been addressed:

Thank you. We have addressed all the points and provide detailed explanations below.

p.659, line 9: "strong relationship between surface vapour d18O and local humidity, and surface air temperature" - rephrase as slightly confusing as from units the relationship meant seems to be between dO18 and temperature (i.e. 2/3 factors, not directly humidity)?

Taken into account. The sentence has been cut into two parts for clarification.

p.659, line 27 To the 4 references cited on the strong NAO imprint and Greenland climate please add the following: Hanna & Cappelen (2003) Hanna, E. and Cappelen, J. (2003). Recent cooling in coastal southern Greenland and relation with the North Atlantic Oscillation. *Geophysical Research Letters*, 30(3), 1132. doi:10.1029/2002GL015797

Done.

p.662, line 12 reword to "therefore decreases with depth".

Done.

p.662, line 18, line 21 "especially for summer temperature". Add reference. E.g. a comparison of summer near-surface air temperatures for various coastal and inland Greenland sites was made by Hanna et al. (2014): Hanna, E., Fettweis, X., Mernild, S. H., Cappelen, J., Ribergaard, M. H., Shuman, C. A., Steffen, K., Wood, L. and Mote, T. L. (2014), Atmospheric and oceanic climate forcing of the exceptional Greenland ice sheet surface melt in summer 2012. *Int. J. Climatol.*, 34: 1022–1037. doi: 10.1002/joc.3743

Done.

p.662, line 26 "NAO defined as the standardised difference in sea level pressures between Gibraltar and Iceland (Vinther et al. 2003)" - it would be better here to give the original Gibraltar-Iceland NAO reference, i.e. Jones et al. (1997): Jones, P.D., Jónsson, T. and Wheeler, D., 1997: Extension to the North Atlantic Oscillation using early instrumental pressure observations from Gibraltar and South-West Iceland. *Int. J.*

Climatol. 17, 1433-1450.

Done. This reference has been added.

p.663, line 18 "water stable isotopes" - doesn't this technical term need brief explanation?

We have briefly expanded this sentence.

p.665, line 10: why does this analysis end in 2007 and not a more recent year, given the rapid recent climate changes and extremes?

This is because of the change in the number of source records (number of individual pit and shallow ice core data) for the most recent years, and the availability of annual accumulation data only up to 2007. We have added this explanation in the revised manuscript.

p.66, line 9: "The highest d_{18O} annual mean value is however encountered in 1928" - why? Was this an unusually warm year or was it just the near-record accumulation alone that was responsible?

We discuss this aspect in section 4.5.2 and have added a reference to the subsequent discussion here.

p.667, line 28: change "albeit not" to "although such values are not".

Done.

p.670, line 5: why not compare with other region (e.g. DMI coastal met station) Greenland temperature records other than just the SW Greenland temperature series?

The correlation between data from the first shallow ice core at NEEM and DMI coastal meteorological station data had already been performed for the last 50 years in Steen-Larsen et al (2011), showing a weak relationship with summer temperature at Illulissat. With our stack record, and longer time series, the strongest correlation coefficients also emerge with the composite south-west Greenland record, which explains this focus in our manuscript.

p.671, line 21: clarify whether these correlations " $R > 0.3$ " are statistically significant.

We only report in the main text the correlations which are significant, and the result of significance tests at 95% and 99% confidence levels are displayed in the associated Table S4.

p.672, line 1 re. weather patterns and Scandinavian Blocking, I haven't seen Greenland Blocking explicitly mentioned in this discussion but I think it is important and worth mentioning.

Thanks for this suggestion. We have tested the correlation coefficient between NEEM records and Greenland blocking. We obtain significant correlations with 180 (R=0.30) and accumulation data (R=0.26). This is now reported in this section and also in the conclusions.

p.673, line 11 "MAR precipitation is slightly larger" - is this statistically significant? 13-29% seems as if it MAY be quite a substantial difference.

We have removed « slightly » (as the difference is statistically significance at the 95% confidence level).

p.674, lines 4 & 5: Again, give significance/p values for these R values.

The p-values are reported in Table 3.

p.676, line 16: Is there any difference in variance between ERA40 and ERA-I for the overlap period?

We have not performed this comparison, as we only have used the nudged simulations, and not compared the reanalyses themselves.

p.677, line 21: "Greenland warming since 1979 is strongly driven by changes in large scale atmospheric circulation (Fettweis et al., 2013a)" - please add the following references: Hanna, E., Fettweis, X., Mernild, S. H., Cappelen, J., Ribergaard, M. H., Shuman, C. A., Steffen, K., Wood, L. and Mote, T. L. (2014), Atmospheric and oceanic climate forcing of the exceptional Greenland ice sheet surface melt in summer 2012. *Int. J. Climatol.*, 34: 1022–1037. doi: 10.1002/joc.3743 Hanna, E., Jones, J. M., Cappelen, J., Mernild, S. H., Wood, L., Steffen, K. and Huybrechts, P. (2013), The influence of North Atlantic atmospheric and oceanic forcing effects on 1900–2010 Greenland summer climate and ice melt/runoff. *Int. J. Climatol.*, 33: 862–880. doi: 10.1002/joc.3475

Done.

p.678, line 16: clarify whether you mean "local surface AIR temperature changes".

Done.

p.679, lines 21-24: the accumulation sensitivity to Greenland temperature also depends importantly on dynamical/storm-track changes - should point this out here.

Done.

p.679, line 25 "We therefore identify unusually strong responses of both d018 and accumulation to local temperature increase, over the decades." Does this seem to suggest changes in moisture-bearing storm tracks impinging more on this part of Greenland? Should probably comment on this.

We have added the following statement : “Further investigations of moisture transport changes are needed to explore the processes at play, such as changes in storm tracks associated with sea-ice retreat in the Baffin Bay area”.

p.680, line 4 "extreme years" - don't these also include 2012 - mention here?

Here, we focus on the extreme years recorded in our NEEM ice core datasets (which end in 2007 for accumulation and 2011 for 180), so we cannot discuss 2012 in the perspective of earlier years within the ice core records.

p.680, line 25: AWSs, not just ice-core records, can also be used to map recent warming.

This has been added.

p.681, Section 4.5.2: shouldn't this include more direct discussion of 2012?

See above.

p.682, lines 3-13: what about Greenland Blocking?

See above.

p.686, line 29: add Hanna et al. (2014) (full reference above) to Fettweis et al. (2013a).

Done.