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Climate of the Past Discussions

Interactive comment on "Greenland temperature and precipitation over the last 20,000 years using data assimilation" *by* Jessica A. Badgeley et al.

Anonymous Referee #3

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Review of Badgeley et al. 2020 on Greenland paleo data assimilation

Badgeley et al. present temperature and precipitation fields for the last 20,000 years over Greenland generated using a paleo data-assimilation technique. This is an interesting and potentially very valuable new approach to investigating past climates. The paper is well written and clearly illustrated, and I am generally enthusiastic about the work.

While the methodology represents a big step forward, the paper is also a step backwards in other regards as it assumes a constant linear scaling of d18O to site temperature for all sites and periods based on the spatial d18O-T relationship. This assumption has been disproven in the last 2 decades through careful work in the ice core community (including some of the papers cited here). This assumption will dominate all the

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spatial and temporal patterns in the temperature reconstructions, and deserves more careful consideration than it is given here. The authors suggest that this problem is alleviated by using the precipitation weighted temperature, but they do not demonstrate this. Below I recommend some comparisons that should be done before the paper is suitable for publication.

My main concern is the use of a single linear d18O-T scaling based on the spatial d18O-T pattern at all sites and locations. While water isotopes are a valuable proxy, its temperature interpretation has proved very difficult. Borehole thermometry and d15N gas thermometry are the most reliable methods to get absolute (calibrated) temperature changes, and both suggest a d18O slope that is around half of the spatial relationship (0.67 permil/K) used here (as the authors acknowledge).

I suspect this assumption will lead to underestimated temperature variability in the posterior. The authors should check this for the abrupt transitions at the three sites (GISP2, NEEM, NGRIP) where d15N-based temperature changes are known (Buizert et al. 2014).

However, there is also a clear spatial gradient, as first noted by [Guillevic et al., 2013], a paper that should be cited and discussed. Guillevic observes that d18O changes are largest towards the north (i.e. NEEM), and smaller towards the south (i.e. Summit). However, the actual temperature changes have the opposite gradient – smallest in the north and largest in the south. This means that the d18O-T relationship has an enormous spatial gradient, from $\sim$0.6 at NEEM to $\sim$0.3 at Summit. The Guillevic temperature gradient is seen in many (all?) climate model simulations and should thus be considered very robust.

These patterns are such that when using a single constant slope (as the authors do), the larger temperature changes would appear to be in the north, as is indeed the case in their reconstructions (Fig 4a, 4c). However, the Guillevic result would actually suggest the opposite pattern in temperature. The authors need to plot the magnitude of abrupt climate warming in their reanalysis (either the 14.7 ka or 11.6 ka transition), and compare it to the d15N-based values. My hunch is that they will find the opposite pattern from the Guillevic result.

It has also been documented that the d18O-T slope is strongly variable in time, changing by almost a factor of 2 [Kindler et al., 2014].

It would be unreasonable to ask the authors to redo all the work abandoning a key assumption; rather I think they should do a careful comparison to d15N-based estimates of abrupt climate change to assess how well their method captures both the magnitude and spatial pattern of abrupt temperature changes – and the implications this may have for the LGM and Holocene optimum patterns shown in Fig 4a and 4c. Perhaps they can provide some suggestions for future work on ways to assimilate the d15N-based climate constraints directly.

If the reconstructed N-S temperature gradient during abrupt change is indeed opposite to the Guillevic gradient, this should be clearly stated in the abstract.

The authors suggest that using precipitation-weighted temperatures alleviates the problems associated with using a linear d18O-T scaling. To validate this claim, at the very least they should show a comparison of the 21ka histories of TraCE 2m temperature and TraCE precipitation-weighted temperature at a key site (e.g. Summit), to show how different these two really are. Ideally, they would show more clearly how this impacts the reconstructed magnitude of the abrupt climate change events (that are most strongly constrained by the d15N data).

General comments:

Please describe the data assimilation method in more general terms understandable to the non-initiated, so the reader won't have to track down the Hakim reference. Can we think of the posterior as a cleverly weighted sum of the randomly selected model timesteps put into the prior?

Is there some relationship between the posterior and the 21ka climate simulation – for example, is the posterior solution for the LGM very similar to the TraCE simulation of the LGM? Is the posterior LGM solution strongly weighted towards LGM model years randomly selelcted in the prior?

The TraCE simulation has quire a coarse grid I imagine? Please specify the exact resolution. I imagine it may even put multiple of the ice core sites in a single grid box. Perhaps the grid box resolution could be drawn onto figure 1? It seems that the spatial fields in Fig 4 are much smoother than the model would be. Did you apply smoothing or some other technique?

How meaningful is it to use global climate simulations and constrain them only in Greenland? From a global perspective, Greenland is essentially a single location and the global climate field is not at all constrained. How well-behaved is the far-field response in the reanalysis? And does this somehow impact the reconstruction? I think doing this with global proxy databases (such as [Shakun et al., 2012] would be a great next step (beyond the scope of this paper of course).

Seasonality is very briefly addressed, but it deserves more attention as it is an important climate parameter. Please specifically address seasonality in both the prior and posteriors. Will the reconstructions made available online have T and/or P seasonality in them, and if so, describe how this seasonality is derived. I imagine the seasonality of the posterior can be derived via the assimilation method?

The authors find an unusually late timing of the Holocene optimum around 5ka – much later than other ice-core based estimate from both d18O and melt layers. Looking at Fig 2, it appears that Camp Century (and perhaps Dye 3) are the only cores that suggest such timing, and since the temperature reanalysis is fully determined by ice core d18O, it follows that those two cores must be responsible for this timing (do you agree with this assessment?). However, as pointed out by [Vinther et al., 2009], these sites experience strong thinning in the first half of the Holocene, which will shift their

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apparent climatic optimum towards a later age (as early Holocene climatic warmth is masked by a cooler site temperature at higher elevation). Could the late (5ka) timing of the climatic optimum in your reanalysis be an artifact of the thinning history of the Greenland ice sheet? Please discuss briefly in the text.

The data assimilation is fully dependent upon the accuracy of the TraCE-21 climate model simulation in capturing Greenland climate. Therefore, the paper needs a short evaluation of how well this model actually simulates Greenland T and P in the modern day. The TraCE T and P fields should be compared to modern-day Greenland reconstructions thereof; I would recommend the works by Box et al. on this topic [Box, 2013; Box and Colgan, 2013; Box et al., 2009; Box et al., 2013], but general reanalysis products such as NCEP or ERA5 are suitable also.

All the figures show relative temperature changes and accumulation changes (relative to the reference period, which is not defined as far as I can tell). But when forcing ice sheet models absolute values are needed. Are these absolute values taken from the last time-slice of the TraCE simulations, or is something better used?

Minor comments

L8: What are “independent ice core records”? d18O? Again, I think the reconstructions should be compared during the abrupt temperature transitions at NEEM NGRIP and GISP2, which is where d15N-N2 provides a very robust estimate of the magnitude of change. Those are the truly independent ice core records to compare to.

L24: This is somewhat misleading, because you’ll always need to do such precip corrections unless you are doing a fully coupled ice-climate simulation. As the ice elevation in the ice sheet simulation evolves, it differs from the reference elevation at which the climate field is defined; this needs to be corrected for via clausius-clapeyron or similar. So also with your forcing the ice sheet models will need to apply thermodynamic precip corrections.

L28: Many more d15N studies to cite here: [Guillevic et al., 2013; Kindler et al., 2014; Severinghaus and Brook, 1999; Severinghaus et al., 1998]

L38: “restricted to a single climate model realization”; wouldn’t this critique apply to your study as well? It appears that both use the exact same climate model run.

L70: “measured layer thickness” is not really true. For several cores you use volcanic ties, in which case the layer thicknesses are not measured but inferred

L136: “captures the. . ..” This is in the eye of the beholder. With the exception of the Bolling warming itself the TraCE run matches the abrupt transitions poorly – there is no YD to speak of.

L145: why not use P-E? is evaporation negligible?

L217: “highly correlated” is a strong statement. Do you have a reference? Normally d18O and site temperature are not highly correlated at most sites on observational time scales (< 0.5).

L224: Based on the recent literature, I think that post-depositional alternation may be the largest complication in interpreting the d18O record. Please mention.

L241: Can you plot T_site and Tˆ*_site together for the last 21ka at a key site (e.g. Summit). That will let the reader judge the impact of using T* instead of T.

How is the seasonality of the posterior linked to the seasonality of the prior?

L261: “grid-cell closest to site” is this also done for T, or do you use 2D linear interpolation or similar? Are there cases where multiple cores share a closest grid cell?

L295: maybe a sentence on how this was estimated?

L313-314: But [Dahl-Jensen et al., 1998] estimates it a lot colder at GRIP, more like -22K cooling at the LGM (25ka). This should be mentioned.

L340: Maybe reference [Buchardt et al., 2012] who did very detailed analyses of this.

L416: are other d18O records really independent? They suffer the same biases from seasonality, source effects, etc. For true independence, compare to d15N-N2.

L438: TraCE has no HTM anywhere! (one of its many problems. . .)

L476: This is more of a discussion than a conclusion item. Consider moving it. Also, see my comment above, the 5ka timing could be an artifact of ice sheet elevation changes.

Figure 4: please add panels (e) and (f) with the T and P change over an abrupt transition (e.g. the Bolling onset). In panel (c), only show the cores that actually constrain the LGM (so not Agassiz, camp century and Renland). Why are the field so much smoother than the TraCE CCSM3 model resolution? Baffin bay has a large temp response with no cores to constrain it – can we trust this?

Fig 5: the "noise" in T (i.e. high frequency signals) at all core sites seem strongly correlated. How come? Could it be that the posterior is more or less reflecting the mean d18O of the various sites?

Fig 6: The largest features in the plot are not directly constrained by any cores. Do you trust these?

Figs 7 and 8 are very technical and could be moved to the supplement.

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