Clim. Past Discuss.,
https://doi.org/10.5194/cp-2020-84-RC2, 2020

Climate of the Past Discussions

Interactive comment on "Cold season warming in the North Atlantic during the last 2000 years: Evidence from Southwest Iceland" *by* Nora Richter et al.

Anonymous Referee #2

Received and published: 29 September 2020

Richter et al. reconstruct cold-season temperature trends over the last 2 ky for southwest Iceland using alkenones produced by lacustrine haptophyte algae. The authors demonstrate that alkenones from lake VGHV record a long-term warming trend, as well as decadal to centennial scale variability within the long-term trend. They couple this temperature reconstruction with a lake energy balance model to support that increasing high-latitude winter insolation is likely responsible for the overarching cold-season warming for the last ∼2 ky, while climate perturbations are likely responsible for high frequency variability in proxy data. The authors contextualize this data in a broader framework by suggesting that this dataset, and more studies like it, could help consolidate discrepancies between global climate model output and proxy reconstructions for

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Discussion paper

the northern hemisphere through the Holocene.

Major contributions of this manuscript:

This work offers important insight into seasonal differences in temperature for SW Iceland (and by inference this part of the N Atlantic) for the last 2 ky

The coupling of proxy inference and lake energy model is a progressive approach for interpreting proxy data by testing it within varying climate forcing scenarios

The presentation of the data is thoughtful and clear

Criticisms of the manuscript:

I find the discussion around the seasonality of this proxy, and the conclusions drawn from it, to be somewhat confusing and at times inconsistent. I think it would be helpful if the manuscript more clearly articulated the chain of logic/evidence that provides that alkenones, which are stated to bloom in spring, can be interpreted more broadly as a record of cold-season temperatures driven by cold-season insolation.

Within this point, I would find it helpful if the background discussion around the proxy touched on the fidelity of alkenones for reconstructing temperature (is it known to have significant error associated with it, or low significance values?) and are there calibration data that covers a climatically similar region? I appreciate that this record is being interpreted qualitatively and it is clearly stated in the manuscript that there is no local calibration data, but I think it would improve confidence in this interpretation of the data to know that it has been tested/utilized in comparable locations, particularly in interpreting high frequency changes as related to climate perturbations and not stochastic proxy noise

I would find it valuable to know if there is a competing effect from declining summer insolation/temperature on spring temperatures and the timing of ice-out. It's unclear if JJA/SON is held constant in the model, or if there is little response to ice-out date/water temps given changing temps/insolation during these seasons (Fig. 5).

Is the lake model consistent with observational data for what controls lake ice-out dates & water temps? (i.e. are there examples in modern observational data of earlier ice-off dates in regions with increasing winter air temperatures?)

Some discussion around if the parameters & outputs of this model are climatically probable for the coverage of the record would improve this manuscript. E.g. An air temperature increase in the winter of +7 deg does dramatically move the ice-out date, but that change in temperature seems far too large for the amount of cold-season insolation change. These bounds, I think, are justified in lines 145-157, but I find this statement/constraint confusing. Observed temperatures in any given season can range by +/- 7, but why would average cold-season temperatures range by this amount over the last 2 ky? Change in insolation seems to have much less impact in ice-out dates in the model (Fig 5) but is credited for driving trends in alkenone data. This reads as a mismatch in results-conclusions as written, and the manuscript would improve from some text that consolidates the results of the model with their interpretation of the data.

The warming trend apparent in this data really seems to start just before ∼400 CE, with temperature values only returning to average values from the start of the record several centuries later. This could indicate the long-term trend is less pronounced than what is captured by this window (i.e. there is a rebounding from depressed temperatures, and then warming above that average only over the last millennia). I think it would be an interesting point to add to consider if the early values (0-200 CE) are the anomaly of the record, of if the record should be considered in the context of these early values.

Overall I think this is a significant and important study, but the manuscript would benefit from some additions to background information and from adding text that consolidates what is learned in the model with their proxy data, and the climate implications of these data, so that it is very clear the conclusions are supported by their data prior to publication.

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