

Response to Reviewer 1

Aguilar and co-authors present new lipid biomarker and stable isotope data from the basal sediments of Camp Century in NW Greenland. Along with a meta-analysis of published Greenland paleoclimate records spanning the current and prior interglacials, the authors use these data to frame climate constraints for a smaller-than-present Greenland ice sheet. The rare basal ice sheet sediments and implications for ice sheet climate sensitivity make this a particularly relevant and tantalizing study. Broadly, I found the methods to be appropriate, however, I have comments that I hope the authors can address to expand upon their assumptions and rationale for proxy interpretations (see detailed comments below). Of course, sediments like these come with substantial uncertainties and necessary assumptions, and I think the authors have generally done a good job covering the gamut. Overall, the manuscript is generally well written with nice figures. There is a commendable amount of information and context provided in the main text and supplement, which I think is important for the reader.

We thank the reviewer for the positive and constructive assessment of our manuscript. We appreciate the recognition of the importance and novelty of the Camp Century basal sediment record. In response to the reviewer's detailed comments, we will revise the manuscript to further clarify key assumptions and the rationale for proxy interpretation and comparison frameworks throughout the text.

Comments

L37: Since we don't really know when during MIS 11, perhaps better to say at some point during MIS 11?

We thank the reviewer for this suggestion. We will revise the text to clarify that while the age constraints place the sediments within MIS 11, the precise timing within this interglacial cannot be resolved, and we therefore refer more generally to conditions during MIS 11.

L52: The Plain Language Summary can be removed...I'm assuming this is legacy from a prior manuscript version

We will remove the Plain Language Summary from the manuscript.

L80-81: ...and constrains climate sensitivity, which seems like a major goal of this study? And links to first paragraph

We thank the reviewer for this suggestion. We will revise the introduction to explicitly state that reconstructing temperature and atmospheric circulation during past periods of reduced Greenland Ice Sheet extent provides constraints on Greenland Ice Sheet climate sensitivity, thereby strengthening the link between the study motivation and objectives.

L90: Can you specify the two regions of deepwater formation?

Thank you for this helpful comment. We will revise the text to explicitly identify the Labrador Sea and the Nordic Seas as the two regions of North Atlantic Deep Water Formation.

L94: Please clarify how tropical and high-lat temperature are in step...phasing? Magnitude?

Thank you for pointing this out. We will revise the text to clarify that tropical and high-latitude temperatures became more phase-coupled, exhibiting synchronous glacial–interglacial variability while retaining different amplitudes of temperature change.

L96: temperatures were not the same at the high-lat and tropics during interglacials, which I'm thinking may not be what you intended, but the phrasing of the sentence is currently unclear

We thank the reviewer for this comment. We will revise the sentence to clarify that interglacial temperatures were not identical between the tropics and high latitudes, but rather exhibited broadly synchronous, orbitally paced variability with differing amplitudes between regions.

L127: May be important to also mention McFarlin et al. (2023) here and the alternative interpretations of depleted lake water stable isotopes being changes in methane cycle

Thank you for this suggestion. This is an important point, but one that is peripheral to the introduction section and brief points about Holocene climate being made here. We will modify this sentence to primarily lean on evidence that does not depend on the interpretation of the stable isotope values of leaf waxes.

L130: clarify that this is “summer” warmth

Thank you for this comment. We will clarify that the reconstructed warmth refers to summer conditions, consistent with the seasonality of the paleoclimate proxies discussed in this section.

L148: Would be helpful for the reader to clarify what methods were used to infer this...cosmogenic exposure dating?

Thank you for this comment. We will clarify that this inference is based on cosmogenic nuclide measurements of the bedrock beneath the GISP2 ice core.

L198: For clarity, reference the later section where you justify that these are lake sediments here

Thank you for this comment. We will revise the text referencing Text 3.1. where we discussed the proxy systematics of the biomarkers.

L199-204: I would suggest rephrasing the fractionation factors as “estimated” or “based on compilations”. This is one major source of uncertainty and it will help the reader to not over interpret these as super well constrained

Thank you for this suggestion. We agree that the fractionation factors are not tightly constrained constants and represent estimates derived from compilations of published observations. We will revise the text to clarify this and emphasize their associated uncertainty.

L206: Instead of account, I may suggest using estimate, as similar to my last point there is lots of uncertainty here (for this the temperature estimate too) so important for the language to help reflect this uncertainty

Thank you for this suggestion. We agree that this step involves multiple sources of uncertainty, including the temperature reconstruction, and we will revise the text to use “estimate” instead of “account for” to better reflect this.

L223: Does ice free here refer to Greenland being ENTIRELY ice free? Please clarify.

We thank the reviewer for pointing this out. We will clarify in the manuscript that the 810 ± 60 m elevation corresponds to the fully isostatically adjusted land surface following the removal of the entire Greenland Ice Sheet, rather than implying that all ice was absent from Greenland (i.e., there may have been alpine glaciers in East Greenland).

L231-232: Based on my understanding of the elevation adjustments, ice being present just outside the Camp Century drill site would have a similar elevation to today (530 m). And then the other end of the potential elevation spectrum is 810 m for no ice sheet on Greenland. Therefore, I don't necessarily agree that 810 includes uncertainties for unknowns in ice sheet size, particularly if the ice sheet was not that much smaller. Would it make sense to calculate lapse rate adjustments based on the two extremes (530 and 810 m) instead of just 810? This may more accurately capture the possible ranges of uncertainty.

We thank the reviewer for focusing on these details. We disagree that even if ice was gone from our site, but nearby, the elevation would be similar to today's elevation (530 m above sea level (asl)). In fact, in glacial settings, even as ice is thinning before ice-off, the crust is rebounding. By the time ice is gone, even if nearby, after a millennium or two the crust is usually mostly re-adjusted. This fact, combined with the possibility of elevation being plausibly higher than the fully compensated elevation due to a nearby forebulge, we settled on 810 ± 60 m as most plausible. Sure, the crust could be lower or higher than this. However, we don't think the full range 530-810 is more accurate, as we can have reasonable confidence that the elevation was not that close to 530 m, and much more likely closer to 810 m asl.

We also realized that our wording in the final sentence of this paragraph was confusing and likely contributed to this misunderstanding. By describing a possible elevation range between 530 and 810 m, we unintentionally implied that the present-day bed elevation (530 m asl) was a plausible endmember for the exposed Camp Century land surface. We will revise the text to clarify that 810 ± 60 m represents our best estimate of the exposed land-surface elevation, with the uncertainty reflecting plausible deviations from full isostatic adjustment and the transient evolution of crustal elevation during ice-sheet retreat, rather than a range extending from the present-day bed elevation.

L274: Are you referring to each samples represents an integrated time of centuries to millennia? At first I read this as the resolution between samples so it may help to clarify your intended meaning here.

Thank you for this comment. We will clarify that the intended meaning refers to the temporal integration of each individual sample, rather than sampling resolution. We will revise the text to state that each sample likely integrates climate signals over centuries to millennial timescales.

L323: I very much appreciated this section dedicated to proxy systematics and interpretation, and inclusion of the Iso2k framework

We thank the reviewer for this positive feedback. We are glad that the proxy systematics and interpretation section, including the Iso2k framework, was well received. We also agree that this section is a key component of the manuscript, as it provides an important framework for interpreting the biomarker records.

L341: Is there a reason why the authors choose to refer to GDGT-0 as caldarchaeol? While this is fine and a matter of choice as it is a formal name, it is far more often referred to as GDGT-0 in the literature and may be clearer for the reader to use this terminology

Thank you for this suggestion. We agree that GDGT-0 is more commonly used in the literature and we will revise the manuscript to adopt this terminology for clarity.

L345: For soil comparisons, have the authors considered including the relatively recent dataset from Raberg et al. (2024)? These include a bunch of additional high-latitude sites as well as a compilation of others that don't appear to be included in this paper's compilation

We thank the reviewer for pointing this out. Our soil comparison was based on the compilation presented by Raberg et al. (2024). Upon revisiting the compilation, we identified several additional high-latitude datasets that were not included in our original comparison and have now incorporated them. We will correct several missing references to the original data sources included in the compilation. These additions result in only minor changes to Figures S3 and S4 and will not alter the overall patterns or the conclusions of the study.

L349: There is no reference provided and I couldn't find results provided in the supplement for the BIGMaC source classification? This would be important to include somewhere if the authors wish to retain this statement.

We thank the reviewer for pointing this out. We will include the results of the BIGMaC source classification in the supporting document (Table S3). We will also reference this table in the main text.

L365: I would suggest changing to "likely" absence. Blaga et al. (2009) define values over 2 as indicating anoxic conditions although I don't necessarily think there is strong justification for this. In any case, reducing the certainty in the statement would be best since we don't really know what the threshold is or how that varies across space and time. I agree that values less than 10 probably indicate a lack of reducing conditions.

Thank you for this suggestion. We will revise the text.

L406: Note that Dion-Kirschner et al. (2020) find that terrestrial plants produce up to 30x more plant waxes than aquatic plants

Thank you for pointing this out. We will revise the text to acknowledge that while comparable terrestrial and aquatic leaf wax concentrations have been reported in some high-latitude settings (Hollister et al., 2022), other studies have found substantially greater wax production in terrestrial plants (Dion-Kirschner et al., 2020).

L415: Modern soil water isotopes are rare, so it may be helpful to reference Harning et al. (2024) here as they report that soil water isotopes likely have a summer bias in Iceland. Since I'm not aware of similar studies on Greenland, these data may be transferrable

Thank you for this helpful suggestion. We will incorporate Harning et al. (2024) to provide supporting evidence from Iceland, where soil water isotopes exhibit a strong growing-season bias, and will clarify that this serves as an analog for Arctic environments.

L423: Stating that lake water evaporation is limited, assumes the lake basin is opened. If the lake is hydrologically closed, summer evaporation can significantly alter lake water stable isotopes in high latitude lakes (e.g., Kjellman et al., 2022; Akers et al., 2024; Harning et al., 2024).

L424: Akers et al. (2024) and Cluett and Thomas (2001) discuss the impact of summer evaporation on lakes, particularly in the Camp Century region of Greenland, so they don't really support the statement. I would suggest revising to acknowledge the potential impact of summer lake water evaporation, and including references from prior comment on L423. Since we don't know whether the sediments studied were originally deposited in open or closed lakes, this therefore likely reflects a "minimum" difference between precip and lake water-inferred isotopes

Thank you for these two comments (L423 and L424). We agree that the hydrologic setting of the aquatic biomarker production environment may influence lake water $\delta^2\text{H}$ through evaporative enrichment, particularly in hydrologically closed systems. We will revise the text to acknowledge the potential source of uncertainty and incorporated the suggested references. We will also clarify that our discussion refers to the environment in which the aquatic plant biomarkers were produced, rather than the depositional environment of the Camp Century sediments. Aquatic plant lipids primarily record growing-season surface-water $\delta^2\text{H}$, but this signal may be modified by evaporative enrichment depending on the hydrologic setting of the source water body.

L430: Suggesting changing account to estimate or something similar

Thank you for this suggestion. We will revise the text.

L494: You previously conclude that the biomarkers were deposited in a lacustrine environment...if fluvial, this would require you to also consider soil brGDGT calibrations and substantially revise your interpretative framework for biomarkers. This seems like a major point to address.

Thank you for this important comment. We agree that our previous wording did not clearly distinguish between the depositional environment of the sediments and the source environment of the biomarkers. Units 3–5 are interpreted as fluvial deposits based on their sedimentology (Bierman et al., 2024; Collins et al., 2025), whereas the proxy systematics indicate that the brGDGTs were predominantly derived from nearby lacustrine environments. We will revise the text to clarify this distinction and explain why lacustrine brGDGT calibrations remain the most appropriate for our interpretations.

L520-521: I believe at least some of these lake records are suggested to have been impacted by Early Holocene anoxia...how was this taken into account when deriving comparative temperature anomalies? Were temperatures estimated as maximum or minimums? How reliable are these?

We appreciate this comment and revisited the published Holocene brGDGT records included in our comparison. The studies explicitly evaluated the potential influence of suboxic or anoxic conditions on brGDGT distributions. For most records, there was no evidence that oxygen conditions systematically influenced the temperature proxy, while one record (Lake Rosaea) was interpreted as having a possible, but limited, influence. Overall, the published studies conclude that these records primarily reflect temperature, and we will clarify this point in the manuscript.

L602-603: Could this reflect a cold bias if the Holocene sediments are being impacted by anoxia (e.g., Weber et al., 2018)?

We thank the reviewer for this comment. We carefully re-examined the published Holocene brGDGT records used for comparison. These studies explicitly assessed potential non-thermal influences, including redox conditions where relevant. While some variability in GDGT distributions has been attributed to seasonal production or localized redox conditions in specific lakes, none of the records are interpreted as being substantially biased in their temperature reconstructions by anoxic conditions. We will clarify this point in the manuscript.

L606-607: What about Kusch et al. (2019)? Although this record is impacted by anoxia and not inferred to accurately record Holocene temperature

Thanks for this comment. That's true that there's a brGDGT time series from NE Greenland, but it is not inferred as primarily reflecting temperature. We will clarify the text to state that there are no GDGT-inferred temperature time series from comparable latitudes.

L658-660: This sentence needs a reference

We thank the reviewer for pointing this out. We will revise the text to clarify that the estimate of at least 100 km of additional exposed land is based on the present-day geometry between the Camp Century site and the west coast of Greenland and added a cross-reference to the section discussing the evidence that the site was ice free during biomarker deposition

L737: CC doesn't need to be abbreviated here

We will spell out Camp Century in the revised manuscript.

L738-739: This is a very important point that occurred to me reading the paper, but this is only brought up at the very end. I would encourage the authors to incorporate this line of reasoning from the outset. How does this change your choice to use warmest 2 thousand year comparison? How do those latter averages compare with the full Holocene for instance?

Thank you for this important comment. We agree that the potential for incomplete preservation of interglacial intervals in the Camp Century stratigraphy is a key consideration for interpreting the biomarker record. We will revise the manuscript to introduce this line of reasoning earlier in the discussion framework in Section 2.4.

In particular, we will explicitly note that the Camp Century record likely reflects discontinuous preservation of warm intervals rather than full interglacial means. This motivates our use of millennial-scale averages from Holocene records, and specifically the warmest two millennia between 11.7 and 5.0 ka, as a means of isolating peak Holocene conditions for comparison with preserved warm phases at Camp Century. We will clarify in the text that this approach is intended to ensure a more consistent comparison between peak warm intervals across records rather than full Holocene or interglacial means.

Text S1: At end of second paragraph, MAF precip units should be cm I believe, and not m

Thank you for pointing this out. We will correct the MAF precipitation units in Text S1.

Text S3.1: Clarify if fractional abundances for branched and isos are based on total total or total branched and isos, respectively. I also liked the approach of combining Arctic brGDGT lake temperature calibrations. In addition to the Full set from Raberg et al. (2021), did you also try the Meth set calibration that separate structural differences?

Thank you for pointing this out. We will clarify that the reported fractional abundances are normalized within each lipid class. Specifically, fractional abundances of brGDGTs are calculated relative to the total abundance of branched GDGTs, whereas fractional abundances of isoGDGTs are calculated relative to the total abundance of isoprenoid GDGTs.

Regarding the brGDGT temperature calibrations, we did evaluate the Raberg et al. (2021) Meth set calibration alongside the Raberg et al. (2021) Full calibration and Otiniano et al. (2024) calibrations. The Raberg et al. (2021) Full and Otiniano et al. (2024) calibrations produced very similar downcore temperature profiles and reconstructed temperatures, whereas the Raberg et al. (2021) Meth set calibration yielded substantially different values. The differences appear to be driven by the structural component incorporated in the Meth set calibration, particularly the influence of the fIIIa' fraction in the Camp Century samples. Because the Raberg et al. (2021) Full and Otiniano et al. (2024) calibrations produced internally consistent reconstructions and the latter was developed specifically for high-latitude lacustrine settings, we retained these calibrations for the analyses presented in the manuscript. We will clarify this point in the Supplementary Information.

Text S3.6 (and main text): Were datasets used for comparison recalculated in terms of brGDGT-inferred temp using the combined lake calibration approach used for Camp Century sediments? If not, this would be good to make datasets more comparable.

Thank you for this comment. Yes, all published brGDGT datasets used for comparison were recalculated using the same high-latitude lacustrine calibrations applied to the Camp Century sediments to ensure methodological consistency and facilitate direct comparison among records. This is described in the main text (section 2.3) and in the Supplementary Information (Text S3.6).

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