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 δ^2H 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 δ^2H, 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).

References:

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Harning, D. J., Raberg, J. H., McFarlin, J. M., Axford, Y., Florian, C. R., Ólafsdóttir, K. B., Kopf, S., Sepúlveda, J., Miller, G. H., and Geirsdóttir, Á.: Spatiotemporal variation of modern lake, stream, and soil water isotopes in Iceland, Hydrol. Earth Syst. Sci., 28, 4275-4293, 10.5194/hess-28-4275-2024, 2024.

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Raberg, J. H., Crump, S. E., de Wet, G., Harning, D. J., Miller, G. H., Geirsdóttir, Á., and Sepúlveda, J.: BrGDGT lipids in cold regions reflect summer soil temperature and seasonal soil water chemistry, Geochimica et Cosmochimica Acta, 369, 111-125, https://doi.org/10.1016/j.gca.2024.01.034, 2024.

Raberg, J. H., Harning, D. J., Crump, S. E., de Wet, G., Blumm, A., Kopf, S., Geirsdóttir, Á., Miller, G. H., and Sepúlveda, J.: Revised fractional abundances and warm-season temperatures substantially improve brGDGT calibrations in lake sediments, Biogeosciences, 18, 3579-3603, 10.5194/bg-18-3579-2021, 2021.

Response to Reviewer 2

In this study, Aguilar et al. present new data of subglacial sediments at Camp Century to evaluate climate conditions in northwestern Greenland during MIS11 and the early Pleistocene / Pliocene. Specifically, the authors use lipid biomarkers to reconstruct temperature and hydrogen isotopes in leaf wax to reconstruct precipitation variability. The authors additionally conduct a careful comparison between there results and existing records across Greenland in order to compare the differing interglacial periods.

The methods and results of the study are scientifically sound and provide remarkable evidence of past conditions in Greenland beyond the available paleo-records. This work is a valuable contribution alongside the growing suite of information coming from the Camp Century basal sediment community. In particular, this study improves our understanding of the sensitivity of the Greenland Ice Sheet to climate forcings beyond the Holocene and Last Interglacial. My congratulations to the authors on a well conducted study and exciting new data.

My major comments on the manuscript are focused on the sensitivity of results to the theoretical “modern” temperature and deuterium values at ice-free bedrock for Camp Century. The additional reliance on reanalysis data to provide modern baselines for other records is a challenge but is likely a necessity for this study.

My minor comments on the manuscript are primarily focused on clarity and the balance between the main text and supplement. I feel there is information within the supplement which should be included in the main text, particularly surrounding information about the core (age control, number of samples, etc). Additionally, there are a number of supplemental figures which are referenced before the main text figures and are not presented in the order they are referenced (ie, the first 3 figures are S4, Figure 1, S11). I recommend moving supplemental figures regarding the new data to the main body in order to highlight the exciting new results from this study.

We thank the Reviewer for the constructive evaluation of our manuscript. We appreciate the reviewer's positive assessment of the scientific quality of the study and are grateful for the recognition that our results provide an important contribution toward understanding Greenland Ice Sheet sensitivity during pre-Holocene warm periods. In response to the reviewer's comments, we will revise the manuscript to improve clarity, expand the description of the Camp Century core and its age constraints in the main text, reorganize the Supplementary Information so that the text and figures are presented in the order in which they are cited, and correct the supplementary figure numbering and references throughout the manuscript.

Major Comments

Line 130: Should this be increased seasonality of precipitation rather than periods of greater snowfall? If winters are increasing in accumulation and summers are decreasing, which is the dominant change in net accumulation?

Thank you for this comment. We agree that our original wording could be interpreted as implying greater net snowfall. We will revise the sentence to emphasize changes in precipitation seasonality, characterized by greater winter snowfall and potentially drier summers, rather than implying an overall increase in annual accumulation.

Line 140: For the "pre-Holocene warm periods", do you mean other interglacials? As written, there are a number of other time periods (ie interstadials) that it can reference. If the pre-Holocene warm periods refers to either the LIG, MIS11, or the Plio-Pleistocene, define that on first use.

Thank you for this comment. We will revise the text to refer specifically to pre-Holocene interglacial periods, rather than the broader term "pre-Holocene warm periods," to clarify the intended meaning.

Methods: While the information is available in the supplement and later (Section 4.1), I would recommend adding a brief overview of the basal sediments themselves early within the methods section. Primary information such as the dating method, number of age points, total number of samples, total length of the section, etc, should be included. The rough age estimation for each unit should be included (ie, LIG, MIS11, pre-Holocene interglacials). This will provide context and motivation for the remainder of the methods.

Thank you for this suggestion. We will add an additional subsection (Section 2.1) with a concise overview of the Camp Century basal sediment sequence early in the Methods section to provide clearer context for the study design. This will now include the total sediment thickness, the five lithostratigraphic units, and a summary of the age control framework (luminescence and cosmogenic nuclide dating). We will also explicitly summarize the main age constraints, including the MIS 11 age of Units 3–5 and the older Plio–Pleistocene to Early Pleistocene constraints for Unit 1, while referring to the Supplementary Information for further details on core recovery and sampling procedures.

Line 197: Why is the first supplemental figure and supplemental text referenced Figure S4 and Text S3? They should be ordered as they are introduced in the main text. Additionally, why is the first figure referenced in the supplement? If it is important, it should be referenced in the main text.

We thank the reviewer for pointing this out. We will reorganize and renumber the Supplementary Information so that the supplementary text and figures appear in the order in which they are first cited in the manuscript.

Section 2.2: I recommend explicitly stating the goal of generating an elevation corrected bedrock equivalent temperature early within this section. To check my understanding, after scaling the Pituffik climatology via the lapse rate for an increase in 810m, the result is temperature at the Camp Century basal elevation in the absence of an ice sheet under modern conditions. This is so that an exposed Camp Century basal environment can be compared against MIS11 and older data. Is this correct or am I misinterpreting this section?

Thank you for this helpful suggestion. Our objective was to generate a modern, ice-free bedrock-equivalent baseline for both temperature and precipitation isotope values at the Camp Century site. This allows the reconstructed MIS 11 climate to be compared with the climate that would be expected at the Camp Century land surface under modern boundary conditions in the absence of the Greenland Ice Sheet. We will revise the beginning of Section 2.3 to state this objective explicitly and to clarify that the elevation correction was applied to both temperature and precipitation isotope values.

Section 2.2: How realistic is the assumption that the only factor changing temperature under the loss of the ice sheet is elevation? Could another factor like local albedo or atmospheric circulation be playing a larger role?

We thank the reviewer for this insightful comment. We agree that changes in local albedo, atmospheric circulation, and moisture transport would likely accompany the loss or reduction of the Greenland Ice Sheet and could influence local climate beyond the effect of elevation alone. Our objective here, however, is not to reconstruct the complete climate of an ice-free Camp Century, but rather to establish an elevation-corrected, bedrock-equivalent reference baseline for comparison with the paleoclimate reconstructions. We will revise this section to clarify that the lapse-rate correction accounts only for the first-order effect of elevation.

Section 2.2: While the upper limit is 810m, how impactful is the difference between the current elevation (530m) and the upper limit (810)? In other words, what is the sensitivity of the results to the uncertainty in your elevation-corrected modern baseline beyond the +/- 60m?

We thank the reviewer for this comment. We agree that our original wording could be interpreted as implying that the present-day elevation (530 m) represents a plausible endmember for the exposed Camp Century land surface. This was not our intention. We will revise the text to clarify that once the Camp Century site became ice free, substantial isostatic rebound would already have occurred, making an elevation much closer to the fully compensated value (~810 m) more likely than the present-day bed elevation. We therefore retain 810 ± 60 m as our best estimate of the exposed land surface, with the uncertainty reflecting plausible deviations from full isostatic adjustment and transient crustal response during ice-sheet retreat. The sensitivity of our reconstructed temperature anomalies to the assumed elevation is modest. Even if a substantially larger elevation uncertainty of ±300 m were assumed, the resulting temperature uncertainty would be approximately ±1.37°C. Because this scenario represents a much larger uncertainty than our preferred estimate of ±60 m, and because it does not alter the interpretation that Camp Century experienced substantially warmer conditions than the modern baseline, we retain 810 ± 60 m as our best estimate of the exposed land-surface elevation.

Section 2.2: If you were to use the ERA5 reanalysis at the location of Camp Century and scale it down to 810m, how does that compare to scaling Pituffik up?

We thank the reviewer for this suggestion. We evaluated the potential use of ERA5 directly at the Camp Century location and scaling the climate variables to the reconstructed elevation of 810 m. However, we retained the Pituffik-based approach because Camp Century is located within the Greenland Ice Sheet interior, where reanalysis products have larger uncertainties due to limited observational constraints and challenges in representing high-elevation ice-sheet processes (Chen et al., 2026; Zhang et al., 2021). In contrast, Pituffik has a long-term meteorological record that provides stronger observational constraints for evaluating modern climatological conditions (Hersbach et al., 2020; Zhang et al., 2021).

To account for the elevation difference between Pituffik and Camp Century, we applied lapse-rate corrections derived from ice-free regions. Although modern lapse rates over the ice sheet could differ because of ice-surface processes, ice-free lapse rates are more representative of the conditions expected at Camp Century during past ice-free intervals investigated in this study. Therefore, we consider the Pituffik-based elevation correction to provide a more appropriate estimate of the local climate conditions at Camp Century during ice-free periods than applying modern ERA5 values directly over the present-day ice sheet. Given the uncertainties in ERA5 data above the ice sheet, and the greater relevance of ice-free Pituffik to our study samples, we decided it was not necessary to assess in detail the similarities/differences between using Camp Century vs Pituffik data to do this calculation

Line 257: How many records did you compile? While the text references the supplemental tables, providing a brief overview would be useful within the main text.

Thank you for this helpful suggestion. We will revise the Methods section to include a brief overview of the compiled paleoclimate dataset used for comparison with the Camp Century record. We now explicitly state the number and types of records included for both temperature and precipitation isotope reconstructions, including eight brGDGT records, seven ice-core records, five chironomid records, and six macrofossil records for temperature, and six lake records, one marine record, and seven ice-core records for precipitation isotopes. This summary will be provided in the main text, while full details of each record are retained in the Supplementary Tables.

Line 274-276: Do the biomarkers represent mean climate, ie annual mean, or mean of summer climate, or the mean of various summer conditions? Please specify. Does each sample integrate climate over a few centuries / millennia, or does each unit?

Thank you for this helpful comment. We discuss the proxy seasonality in detail in section 3, and this varies slightly by proxy, so we chose to not discuss the details here as well. We will clarify in the text that the exact temporal integration of individual samples is not well constrained by the available chronology. Instead, we state that the biomarker records likely represent long-term average environmental conditions over extended timescales, rather than short-term climate variability.

Line 291-294: Is there station data from Pituffik to compare against rather than ERA5? It would be useful to establish the relationship between the ERA5 reanalysis and actual observations from your sites (at least Pituffik), as there are biases in reanalysis products outside of heavily observed regions.

We thank the reviewer for this suggestion. We agree that comparison between reanalysis products and local observations provides an important assessment of potential biases. Although a direct station-based comparison was not performed here, ERA5 assimilates available meteorological observations through its data assimilation system and provides a spatially and temporally continuous estimate of modern climatology (Hersbach et al., 2020). Because our study requires a continuous climatology that is also applied consistently across comparison datasets, we retained ERA5 for this analysis.

We acknowledge that reanalysis products may contain biases, particularly in data-sparse regions such as the Greenland Ice Sheet interior, where observational constraints are limited (Chen et al., 2026; Zhang et al., 2021). However, Pituffik is supported by a long-term meteorological record and therefore provides a better-constrained location for comparison than the high-elevation ice-sheet interior.

Line 593-594: What unit/units are you referring to for MIS11?

Thank you for pointing this out. We will revise the text to and state that the MIS 11 sediments are the from Units 3-5.

Line 632: There has been a range of recent work on MIS11 and LIG Greenland extent, in references such as Creel et al. (2026) and Holschuh et al. (2026), which should be included here.

Thank you for this suggestion. We will incorporate the recommended references (Creel et al., 2026; Holschuh et al., 2026) and will revise the text to acknowledge recent observational and synthesis studies that further constrain Greenland Ice Sheet extent during the Last Interglacial and MIS 11.

Line 702: Is the increased contribution of local moisture / increased evapotranspiration the cause of the enrichment in d2H as you proposed in the previous paragraph? If so, I would update the text to reflect that.

Thank you for this helpful suggestion. We will revise the text to state that we interpret the enriched summer water vapor δ^2H values as reflecting an increased contribution of locally recycled moisture driven by enhanced terrestrial evapotranspiration over a more extensive ice-free and vegetated landscape.

Line 713-717: Please clarify this statement. The ice core records will be similarly influenced by seasonal biases in precipitation (for instance, He et al. (2021)) which influence d18Oice records.

We thank the reviewer for this helpful comment. We agree that our original wording oversimplified the interpretation of Greenland ice-core isotope records. We will revise the text to clarify that ice-core $\delta^{118}O$ reflects precipitation-weighted snowfall, with the seasonal weighting varying through time (He et al., 2021), whereas the Camp Century leaf-wax and lake-based isotope records primarily record summer moisture conditions.

Figures

Figure 3: Are the data arranged north to south (in the caption), or north to south within each proxy type? I would recommend plotting north to south, starting with sediments and ending with ice cores, such that there can be a more direct comparison between similar proxies at similar latitudes.

We thank the reviewer for this suggestion. We explored several alternative figure layouts, including a north-to-south arrangement. However, because Figure 3 synthesizes multiple proxy types with different climatic sensitivities, we found that the current organization provides the clearest comparison among records while avoiding a less intuitive mixing of proxies or archives.

Figure 4: Add acronym for Camp Century (CC), as both Camp Century and CC are used in the figure.

Thank you for this suggestion. We will add the acronym of Camp Century in the figure.

Figure S4: "…Greenland using Raberg et al., (2021) (red), and Otiniano et al., (2024) (blue)." Specify the use of calibrations within the caption.

Thank you for this comment. We will revise the figure caption.

Minor / Stylistic Comments:

Line 35: Add a comma following "Greenland" (… Camp Century, northwestern Greenland, to reconstruct…)

Thank you for this. We will review this in the manuscript.

Line 38: Use a sentence break rather than a colon.

Thank you for this. We will review this in the manuscript.

Line 73-75: Phrasing is complex – a possible alternative is "While melt due to increased atmospheric warming may be offset by increased winter accumulation, the elevation-albedo feedback…"

Thank you for this suggestion. We will revise the sentence to improve clarity by simplifying its structure while retaining the original meaning.

Line 91-94: Separate into two sentences following "… carbon cycle", so that the high latitude and mid-to-high latitude connections can be clarified.

Thank you for this suggestion. We will separate the sentence into two sentences to improve readability and clarity.

Line 95-96: Phrasing is unclear and contradicts following sentence. A recommendation is: "… glacial periods became increasingly cold, whereas interglacial conditions remained broadly stable for both the tropics and high-latitudes".

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.

Line 98: MIS acronym is already introduced at line 86.

Thank you for pointing this out. We will remove the redundant definition of the MIS acronym.

Line 97-103: For the comparison between North Atlantic and Eastern Arctic, it would be useful to state which archives these data come from (ie, North Atlantic marine sediments/terrestrial Greenland biomarkers/lake sediments in Eastern Arctic).

We thank the reviewer for this suggestion. We will revise the text to identify the archive types underlying these paleoclimate reconstructions.

Line 125: Swap “Time series of…” for “Reconstructions of…”

Thank you for this. We will review this in the manuscript.

Line 129: Swap “There is therefore…” to “Therefore, there is…”

Thank you for this. We will review this in the manuscript.

Line 441-442: Remove the colon as the list only contains two regions.

Thank you for this. We will review this in the manuscript.

Line 577-580: Clarify this statement or break into multiple sentences.

Thank you for this suggestion. We will revise the text by separating it into multiple sentences to clarify the reasoning linking the biomarker evidence, then constrained depositional age, and the inference that the Camp Century site was ice-free during MIS 11.

Line 753-757: Separate into multiple sentences.

Thank you for this suggestion. We will the original sentence into two sentences to improve clarity while preserving the intended interpretation and supporting evidence.

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