This is a really interesting manuscript that models the thermal status of the now-vanished Laurentide Ice Sheet (LIS) in the northeastern United States and Canada. Model results provide insight into the thermal conditions at the base of the ice sheet which can be compared to both geologic observations of glacial erosion and isotopic/geochemical measurements of proxies such as cosmogenic nuclides.

I am not a modeler so have focused most of my comments on the less technical side of the text although reading the modelling section did bring up several ideas that I have made comments about. Given my lack of expertise in the modelling domain I posed these as questions that hopefully the authors can address because many of the readers of this work may have similar questions.

I attach a marked up PDF from my reading and summarize below what I consider to be the most important points to address in revision of the manuscript. I believe addressing these suggestions will make the manuscript more readable to a broader audience and have tried where possible to offer specific guidance to the authors. If the authors have questions about my review, I encourage them to contact me. Paul Bierman

We would like to thank Paul Bierman for his thorough review of our manuscript, and appreciate your time taken to help improve our manuscript. We have gone through your comments (in PDF) and updated the editorial comments raised and recommended. Because the review was done as PDF comments, we have taken those comments, written them down here with our answers below. We first address the larger points as follows:

1. The manuscript should be more inclusive in its referencing of prior work. Some earlier papers are not yet cited and there are claims made that are without citations. I've made suggestions on the PDF and the authors can find more citations to add.

Thank you for taking the time to point out additional citations. We have noted in our response to comments (in the attached PDF) where these changes have been made following your guidance.

2. The abstract could be much stronger and more informative. There are ambiguous statements and words that leave the reader uncertain. I suggest a significant and careful edit on the abstract after the paper is revised. See specific suggestions on PDF.

Thank you for the help and recommendations to clarify the text in the abstract. You will see in the response to your comments (attached PDF) and the tracked changes, we have made edits to adjust the clarity of the abstract.

3. While adding references, I suggest the authors look more deeply into the geomorphic and soils literature of New England. There are papers relating to preserved saprolite that are relevant as well as to striations at and near the peaks of most ranges suggesting that at some point the ice was indeed warm-based. Similarly there is striation mapping near coast. Adding a more inclusive discussion of the landscape will provide deeper context for their results. It will also illuminate the importance of transience something that must be key for the LIS but is not yet discussed (and should be). I allude to this in a question on the ms about model spin up.

We have updated the text to include a more detailed description of the regional geomorphic features and their role as warm- vs. cold-based ice indicators.

"as requested

"Glacial striations and roche moutonnée found below the summits of the Presidential range in New Hampshire and Mt. Katahdin in Maine indicate the presence of warm-based ice locally (Goldthwait, 1970; Davis, 1978; 1989; Thompson et al., 2002). Similar evidence with the addition of weakly developed soils on the summits of the Presidential Range indicate cover by erosive, warm-based ice (Goldthwait, 1970)."

"Furthermore, a lack of precise age control for many of these features leaves the question of temporal variability unanswered and whether or not the thermal regime was consistent throughout glaciation and deglaciation."

Transience is something we acknowledge in the current manuscript. You will see in track changes and our response to your comments that we have further acknowledged this assumption.

4. The ms currently moves between spatial changes (plan view) and elevation changes but not in a coherent and predictable fashion. The story will be easier to follow and will benefit from more clarity and an organization structure that clearly and separately considers the elevation effect vs latitude effect on basal thermal regime.

We appreciate this comment and the suggestion to reorganize the manuscript to distinguish between spatial and elevational changes. However, we opted to leave the manuscript organized as is since the focus of our work was to evaluate elevational changes solely, consider those for various regions in the NE USA, and not the latitudinal effect on the thermal regime. We do not include any discussion of changes in the basal thermal regime nearer the margins instead focusing on mountainous regions that would have experienced similar ice-coverage histories (i.e., deglaciated around the same time).

5. From an ease of reading perspective, some paragraphs are really long and contain multiple foci. Breaking into shorter paragraphs with topic sentences will make the paper easier for the reader to understand. Similarly, there are places (I have tried to call them out in the text) where wording is inexact and thus could confuse readers. I have done my best in the PDF to suggest places it could be improved for clarity. There are sections of the discussion that read like methods and likely should be moved to the methods section or removed.

   Yes, thank you for the editorial suggestions. Following your guidance, we have inserted page breaks where necessary and moved/removed some text in the discussion. Please see our response to your comments (attached PDF) for point-by-point responses.

6. The long time of model relaxation seems to violate known ages and geologic history of ice in New England. This should be addressed explicitly. See extended note on PDF.

   We think that this point raised has been addressed in our discussion, and the reviewer made a comment to address this point further up in the methods. Rather than move this text (we think it is proper to talk about this assumption in the discussion section), we also added text at the end of the methods section to illustrate the acknowledgement of this shortcoming in our model setup.

7. The precision of elevation boundaries to the nearest meter seems to overstate the actually precision of the modelling and the topographic data. Suggest rounding to at least the 10s if not the hundreds of meters given all of the uncertainties.

   Yes, thanks for the recommendation – we agree. We have updated the precision.

8. I agree strongly with the first reviewer that more direct comparison between the model and geologic/isotopic data sets would be very useful and strengthen the paper.

   We agree that a more robust comparison between the existing geologic/isotopic data would be beneficial. However, for the purposes of this manuscript that may be difficult. Any geochronologic data (e.g., TCN exposure ages) from the high peaks demonstrate nuclide inheritance and provide little benefit in constraining a time component. Additionally, as our model simulations are solely concentrated on the LGM, any geochronologic data younger than the LGM, that indicate warm-based ice conditions, are not yet relevant and would require a transient simulation of the deglaciation. Therefore, geologic/isotopic data are primarily considered spatially as indicators of basal thermal conditions with correlations drawn against modeling results. As this approach is not statistically robust, we have softened some of the language (e.g., "…simulations *broadly* agree with geologic interpretations…") and hope that addresses the reviewers' concerns.

Response to Reviewer comments in PDF (we have taken the pdf comments and placed them here):

- Line 12 Abstract: The abstract needs a careful edit. The language and geographic descriptions are ambiguous and will confuse readers not familiar with the specific prior literature and cosmogenic nuclides.
    - Changes have been made as requested, and we have shortened/edited the abstract.
- Line 16: not clear of what? presume you mean cosmogenic nuclides. this should be made clear to the reader.
    - Change has been made as requested.
- Line 20: which? this is ambiguous
    - Removed 'These'
- Line 21: I would think of that as Long Island, NJ and such but data also suggest summits up ice. Suggest broadening.
    - Broadened by stating 'NE USA'
- Line 25: what region?not well defined. High summits? southernmost extent?
    - Changed to 'NE USA'
- Line 29: I think you mean interpreted from...people interpret data don't.
    - Change has been made as requested.
- Line 30: higher than what? seems like jargon
    - This is a common term in ice sheet modeling, but we removed this to avoid confusion. It is not necessary to our statement here.
- Line 37: data are plural
    - Change has been made as requested.
- Line 42: finer and higher need comparatives - presumably finer and higher than older models?
    - We have updated the text as recommended.
- Line 51: and geographically! that's a big sea level change. I'd make a nod to that.
    - We have updated the text as recommended.
- Line 60: There are several new Halsted papers that would strengthen this list - especially the JQS paper comparing chronometers.

  Halsted, C. T, Bierman, P. R., Shakun, J. D., Davis, P. T., and Corbett, L. B., (2023) A critical re-analysis of constraints on the timing and rate of Laurentide Ice Sheet recession in the northeastern United States, Journal of Quaternary Science. 110.1002/jqs.3563

Also, there is the Corbet NJ terminal moraine area paper with dates to diversify this list and the second Balco paper (CT coast) and a more obscure Drebber paper. I would make this an inclusive rather than selective list of cosmo papers in NE, there are not that many.

Corbett, L. B., Bierman, P. R., Larsen, P., Stone, B. D. and Caffee, M. W. (2017) Cosmogenic nuclide age estimate for Laurentide Ice Sheet recession from the terminal moraine, New Jersey, USA, and constraints on Latest Pleistocene ice sheet history. Quaternary Research. v. 87(3), p. 482-498. doi.org/10.1017/qua.2017.11

Drebber, J., Halsted, C., Corbett, L., Bierman, P., and Caffee, M. (2023) In-situ cosmogenic 10Be dating of Laurentide Ice Sheet retreat from central New England, USA, Geosciences 2023, 13(7), 213 doi.org/10.3390/geosciences13070213

- Thank you for the help and recommendations. We have updated the list.

- Line 71: We replaced 'difficult' with 'uncertain'
- Line 74: I think that you meant the GSAB paper here for dipsticks?

Halsted,C., Bierman,P., Shakun, J., Davis, P. T., Corbett, L., Caffee, M., Hodgdon, T., and Licciardi, J. (2022) Rapid southeastern Laurentide Ice Sheet thinning during the last deglaciation revealed by elevation profiles of in-situ cosmogenic 10Be, GSA Bulletin. doi.org/10.1130/B36463.1

- Yes, thank you for the correction. We have updated this reference.

- Line 81: erosion depth though controls cosmo inheritance it's a combination of time and rate which needs to be disentangled.
  - Does the reviewer suggest changing this text? Erosional depth can be related to erosional patterns, so we would keep the text as is for now – but happy to change if needed.
- Line 93: Break Paragraph, new idea
  - Change has been made as requested.
- Line 103: Break Paragraph, new idea
  - Change has been made as requested.
- Line 103: need to define this for reader...and from a pedantic view, I'd argue that the archive is rock and the proxy that informs us is primarily cosmogenic nuclides but there's also a much older literature on weathering and saprolite preserved in NE which must indicate lack of deep erosion! Worth digging into that.
  - Defined "geologic archives" earlier in the paragraph.

    "*Geologic archives such as glacial erratics, striations, and soil development, as well as isotopic evidence from terrestrial cosmogenic nuclide (TCN) surface exposure ages, can be interpreted to reflect subglacial thermal regimes (i.e., cold- or warm-based ice) in the past.*"
- Line 114: above it's archives - best to be consistent
  - We have changed to 'archives' to be consistent.
- Line 115: I see no need to support - why not evaluate! support seems like a preconceived notion.
  - We have changed 'support' to 'evaluate'
- Line 120: Removed, 'comprising of the states listed in'
- Line 198: please define for reader how you define low and high with a specific metric.
  - We have updated the text to refer to 'gradients' in bedrock topography.
- Line 200: per above comment, best to be consistent...is this the archive of previous pages? geomorphic data is not cosmogenic.
  - Have edited to read 'geologic archives'

- Line 207: but isotope data are not geologic...perhaps geochemical? geologic would be moraine mapping, rock types...
    - We have changed the text to 'geochemical'
- Line 274: I am not a modeler and may be misunderstanding this approach but the LIS did not occupy New England for 20,000 years. Existing data suggest that it crossed over the US border about 30,000 years ago and that the margin was largely north of the border by 13,000. thus is was not in steady state thermally. It may be that I am off base here (in which case please explain why this "relaxation" is valid or that it's impossible to model transience (which is how I read this) but at least there needs be a discussion of what this deviation from the geologic reality might mean for results - some kind of sensitivity test would be great! and really illustrative for the reader.
    - We discuss the limitations in conducting a 'steadystate' simulation of LGM conditions in the Discussion section: see lines 518 and beyond. While it is true that the ice sheet at the LGM may not have been in true steady state, its characteristics likely reflect a more complex history. As the ice advanced southward, ice temperature would have been the result of the accumulating new ice (which would reflect the transient surface climate conditions) and advection of ice from upstream. Therefore, it is difficult to assess how this may influence results. However, our methodology is similar to how ice sheets are initialized currently, and how we assess the basal conditions of the present-day ice sheets (Greenland; see MacGregor et al., 2016 (referenced within)). These reconstructions are taken with the caveat that they may not capture the thermal history exactly as the surface conditions were transiently evolving. Nevertheless, we included text outlining this caveat in our discussion. *We note the reviewer commented positively on this in our discussion, and suggested to move this text to end of this section.

    - Ultimately, we left the text in our discussion and instead have also added some more text at the end of the methods section to address this:

      "This methodology has been applied to study the basal temperature of present-day ice sheets (Seroussi et al., 2013; MacGregor et al., 2016; 2020), and although it assumes the ice sheet is in equilibrium with climate, we acknowledge that the thermal conditions of the LIS during the LGM likely reflect transiently evolving ice geometry and climatic conditions experienced during the growth and advance to the LGM maximum. "

- Line 318: suggest paragraph break here, new idea.
    - We have corrected this as recommended.
- Line 403: really should cite Marsella's very large data set from Baffin that discusses polythermal ice and the extension of that data set by Corbett. See:

  Corbett, L. B., Bierman, P. R. and Davis, P.T. (2016) Glacial history and landscape evolution of southern Cumberland Peninsula, Baffin Island, Canada, constrained by cosmogenic 10Be and 26Al. Geological Society of America Bulletin. v. 128(7-8), p. 1173-1192. doi.org/10.1130/B31402.1

  Marsella, K. A., Bierman, P. R., Davis, P. T. and Caffee, M. W. (2000) Cosmogenic 10Be and 26Al ages for the last glacial maximum, eastern Baffin Island, Arctic Canada. Geological Society of America Bulletin. v. 112(8), p. 1296-1312. doi.org/10.1130/0016-7606(2000)112<1296:CBAAAF>2.0.CO;2
    - Thank you for these references. They have been added.

- Line 413-416: this reads more like methods than discussion

- Agreed. We changed the sentence to reference previous mention in the Methods.

  "Our downscaled, local simulations broadly agree with geologic interpretations (Davis, 1989; Bierman et al., 2015; Corbett et al., 2018; Halsted et al., 2023) that suggest cold-based ice existed across areas of high relief in the NE USA and the existence of polythermal ice for the region during the LGM."

- Line 418: and Corbett's Mansfield paper...

  Corbett, L. B., Bierman, P. R., Wright,S., Shakun, J., Davis, P.T., Goehring, B., Halsted, C., Koester, A., Caffee, M., and Zimmerman, S. (2018) Analysis of multiple cosmogenic nuclides constrains Laurentide Ice Sheet history and process on Mt. Mansfield, Vermont's highest peak, Quaternary Science Reviews. doi.org/10.1016/j.quascirev.2018.12.014
  - Thank you. We have added that reference.

- Line 424: I think that there are Gosse data from here and you should look at in review paper by Cavnar and the cites in it for more suggestions of inheritance. As well the work of LeBlanc. See:
  Cavnar, P.M., Bierman, P.R., Shakun, J.D., Corbett, L. B., LeBlanc, D., Galford, G.L., and Caffee, M. (in review, 7.2024) In situ Cosmogenic 10/Be and 26Al in deglacial sediment reveals interglacial exposure, burial, and limited erosion under the Quebec-Labrador Ice Dome. Geochronology. doi.org/10.5194/egusphere-2024-2233
  LeBlanc, D. E., Shakun, J. D., Corbett, L. B., Bierman, P. R., Caffee, M. W., and Hidy, A. (2023) Laurentide Ice Sheet persistence during Pleistocene interglacials, Geology. doi.org/10.1130/G50820.1
  - Thanks for the references, but this paper we cited has a study area which matches closely with the simulated frozen bed - that is why we chose to cite this paper in particular.

- Line 425: data plural, "are"
  - Change has been made.
- Line 432: Removed "when looking at" per reviewers request.
- Line 432-437: this reads more like methods than discussion
  - Have removed the sentence: "Our experiments relied on a small ensemble of simulated surface climate at the LGM from climate model experiments, each simulating a varying magnitude of LGM cooling and precipitation change."
- Line 437: you don't observe this - others report. reword.
  - Change has been made as recommended. "others report"
- Line 448-457: this is all background that would be more appropriate as a set up in the intro than here in the discussion.
  - Deleted most of this section and made specific reference to the individual peaks in the updated introduction. Updated sentence to:

    "Regional geologic interpretations of isotopic data suggest that a thermal boundary between cold-based (low erosive) ice and warm-based (erosive ice) existed at ~1200 m across areas of high bedrock relief in the NE USA (Halsted et al., 2022). Yet, undated geomorphic indicators of warm-based ice on Mount Washington are found at 1680 m and 1820 m."

- Line 458: is this level of precision really achievable? Suggest at least rounding to the 10s of meters.
  - We have rounded to the nearest 10s and updated for all other references to the precision in the text.

- Line 465: this is confusing...it's more a statement of fact (perhaps better in intro) than a decision of which might be right?
    - This sentence was left as is as we believe it acts as a direct comparison to the available geologic data regarding location of the thermal boundary as is done for the other local simulations (e.g., Mt. Katahdin and Mt. Washington). We believe this sentence acknowledges a discrepancy with one interpretation of the data, yet, still supports a variable thermal boundary for the NE USA which is demonstrated in our regional simulations.
- Line 471: I think this should be "agrees"
    - Change has been made.
- Line 473: if it agrees well, then you should cite these reconstructions and elaborate - are they geologic? isotopic?
    - Removed reference to the geologic reconstructions in this paragraph. The goal of this paragraph is to acknowledge that climate was not in a steady state - unlike what is simulated in our model. The connection to geologic reconstructions is not necessary here and is discussed earlier in the paper.

- Line 480: Ok, good to see this. I might move it up to near where I queried assumptions or even into methods.

    - Yes, we hope this helps answer some of the questions posed previously by the reviewer. Instead of moving this up we prefer to keep as is since it is a discussion point of some caveats in our experimental setup. We have instead opted to add additional text to the end of the methods section. See our response to your comment raised above in the methods section (Line 274).
- Line 490: they are not difficult to conduct...but the data are difficult to interpret. that's different. please reword for accuracy.
    - Good point. We have updated the text as recommended.
- Line 497: first mention of erratics vs bedrock - probably should be part of the intro
    - Reference to glacial erratics now made in the intro with reference to studies by De Laski, 1872; Tarr, 1900; Antevs, 1932; Davis, 1989.
- Line 498: really should cite earlier work including Marsella et al that suggested this.
    - Yes, thanks for the recommendation - done.
- Line 518: no need to be self-congratulatory – reader will get this! It's a cool paper on its own.
    - Change made as requested.
- Line 523: yes, but there are others and they all should be mentioned! especially the earlier ones by other authors.
    - We have updated with more references.
- Line 527: but the references cited are not lower latitude (or not substantially) and one is in the midwest on a lobe underlain by far less topography. It's also continental climate not adjacent to the ocean - the conclusion is not the place to discuss this but the discussion is!
    - Upon re-reading we agree, it seems out of place. Have removed.