Collins et al. (2024) present a comprehensive multiscale analysis of the sub-glacial core collected from Camp Century, Greenland, analyzing physical, geochemical, and cryostratigraphic characteristics. The authors use X-ray diffraction (XRD), micro-computed tomography (μCT), and scanning electron microscopy (SEM), to identify and characterize five distinct sedimentary units, each with characteristic structures, mineralogical compositions, and cryostructures, and then use these data to infer a sequence of depositional environments, spanning glacial to interglacial to glacial. Their findings add to a growing body of work on this subglacial material and provide further context into Greenland's paleoclimate history and ice sheet dynamics, though some of their interpretations remain speculative without further supporting evidence.
The reviewer accurately captured the nature and aims of our work. We agree that our interpretations are limited to the evidence we have. Fortunately, new geochemical and biotic data are coming and we will include some of these (with citations to abstracts for now) so as to make interpretations more robust. Nevertheless, as described in the points below, we will carefully assess the level of speculation within our interpretation.

Overall, this paper is well-written and polished, and the data and generally analyses are robust. However, my primary critique of this work is that the authors' interpretations occasionally overreach given the data presented and some aspects of the presentation lack sufficient detail to clearly follow their logic. While the authors' interpretations are plausible, it appears they could represent one possible scenario among several.
This is a reasonable critique and we will work in revision to be more explicit about what data we have and what suggestions are more certain and which are more speculative.

This is particularly uncertain when inferring the timing of cryostructure formation or depositional environments from qualitative observations. I believe the paper provides a solid empirical foundation and presents a valuable dataset but lacks specific evidence for some interglacial interpretations.
We have significant additional data to support interglacial age assignments (see below) but no firm data on the age of cryostructures and will state that clearly.

I describe my main concerns with the interpretation of the sediment facies below and then follow up with minor concerns.

"The higher ice content and the difference in cryostructures between Units1 and 3 supports the hypothesis that Unit 3 was deposited in or by liquid water."
The specific differences in cryostructures are not clearly presented in this manuscript. The paper lacks plots or tables that detail ice content by depth, as well as quantitative descriptions of ice lens characteristics such as shape, spacing, orientation, and thickness.
Agree, we will do our best to provide more of these type of data in revision and reduce our emphasis on these structures.

These details could provide valuable clues to the origin of the ice lenses and would allow for comparisons with theoretical predictions. Including a figure (in the supplemental material if necessary) that details and compares the various cryostructures within each unit in high resolution would greatly enhance clarity, as these structures are frequently referenced as evidence for interpreting the origins of each unit. Currently, it is challenging to contextualize the manuscript's claims alongside the physical evidence. The supplemental movies provide a helpful visual, but lack sufficient detail to quickly orient the reader.

This is an excellent suggestion we will do our best to incorporate specific details of cryostructures from the CT scans.

(As a note, I was unable to view the CT scan videos using an M1 Mac on either Safari or Firefox and ultimately had to download Chrome, which played them successfully.)

We deposited the scans in an NSF-supported repository (under review) and I have downloaded them in Safari on Intel, Firefox on PC and M2 Mac – not sure what the difficulty might be but not in our control.

Regarding the Interpretation for Unit 2

The authors suggest that Unit 2 is a ~1-m-thick remnant of basal ice (or even firn) preserved from a previous glaciation that survived interglacial conditions near the surface (e.g., Figure 8). This seems challenging to support without corroborating evidence, such as ice crystallography or age constraints.

We have neither ice crystallography nor any robust age constraints on the unit 2 but, initial luminesce data, neither complete or public suggests it is similar in age to the material above it (400-450 ky). The notion of firn/basal ice is supported by unpublished stable water isotope data. We will revise paper citing abstract with those data and change the wording to integrate possible alternative as this data is too broad to be integrated within this work and is currently the subject of articles in preparation

The resemblance to "basal ice" is unsurprising, given that this layer is located in subglacial frozen sediments near the ice-bed interface, akin to a frozen fringe. Theory predicts that a frozen fringe could grow meters thick and form ice lenses in basal till under freezing conditions (e.g., Meyer et al., 2023), following similar physics as frost heave in permafrost—an occurrence well-documented in the field (e.g., Christofferson and Tulacyzk, 2003; Fitzsimmons et al., 2024). A recent paper posits that clasts can migrate into ice through thermal regelation (Pierce et al., 2024), which could provide a mechanism to progressively incorporate till from the underlying layer. Ice crystal structure analysis could clarify whether this is highly sheared basal ice left over from a previous glaciation or ice grown through cryosuction, which would exhibit distinctive characteristics. Without such data, the timing of its emplacement remains speculative. One might imagine Unit 3 being deposited over Unit 1, with later ice lens growth. Based on the information provided, I don't see how it could be said with certainty.

The reviewer is correct – without direct geochronology we cannot for certain pinpoint the age of unit 2. Pore ice pH and conductivity of the core (Bierman et al 2024) show that they are dramatically different geochemically, we will make that clearer in the revision.

Additionally, while the presence of vertical cracks is intriguing and could potentially represent freeze-thaw cycles, I do not believe they are a smoking gun. This layer could have experienced any number of glaciotectonic events that induced brittle strain, and the history is difficult to deduce from a single core, which is essentially a point source.
We agree that freeze-thaw is not the only possible explanation, we will expand this discussion.

Regarding the Interpretation for Unit 3
The authors describe Unit 3 as a slump deposit, partly based on the presence of normal grading. This interpretation is not readily apparent to me from the evidence provided in Figure 2. The core sections labeled as Unit 3 are divided between 1060-C3 and the lower portion of 1060-C1, with a substantial gap between them, which complicates a continuous interpretation. In the lower section (1060-C3), the sample is clast-rich, while the upper portion (1060-C1) contains mostly fines. This contrast presumably forms the basis for describing Unit 3 as normally graded. However, 1060-C3 does not visually appear graded; it lacks the clear stratification expected in a slump deposit and could simply represent basal till, with clasts suspended in a finer matrix.
We will re-examine evidence for normal grading and add to the revised ms additional data from other sources that also suggest this is a slump deposit. We will also revisit the terminology as we may have a different definition of slump deposits as the reviewer to consider the term debris flow as more appropriate. We however would like to point out that this portion of the core in in fact very complete and continuous as shown on figure 2 with the ct-scans of subcores 1060-C3, 1060-C2 and 1060-C1 in direct connection.

The missing sediment between 1060-C3 and 1060-C1 leaves some ambiguity in interpreting the contact between these layers. This discontinuity between a poorly sorted till and fines could indicate separate depositional events/mechanisms, weakening the argument for a continuous slump feature. Additional data—such as grain size distributions with depth or clast fabric/orientation relative to underlying till sections—would lend stronger support to the slump hypothesis (though it would still be difficult to ascertain from a single core without lateral context). Clast orientation specifically could help distinguish a traction till, characterized by horizontal shear, from a flow till, deposited downslope. In lieu of these constraints, alternative interpretations are plausible.
All of these ideas are wonderful but with a several inch wide core, not possible to test. We will work to provide viable alternative explanations while continuing to focus on the interpretations we consider best supported by our observations and data. We also want to point out that 1060-C2 is not missing and that the sample was analyzed by XRD, SEM and ctscan.

"The paucity of grain coatings in Unit 3 compared to Unit 1 (Fig. 5) could indicate that the slumping process disrupted grain coatings or mixed sediment from Unit 1 with sediment from the upper units, 4 and 5."

If Unit 3 was deposited as a basal till and subsequently covered by fluvial deposits, then the lack of grain coatings might be expected. My impression is that the main evidence for interpreting Unit 3's deposition as subaerial or subaqueous, rather than glacial, is the presence of weathered coatings in Unit 1 below, which suggests significant exposure at Earth's surface. This would imply that ice retreated, exposing Unit 1, before Unit 3 was subsequently deposited. An alternative hypothesis could be an ice readvance that deposited Unit 3. While the paleo-climatic sequence for this specific region isn't my specialty, are there regional constraints that would support this specific locale being ice-free for the full duration of the interval given by the age constraints of Units 1 and 5, or is this largely unknown?
We will clarify our description. We don't consider a re-advance as a likely source of a very thin (dm) layer of dimicton. Until our work, there was no understanding of the paleoclimatic sequence for Camp Century area because it's been buried by ice since 400 ky. Overall, we will better state and review what chronologic constraints exist for the sub-ice material.

"The sub-horizontal, braided, lenticular cryostructures are consistent with syngenetic permafrost formation, suggesting based on cryostratigraphy, little influence of liquid water in Unit 1 after permafrost formation (French & Shur, 2010)."

Based on the evidence provided in this manuscript, the timing of these lenticular cryostructures remains ambiguous. While the authors suggest they formed as syngenetic permafrost in a subaerial environment, it is equally plausible that they developed subglacially over a range of timescales. Without additional dating or contextual evidence, attributing them definitively to subaerial permafrost formation is speculative.
Agree, we will provide this alternative and make it clear there are other possible processes that could have formed

"Overall, the similar sedimentary structures and mineralogy suggest that Unit 3 was originally a part of the subglacial till formed below the ice (Unit 1) that was subject to slumping due to saturation of liquid water during interglacial conditions."
While I concur that this likely originated as subglacial till, the assertion that it is a slump deposit in an interglacial seems rather uncertain. As mentioned above, the timing here could have other possibilities. Even as a slump, perhaps it could be subglacial as well, perhaps representing the collapse of a canal's sidewalls.
We will be more specific in the logic we use to suggest the origin of unit 3 and cite other data. For example, luminescence ages on unit 3 show that it too was exposed at MIS 11 making a subglacial origin unlikely. WE appreciate the suggestion of a subglacial basal channel collapsing wall and will consider this option.

"Water and vegetation: During interglacial conditions, the permafrost landscape was subject to freeze-thaw cycles as shown by the vertical lenses of clear ice in Unit 2. Till, saturated by water, flowed downslope and buried the ice forming Unit 3. Interglacial conditions supported plant growth and the development of a headwaters fluvial system that eroded the upper portion of the flow-till deposit, stripping grain coatings but not changing grain shape. The fluvial system then deposited bedded sand, initially fine-grained and then coarser-grained material."

The evidence presented here doesn't necessarily indicate a unique interglacial, subaerial depositional environment:

1. Ice lenses could have formed in these layers subglacially over an ambiguous range of timescales.
2. Unit 2 may have grown after the deposition of Unit 3. If Unit 3 represents a slump (though this is not convincingly demonstrated), it's plausible that it collapsed as a subglacial feature, potentially forming canal walls.
3. The introduction (Lines 60-62) states the presence of plant and invertebrate fossils in the core necessitates the site was ice-free during MIS11, but the authors do not explicitly state what units described in this paper contained signs of biological life. Fossils in Units 2-5 would significantly bolster the interglacial deposition argument, yet this isn't explicitly discussed in this manuscript. In particular, are there biological remains in Unit 3?

There is a lot to unpack here – some of which we can address with certainty, some we have other data that helps constrain the stratigraphy, and the rest of which will, due to lack of data, remain uncertain. For example, we have found biological remains in every sample from the core – this is the focus of an upcoming paper. There are number of geochemical and isotopic measures showing the close similarity of units 1 and 3. But, unit 2 remains poorly understood with come suggestions from stable water isotopes that it could be firn deposited on the underlying unit 1 and then buried by unit 3. In revision, we will add the information about biologics requested by the reviewers and where appropriate discuss alternative hypotheses and where useful, present other data in support of our hypotheses.

Line 172: What evidence is there that this has undergone minimal deformation? Line 348 mentions deformed bedding, for instance, with no further explanation. Has previous work assessed the microstructure in detail? A citation would be warranted here.
This work is the first attempt at a partial microstructural analysis of the core using ct-scan. The evidence is visual. We will state this explicitly and add a reference to previous work when referring to the macrostructural analysis in this method section.

Line 316: "Our multiscale analysis supports, refines, updates, and provides more justification and detail regarding unit delineations proposed earlier (Bierman et al., 2024; Christ et al., 2021, 2023; Fountain et al., 1981)." It would be helpful to identify in the discussion how observations made in this paper agree or disagree with past interpretations of the units.
This is a good idea which we will implement in revisions.

Line 322: Technically, it is all subglacial material. Does this refer to Units 1 and 2? We will clarify that this refers to the entire subglacial core material

Minor punctuation errors

- **Line 47**: "1969)), but the sub-glacial material" → missing comma.
- **Line 172**: "minimally, if at all, deformed."

We will correct these errors.

Works cited

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Fitzsimons, S., Samyn, D. and Lorrain, R., 2024. Deformation, strength and tectonic evolution of basal ice in Taylor Glacier, Antarctica. *Journal of Geophysical Research: Earth Surface*, *129*(4), p.e2023JF007456.