The paper by Catherine Collins and co-authors presents a new rich dataset from the precious sediment samples collected at the base of the Camp Century ice core. The analyses include microCT, XRD, SEM-EDS and SEM imaging. The authors looked at the sedimentary facies in order to decipher the processes responsible for the deposition and preservation of sediments and infer some paleoenvironmental interpretation. The paper is well written and the figures have been made with great care, although several improvements can be done (see detailed comments below).
We appreciate reviewer's enthusiasm for our work.

This paper refers a lot to another one submitted to the journal *The Cryosphere* by Bierman *et al.* (still a preprint at the time this review is written).
This paper is now published.

Unfortunately, the reader needs to read the Bierman *et al.* paper to understand several things, such as the sample numbering and the stratigraphy. I think this essential information needs to be incorporated within this paper. For instance, the uppermost core section is the one on the right in the figure, which is not what one usually when presenting sediment cores. Incorporating panel B of figure 2 of the Bierman's paper would be very helpful.
With the editor's permission, we are happy to make this change. We were worried about replication but the reviewer makes a good point.

A disturbing feeling while reading the discussion section of this paper is that many arguments for the interpretations of the sediment facies are coming from previous papers. A clear distinction between the interpretations based on this new dataset, and the data presented elsewhere is needed.
This is a useful comment. We will work in revision to make clear what the new data set tells us that prior data did not.

My main concern is about the interpretation of the sedimentary facies. I'm not a specialist of glacial/proglacial/subglacial microfacies, but in such settings, it is critical to have an idea of position of the ice, the direction of the flows, the topography, the slopes, etc., to make a solid interpretation, which is impossible to obtain from a single sediment core.
We agree that our ability to make interpretations in limited because of the size of the core and our inability to place what we see in a broader landscape context. In particular, the core was never oriented and the sub-core stored without relative orientation. We will add a statement to the discussion making this clear.

Moreover, it is very likely that the sediments recovered at the bottom of 1350 m of ice experienced some glaciotectonism. This is not discussed in the manuscript, and it should, because it is not sure that the ice cap was cold based all the time, especially if one of the interpretations suggested by the authors is that a river flowed at this site. In short, I think that the observations remain interesting but that the facies interpretation is pushed too far.

It would be useful to have the input of a specialist of microsedimentological analyses of glaciogenic sediments.
We will consider glaciotectonism as we revise the manuscript. Others are conducting more detailed analyses of the CT scans which may provide more specific data based on clast orientation but that work is in progress and outside the scope of this particular paper. We believe the thermal state of the ice sheet and the presence of moving water when the area was deglaciated are two independent observations.

For the statistical interpretation, the choice of the variables considered is not well justified; for instance, why including the depth of the sample as a variable (it could be an explanatory data).
We may not fully understand the second part of the reviewer's question - "why it couldn't have been an explanatory variable?" We did use depth as one of our predictor (explanatory) variables. We will include justification for our variable choices.

Finally, there are many small mistakes and inaccuracies in the µCT dataset description making the paper arduous to read. There are details about the µCT data acquisition in the online public repository (congratulations for that effort), but it would be nice adding some information in the paper and/or in the repository in order to be able to reproduce the measurements (see my detailed comments below).

Unfortunately, I was unable to open all the movies made on processed images.
We will investigate with the repository why this might be the case.

While the movies are a very nice addition, it is quite impossible to analyse these images. The addition of images that can be handled by an image analysis software would have been better in my point of view, but this is already very nice.
Because of the nature of the core material, the detailed microstratigraphical analysis of the core was not achievable to date. It is a technical and methodological challenge that we currently investigating and consider worthy of a subsequent manuscript. The raw SEM and CT scan data is currently in review by the artic data center and should be accessible within a few weeks.

Detailed comments:
Line 65 : « sequence stratigraphy » has a specific meaning in sedimentology that is not related to the reality you want to describe. I suggest changing this by “the lithological succession”.
Agree.

Line 166: “shrinking as sediment freezes”. I’m not a specialist of these structures, but it does not seem logical to have shrunk when ice has a 10% higher volume. Can you expand on that?

Thank you for pointing this out. As written this sentence excludes part of what is included in French & Shur, 2010 which is explained as follows: “Reticulate cryostuctures are thought to reflect desiccation and shrinkage as sediment progressively freezes and moisture migrates to the advancing freezing front (e.g., Mackay, 1974).” We will add this citation and clarify the sentence to include this nuance.

Lines 1972-173: this statement is strong and should be supported by evidence or a reference to other studies.
We will add references and more supporting evidence.

Lines 1978-1982: these sentences belong to the introduction, not the method section.
We will move these sentences.

Method section: there is little information about the quality of the storage and the transport conditions during the four transfers of the samples that occurred through time. If there is no information, this should be stated. Also, the reader is referred to another paper for the sample handling; I suggest adding a more specific reference, for instance, what figure one needs to look at? Or to add that information or figure in the supplements.
There is no information about these sample transfers and storage – we will add a statement to that effect to the manuscript. What little we have been able to learn/discover is included in the Bierman et al 2024 Cryosphere paper.

Lines 186-187: please add some technical information about the energy used to scan the samples. The sample size, the geometry of the acquisition and if any filter was used. Was the reconstruction made using the NRecon software correcting for artefacts such as beam hardening?
We will add this information to the manuscript.

Line 292: perimeter is a quantity that is not robust when small grains are measured. What is the size of the grains analysed by Fiji, in number of pixels? One needs to have at least 300 pixels to have a robust measurement. See for instance, Francus and Pirard (2004). Testing for sources of errors in quantitative image analysis in P. Francus (ed.) 2004. *Image Analysis, Sediments and Paleoenvironments*. Kluwer Academic Publishers, Dordrecht, The Netherlands. The area measured in 2D slices underestimate the size of particles (same ref and many others). This should be stated somewhere in the text.
We did filter small particles when doing the image analysis. These details will be added to the manuscript.

Lines 295-298: I believe this might be problematic to compare these shape parameters taken on images with different resolutions (see again the same paper motioned above). I’m not sure what the Tukey-Kramer honestly significant difference statistical test does, and maybe it is good enough, but you should discuss a possible bias.
Thank you for this suggestion. We will consider biases in these tests used and add commentary to the manuscript.

Lines 301-313: I'm not sure to understand in what order these statistical analyses have been performed: first, first PCA on the image analysis data set on quartz grains only, and then K-means clustering on all data? Correct? Please try to clarify your text.
We will clarify this.

Lines 316-322: are these new observations, or corroborating the results of the paper cited? The wording is not clear about this.
These are new observations from the paper. We will clarify the wording on this front.

Figure 2: this figure is good-looking, and I'm sure you spent a lot of time on it. However, it needs to be improved. First, historical images are not visible in the panel a). The figure would gain in readability if the top and the bottom of the section were indicated. A scale is missing. Adding a depth scale is also needed to help read the following lines. Panel b): What is exactly the full view? Do you mean a topogram, i.e., the equivalent of a radiograph? On what is based on the colour code? And what does it mean? These are false colour, right? So what do you gain here to transform the greyscale original images to these colourful images? If you gain something, that is correct, but if not, you should consider greyscale for the CT images. Also, I think that *Climate of the Past* has a policy regarding these figures to make sure that colour-blind people can read them. Finally, what exactly is showing the *Particle view*? Have you segmented all the denser particles from the volume and made a sum of them in one direction? How have you made this segmentation? What is the smallest size of the threshold particles?
To answer this comment, we will first answer the clarifying questions included:

- What is exactly the full view? Do you mean a topogram, i.e., the equivalent of a radiograph?
    - Full view is an unaltered scan (i.e, no density filtering or lateral slicing/cropping). It goes beyond a topogram as it is a still of a reconstructed scan. We will clarify this wording
- On what is based on the colour code? And what does it mean? These are false colour, right? So what do you gain here to transform the greyscale original images to these colourful images? If you gain something, that is correct, but if not, you should consider greyscale for the CT images.
    - The color is based on density but there is no consistent density assignments to the color values, they are relative withing each core segment. We assigned the denser particles within a sample a deep red color. We think this eases interpretation as red is often attributed to higher values. In the original grey scale, the dense particles are lighter in color which could be counterintuitive to some. In addition, colorization of the grayscale density scan improves readability as the human eye can distinguish between approximately 10 millions hues compared to about 1000 gray values.

- Also, I think that *Climate of the Past* has a policy regarding these figures to make sure that colour-blind people can read them.

    - We thank the reviewer for this important insight and will provide alternative visualizations of this figure in the supporting information, including in grayscale. This is easily achievable using the imageJ "daltonize" tool which generate image adapted to protanopia, deuteranopia, tripanopia and other types of color blindness.

- Finally, what exactly is showing the *Particle view*? Have you segmented all the denser particles from the volume and made a sum of them in one direction? How have you made this segmentation? What is the smallest size of the threshold particles?
    - You are correct that "particle view" is showing dense particles at a certain threshold. This was done with the Brucker CTVox software which includes a thresholding tool. The size of particles is dependent on the resolution of the CT scanner (70 microns).

Your comments regarding readability are very useful, thank you for including them. We will consider these to improve this figure.

The labels "units 1" and "unit 2", in the 3rd column of panel a) seems to have been inverted, making the explanations below very difficult to understand.
Thank you for pointing this out. We will revise this to improve readability.

The vertical clear ice inclusions are not obvious. Are those the very narrow vertical lines, one on the left of the image, the other in the centre?
Yes those are the vertical inclusions.

Line 333: please add the name of the samples, i.e.1063-6 to 1062-1, in a similar way than you did below.
We will make this change in revision.

Line 335: what size are the clasts?
We will add this information in revision.

Line 336: sample 1062-4 seems to be a better example of these ice lenses with a braided lenticular pattern.
Yes 1062-4 does exhibit this pattern. We will consider emphasizing this sample as a good example for this pattern.

Lines 337-339: it is not clear what the reader has to look at in figure 2a that is representing ice. Where exactly are these 2 ice-rich layers?
We will add this information in revision.

Lines 340-341: it seems that the topmost sample of unit 1 is 1061-D5, right? Unless you call a “section” something that corresponds to a core tube in Bierman’s paper. This is why you need to incorporate the information from the Bierman’s paper about the sample names in this paper as well.
We will add this information in revision.

Line 342: I count 7 samples.
We will double check this value.

Line 343: how have you obtained this density and the % ice content? Is it with µCT? If yes, this is not trivial to obtain, and you should explain how you acquired these numbers.
This is explained in Bierman et al. which we will reference here and explain.

Lines 344-345: not all the samples display 45° bedding: samples 1061-D1 and 1061-D2 display horizontal contacts.
There are some artifacts from CT scanning that make some interpretations of these features difficult. We will investigate these samples to confirm the validity of these interpretations and clarify this shortcoming in the text.

Line 345: where is sample 1060-D3 in panel a)? Maybe you mean 1061-D3? If this is the case, then panel b) labelling needs to be corrected. Actually, sample 1060-D3 does not seem to exist elsewhere.
Thank you for pointing out this oversight. This will be corrected. The sample is incorrectly labeled as 1060-D3 in panel b.

Line 348: this sentence “The samples in this unit are 1060-C3, 1060-C2, and the lower portion of 1060-C1.” should start the paragraph. I do not see the bedding, the grading and the cryostructures in the µCT-Scan images.
We will clarify this in the text.

Line 349: the text says here there is no bedding but the line above, there was bedding. Please review your text.
We will review this in the manuscript.

Line 351: can you better show on the picture the reticulate structure that you mention here?
We will review the samples for a better example of reticulate structures.

Line 352: bedding is visible in 1060-C1, but cryptic in 1060-C2.
Some filtering of ice/particles can reveal these features. There is a link to the entire archive that should include multiple view points of the samples for reference.

Line 353: How the ice content has been measured? (Same question as above)
This is explained in Bierman et al. which we will reference here and explain.

Lines 354-355: how can the reader know what is the a) sample and the b) sample in Figure 2? One can guess that the b) is on the right, but please add something about this in the figure.
We will review this.

Lines 355-356: Authors write "Directly below the contact bedding curves from sub-horizontal, downward to nearly 90° which continues into 1060-C", but the bedding is not visible in the picture of 1060-C2.
Good point, we will consider adding specific examples of stratigraphic features in the SI for readability.

Section 4.1: are the results presented here from the observation of the µCT scan image only?
Yes. They are supplemented by previous work in Bierman et al. 2024

Figure 3: Please change the label "14A" (standing for 14 Angstrom I suppose) from the legend into something like "clay minerals". Also, it seems that there are more amorphous minerals in samples 1060-A2 and 1059-7. Could you comment on that?
We'd rather not change the label to "clay minerals" as this would restrict interpretation. While it is true that the majority of minerals with a 14A reflection are phyllosilicates, principally in the chlorite group, other minerals can display predominant 14A d-spacing (tobermoreite or zeolites for example). We do not attribute significance to the higher level of background in the XRD of 1060-A2 and 1059-7 as we do not identify a broad "hump" at mid d-spacing value. In fact the high initial background is due to a stronger contribution of the background slide due to a smaller amount of material available for analysis in these samples.

Line 383: "selected" instead of "select", right?
We will consider this edit.

Figure 4a: what the vertical axis in the area plots means? Is it depth in the section of the sample? This is not easily readable. I would only keep the histograms. Also, in general, one plots those particle size distribution using a logarithmic scale.
We are not sure that we understand that comment, which means that our figure is indeed hard to read. We suspect that the reviewer wonders about the spread of the individual points. We will consider removing these and plotting on a logarithmic scale.

Figure 4b: this plot is not readable. I suggest making scatter plots with the size, this will be helpful to check if size influences the two other parameters.
Size is indicated by the size of each dot on the graph. We will consider this for readability.

Lines 424-425: it would be a good idea to repeat here the formula of the parameters, so the figure is self-standing.

We will add this to the figure.

Lines 434-435: I suggest adding here what group the samples belong to (for ex. A-samples,...)
We will make this change.

Line 436: “maximum mean value” is more appropriate, same for the minimum.
We will make this change.

Line 437: is roundness distribution unimodal? I really doubt it is, the spread is very wide when looking at the figure.
We will look closer into these results.

Figure 5b: is the sum of the two plots making 100%. If yes, why do you need two plots?
The sum is not 100% as it doesn’t represent moderately coated grains.

Lines 448-449: see my earlier comment on potential bias of the size of the particles on the shape indices.
Thank you we will implement the discussion of bias.

Line 454: sorry, but Fig.5a does not allow to see the coatings. Fig5 is too small for that.
We will adjust the figure for readability.

Line 470: “grains within the core”: which one?
We will replace these words with "grains from almost all sedimentary units” to avoid confusion.

Lines 462-471: it is a pity that there is no detailed account of all these features, to have at least a semi-quantitative view of their occurrences.
We will consider how to add this.

Section 4.3: how the analysed grains have been selected? Random selection is quite important to avoid biases, or maybe all visible grains were selected.
Random selection was used, we will add this to the method description.

Figure 6: the figure is very nice, but the element code in panel g is not clear and the scale shows the ∞ character, I suppose instead of the µ one.
We will correct this.

Figure 7c: the variable labels are too small.
We will correct this.

Line 495: why have you included depth as a variable? I suppose it is the depth in the core. If it is, then I think this is biasing your statistical analysis, forcing the samples with the same depth to be similar (spatial autocorrelation). I think you should remove this variable, and redo your statistical analysis without that variable. This brings the question how the variables included in the analysis have been selected? Can you expand on that?
We used all the data we could for this analysis as it is exploratory. As depth is relevant in stratigraphy and interpretations of stratigraphic deposits, we elected to retain depth as a variable so as to include as much information as possible into the algorithm.

Line 506: in the introduction, you write that the sediment core is made of several units, but here you assess this unit assignment. From this, the reader understands that the units have been previously defined, and I suppose this was made in previous papers. The authors should clearly distinguish the interpretation derived from the dataset presented here from the other proxy not presented here. Also, could you suggest a change in the unit assignments using your dataset only?
What is new here and different from Christ et al. (2021) and Bierman et al. (2024) is the thorough and in depth stratigraphic characterization based not only on visual observation but on numerous parameters measured at a variety of scales. While previous work identified units macroscopically, here we assess the microscopic, sedimentologic, and mineralogical nature of these units and propose a scenario for the past evolution of the ground surface environment beneath Camp Century. We will as the reviewer suggests point out this fundamental difference in the revised text more clearly and provide specific details about how what we have done in this study differs from studies that came before.

Line 530: I suggest adding “subsequent” here : (…) before subsequent cooling (…)
We will correct this sentence.

Line 535: which cryostructure are you talking about?
Will add specifics.

Line 536: this is the first time you mention that slumping is occurring. You should first demonstrate that the sediment is indeed a slump.
This came up in the other review and we will address by providing additional data consistent with slumping.

Lines 538-539: slumps can also produce debris flows, which are sediment facies that are not well sorted. If there is a normal grading, it is more likely that a turbiditic current occurred, implying that the environment is not compatible with sediments flowing downslope. Many questions come to mind here: for instance, was the site in an aerial or aqueous environment?
Similar questions were raised by the other reviewer, using all data available to us, we will reconsider our interpretation and terminology and provide a more rigorous analysis of this unit.

Lines 535-544: this interpretation for unit 3 is very hypothetical. I don't think that these inferences can be made out of a single core. One needs to have observations about what is happening laterally.
There are no lateral observations as this is a drill core but we do have other data indicating very strongly that unit 3 was derived at least in part from unit 1 and that it is a poorly sorted diamict. During revision we will make a more reasoned argument with supporting data.

Line 556: sediment content: what do you refer to here? Grain size? The horizontal alignment of grains is present only in a few samples.
We are referring to the suspended grains in the ice matrix in Unit 2. The wording will be revised for clarity.

Line 562: fluvial sediments are usually better sorted that these.
As the degree of sorting in fluvial systems is dependent (among other parameters) on the energy of the system and size of the stream we think that this term is applicable to a small periglacial stream.

Line 565: "Multiple lines of evidence": please specify which ones. The ones from this dataset or from other papers? Your dataset is not very convincing that this unit has been created by a river.
We will be more specific in citing data sets supporting our rationale.

Line 569: is there information about the topography under the ice cap?
Yes. There is a large radar dataset and we will report on it but it is at a scale much too coarse to evaluate site specific topography.

Line 591: this is counter-intuitive: glacial tills are usually coarser than fluvial sediments.
We will consider this and add citation to back our findings.

Figure 8: the font size in the boxes are too small.
We will correct this.