Review of: ***Englacial Architecture of Lambert Glacier, East Antarctica*** by Rebecca J. Sanderson and others.

The paper focuses on analysing englacial stratigraphy from radio-echo sounding data from the Lambert Glacier catchment in East Antarctica. For the analysis, an internal-layering continuity index (ILCI) is applied to classify the survey region into areas of high and low layer continuity. Four areas are discussed in detail and the ILCI and other features, such as englacial folds, are set in the glaciological context, e.g. with ice velocity and bed topography. On this basis, interpretations are made on the causes of folding or other reasons for layer discontinuity, as well as interpretations of the ice-dynamic stability of the glaciological system over the last thousands of years.

The paper is very well written, logically constructed and well structured. The many illustrations give a good overview of the radar stratigraphy in connection with important features such as ice velocity, topography and features on the ice surface. As I understand it, this is the first major analysis of the radar stratigraphy of this area where a very large data set is covered. Due to the large amount of data, it makes sense to use an analysis tool like the layer continuity index, especially when it comes to getting a first overview of this region.

In my opinion, however, some of the interpretation of the results of the ILCI values, in particular, what they mean and how the ILCI values are influenced by the features in the radargrams, should be improved and complemented. In addition, I am not yet convinced regarding some of the interpretations and statements being made, especially regarding the development of folds. I also have a different point of view in a few passages in the text as well as questions for clarification.

I am aware that the analysis and interpretation of such data are complex and take time, simply because of the amount of data and number of different features it contains. The authors have a good job on focusing on certain aspects and the paper makes an important contribution, which is highly relevant to the glaciological community and for our understanding of the Antarctic ice sheet. Therefore, I would be very happy to see this paper published, even though I think that a few things should be presented or added in more detail. I hope that my comments and questions will help to clarify a few additional things and still unclear issues (which at least are not 100% clear to me). In the following, I will address a few main points as well as specific points in the text.

Main points

1. Factors influencing the ILCI

The ILCI is supposed to be a measure of the continuity of the internal layers and it would be interested to know if the following things were taken into account for the evaluation of the index:

- Does the orientation of the radar lines, e.g., with respect to ice flow direction or with respect to the fold structures in Zone 2 affect the ILCI values? If yes, how, and maybe one sentence could be added to section 3.2 in that regard.
- In the radargrams in Figure 4, 5a,b,c (especially in this one) and 6a a strong birefringence signal is visible. It would be interesting to know how the birefringence affects the ILCI and whether this has been considered in the calculation. My assumption is that if the birefringence patterns are aligned horizontally, they probably don't have much effect, but if they are tilted, they have an effect depending on the window size. If the birefringence affects the continuity index, then the interpretation of the results becomes even more complicated. Depending on how the fabric is pronounced in the horizontal plane in these regions and how the window size is chosen, the interpretation of the results becomes more complex, but also contains more information.

  The presence of this pattern should be mentioned in general (see Young et al., 2021; Gerber et al., 2022) and the potential effect on the ILCI should be included in 3.2 and the discussion, i.e. whether and how birefringence affects the continuity index and what the birefringence signal could potentially mean in terms of ice dynamics.

Young, T. J., Schroeder, D. M., Jordan, T. M., Christoffersen, P., Tulaczyk, S. M., Culberg, R., & Bienert, N. L. (2021). Inferring ice fabric from birefringence loss in airborne radargrams: Application to the eastern shear margin of Thwaites Glacier, West Antarctica. Journal of Geophysical Research: Earth Surface, 126, e2020JF006023. https://doi.org/10.1029/2020JF006023

Gerber, T., Lilien, D., Rathmann, N., Franke, S., Young, T. J., Valero-Delgado, F., Ershadi, R., Drews, R., Zeising, O., Humbert, A., Stoll, N., Weikusat, I., Grinsted, A., Hvidberg, C., Jansen, D., Miller, H., Helm, V., Steinhage, D., O'Neill, C., Gogineni, P., Paden, J., Dahl-Jensen, D., and Eisen, O.: Crystal fabric anisotropy causes directional hardening of the Northeast Greenland Ice Stream, Preprint (Version 1), Research Square, https://doi.org/10.21203/rs.3.rs-1812870/v1, 2022

2. Linking ILCI values with ice flow velocity

In the paper, the authors often connect low ILCI (i.e., disrupted layering) and high flow velocity. This is one of the statements repeated very often in the paper. However, my impression from the results of the analyses is that other specific correlations are the much more useful background information related to the disturbed stratigraphy and these should be brought to the foreground. It is true that, especially in Zone 2, the low ILCI is associated with higher flow velocity (and acceleration). However, I don't think that just an increase in flow velocity can disrupt internal layering, because it would only lead to along-flow dilatation, which should have no other effect on the layering than thinning. My feeling is that horizontal shortening in the ice that starts in this region of convergent flow and acceleration, that leads to englacial folding, which in turn leads to backscatter loss at the steep internal layers. This leads to a low ILCI, but at this point the ILCI itself is no longer relevant but rather the fold geometry, which allows further analyses and interpretation. In my opinion, the statement that a higher flow velocity (magnitude) is related to disrupted layering is too simplistic. It coincides in this Zone but is not necessarily related to each other.

3. The origin of flow bands

The paper suggests that *so-called* flow bands are formed by variable basal sliding, and much of the discussion focuses on inferring basal shear stress in the region and comparing it with results from other studies of basal shear stress in other Antarctic ice streams. I am not yet convinced that the "flow bands", which to me are open cylindrical folds extending parallel to the ice flow, are caused by changes in basal shear stress. I have the impression that it is more of an assumption than a solid interpretation. The references to the literature in this context were not fully informative enough for me as to how the folds we see there are formed by variations in shear stress at the base of the ice. A long part of the discussion is then based on this interpretation, which I find problematic if this interpretation is not supported by more arguments.

My suggestion would be, on the one hand (if the authors keep this explanation as the most likely explanation for the formation of these folds), to explain in much more detail how differences in friction at the base produce exactly these folds (with fold axes parallel to ice flow). More references would also be helpful here, especially clearly referencing the mechanism and how it relates to the folds we see.
Secondly, an alternative explanation that the folds we see in Zone 2 in the graben System are caused by convergent flow leading to horizontal shortening (Bons et al., 2016) should be included. My impression is that the graben system itself is causing the convergent flow, as it is the only way for the ice to flow towards the coast. This mechanism would be independent of the basal properties and would fit particularly well with the alignment of the fold axes. In my opinion, it would be helpful to show flow lines in combination with the ILCI values and the fold axes (here called flow bands). The acceleration of the ice flow would compensate for the mass gain due to the convergence and only lead to the dilatation of the folds along flow, which should have no influence on the folds shape and only reduces the amplitudes. For me, this hypothesis is even more plausible than the theory listed in the paper. In any case, arguments should be listed that speak for or against the respective theory.

Bons, P. D., Jansen, D., Mundel, F., Bauer, C. C., Binder, T., Eisen, O., Jessell, M. W., Llorens, M.-G., Steinbach, F., and Steinhage, D.: Converging flow and anisotropy cause large-scale folding in Greenland's ice sheet, Nature communications, 7, 1-6, https://doi.org/10.1038/ncomms11427, 2016

Specific points

L33-35: I am not sure if the statement that the ILCI values are *“revealing the transition from internal-deformation-controlled to basal-sliding-dominated ice flow”* should be made (see further comments).

L35: *“which align”* → what does align, the fold axes of the folds?

L93: What is the *"(65)"* referring to?

L98: *"The RES data used for this study were acquired in 2007-2009 over the northern region…"* → over the northern region of the RES grid?

L98-101: What is the vertical and horizontal resolution of the RES data?

L113 (Section 3.2): I think it would be good to mention that the overall quality of the radar data and the processing level have an effect on the continuity index as well as "other" potential causes, such as surface clutter (which is not the case here) or as in this case potentially the superimposed birefringence signal.
Another thing that could be added is a short statement about the benefit of this type of analysis in contrast to other more time-consuming methods, such as tracing many IRHs. For me, the advantage of using an ILCI would be that certain information is available for a large data set, which gives first clues for the overall nature of the internal structure and where to apply further, more particular analyses.

L122: add a space *"tracks(Fig. 2)"*.

L166: To me, this section highlights that it is probably not an optimal idea to focus on linking the ILCI values to the magnitude of ice flow. First, because Zone 4 would be an exception (which, to be fair, is discussed later in the discussion) and second, because not the magnitude, but the pattern of flow seems to be the controlling factor in combination with the bed topography, the quality of the radar data and superimposed features, such as the birefringence signal.
What I think would be extremely helpful is to plot flow lines or some other marker for horizontal shortening (90° to ice flow), in particular at the onset of Zone 2, because it looks like the ILCI is rather a signature for convergent flow (which I would expect at the trough onset) creating buckled folds and where buckled folds are transported downstream.

L188: *"zone 1"* → Zone 1

L200-201: The ILCI values for Zone 4 are really surprising and confusing to me. When I have a look at Figure 7, which is located in this zone, the stratigraphy looks very continuous but the values are extremely low. Hence, there is something in this data that falsely gives low ILCI values. At this point, I would not conclude that the radar stratigraphy in this region suggests *"disrupted flow"*. If so, what would actually be *"disruptive flow "* ?

L216-217: ***"****The spatial correspondence between ice flow exceeding 15 ma^{-1} and the marked shift in ILCI values in Zone 2 (Fig. 3a) suggests that the onset of ice flow is important for the englacial structure of Lambert Glacier."*
→ I agree, but the question is what causes the change in ILCI (which is caused in this zone by the layer buckling, correct?). For me, it looks like this is also the region where ice flow would converge (maybe this could be checked), regardless of the change in ice flow velocity.

L222: *"fold width"* → maybe better "wavelength".

L223: *"The amplitude of folding increases with ice thickness".* → What does this mean, that larger folds are found where the ice is thicker in general or that the fold amplitude varies in the ice column and increases with depth?

L225-229: I think the structures you are referring to are not the fold axes but the axial traces (the line that connects the hinges of the synclines or anticlines in your radargrams vertically), or if combined to a plane, the axial planes: see https://cdn.eduncle.com/library/scoop-files/2022/7/can_image_1657899700300.jpg).
The fold axis would be oriented "horizontally" by combining the hinge points of the same anticline or syncline from one radargram to the next one, hence more or less parallel to the direction of ice flow.

L233: Here, it says *"fold bands"*, should it be "flow bands"? If not, what are fold bands?

L236: *"high-frequency"* → maybe better "short-wavelength"?

L243: *"The fold runs parallel to ice flow"* → Do you mean "The fold axis runs parallel to ice flow"?

L243-244: *"[...] in an area of accelerating and converging ice-flow velocities (~ 15 ma-1 to ~ 50 ma-1)."* → What are "converging ice-flow velocities"? What I think is meant is that ice flow is accelerating (from 15 to 50 ma^{-1}), and at the same time, ice flow is convergent. So, ice flow is indeed convergent there, which would support the fold formation hypothesis from Bons et al. (2016).
I'm not saying that it should be done, but something that would be very interesting to see is how much horizontal shortening (90° to ice flow) along one of the fold axes (possibly the central one) is actually happening and if that fits to the change in wavelength of the folds along ice flow.

L245-247: *"Assuming that the fold formed and then advected down-ice with flow, we estimate that this englacial fold could have persisted for at least 10.5 ka (based on calculations of the average current ice velocity (Mouginot et al., 2019) and the distance between each fold on individual flightlines)."*
→ This is an excellent approach to investigate the stability of the system. It would be interesting to specify (in one of the maps) where you define the starting point of fold formation. Would it just be the location where in the radargrams the fold appears for the first time, hence the southernmost of the yellow dots in Figure 6a?

L250: Maybe also add Siegert et al. (2004) here.
Siegert, M. J. et al. Ice flow direction change in interior West Antarctica. Science 305, 1948–1951 (2004)

L267-269: Could you specify which of the zones you are referring to?

L269-270: *"This low ILCI in the upper ice-stream catchment is not associated with current enhanced or fast ice flow".* → If this refers to Zone 4, it says something different than in the statement in L200-201 (disruptive flow).

L269-271: Is it possible to refer to a Figure to see which region and radargrams are referred to?

275-277: *"Second, it is possible that disruption of the layers giving rise to low ILCI is a result of power loss from dipping englacial reflectors, as seen elsewhere in Antarctica (Holschuh et al., 2014; Winter et al., 2015)."* → Has this been checked, or is it an assumption?

279-283: The last of the three possibilities (like all three actually) have been listed here as potential reasons. Is it possible to look at the radar data to determine which of the three causes the low ILCI? Are the ice packets, as in Bell et al., 2011 to be found here or not? Here again I see the problem that we do not learn much from the ILCI values per se. If something is to be learned about the englacial architecture, it should be checked or excluded what is causing the low values.

L293-295: *"We propose that variable basal sliding, and therefore variable ice-flow speeds across zones 2 and 3, are the primary reason for high ILCI values in the region (which qualitatively define visible internal layer buckles) (Fig. 5)."*
→ Shouldn't it be rater "low" values instead of "high" in these zones?
I don't fully understand how variable basal sliding in these zones would create the observed folds. If the vertical velocity profile changes along flow, you may end up with something like the moving patches of high and low resistance as proposed by Wolovick et al., 2014? This would create folds (under particular conditions), which would, however trend (the fold axis) 90° to ice flow. But the buckled folds observed here trend parallel (the fold axis) to ice flow. Also, it is proposed later that the change in basal shear strength is rather gradual than abrupt.
If this interpretation on how the folds in Zone 2 are formed is the only one presented here, there is more explanation needed as well as references on how variable basal sliding would create the observed folds. The explanation that I would add as an equally well possibility is that the folds are created via horizontal shortening due to the flow convergence (Bons et al., 2016) when the ice is flowing into the main trough. This would match the orientation of the fold axis and would be independent of processes at the ice base.

Wolovick, M. J., T. T. Creyts, W. R. Buck, and R. E. Bell (2014), Traveling slippery patches produce thickness-scale folds in ice sheets, Geophys. Res. Lett., 41, 8895–8901, doi:10.1002/2014GL062248.

L305-312: Maybe I need clarification, but in the first paragraph of Section 5.1 you proposed three possibilities for the low ILCI values (assuming that in section 5.1 you were referring to this zone). But here, only the attenuation is mentioned. How does this fit together?

L319-320: *"As we assume that these buckled layers are the product of lateral shear stresses at the transition from slow and fast flow (Siegert et al., 2003a), [...]"*

→ I had a look at Siegert et al., 2003 and tried to understand the argumentation chain: onset of fast flow -> variable lateral shear stresses --> leads to layer buckling as observed here. However, Siegert et al., 2003 write:
"It should be noted that no attempt is made to quantify the degree of layer buckling or determine the mechanics responsible for "buckled" internal layering. It is likely that buckled layers occur as a consequence of high longitudinal stresses within regions of enhanced ice

flow, and lateral shear stresses at the transition of fast- and slow-flowing parts of the ice sheet (Jacobel and others, 1993). The assumption made in this paper is that these stresses occur as a consequence of enhanced ice flow, and that internal layers will become more buckled the longer such stresses are applied."
Jacobel and others, 1993 discuss a lot of possibilities for their buckled folds, but I have the feeling that at the end there is no clear conclusion that basal shear stresses are responsible for the folds they observe. They also state: " Certainly, the shorter wavelength of the folds on either side of the bedrock rise is suggestive of greater lateral compression there."
→ which points towards the horizontal shortening theory.
I did not investigate the origin of the basal shear stress theory further and I had the impression that variations in basal shear stress is rather an assumption and one of many possible explanations. If there is literature that allows a clear connection to be drawn between the folds observed here and the basal shear stress approach, it would be good to discuss it here.

L333-334: *“[...] therefore the flow bands are likely to have formed as a result of differential basal conditions causing high basal sliding (and resultant ice flow speed up) [...]”*
→ My comment here is a repetition of previous comments that it is unclear to me how the change in basal properties leads to the folds in Zone 2.

L421-422: *“The englacial stratigraphy in Zone 2 demonstrates a gradual (rather than abrupt) transition from internal deformation to basal sliding at the onset zone.”*
Again, a repetition of previous comments that it is not clear to me how the change in basal properties leads to the folds in Zone 2 and I am also not convinced that the folds in Zone 2 demonstrate this transition. If so, the relationships should be explained precisely and more clearly.

L447-449: It has already been partly addressed in previous paragraphs, but for me, a conclusion of this study is that the ILCI values alone are not very meaningful. The interesting thing is what they say about radar stratigraphy when looking at low and high continuity zones. Low values can indicate folded layers, but also low resolution of the radar system, poor visibility of the layers due to high attenuation or being influenced by signals in the radar data, which are not just internal layers.

I would like to thank the authors again for a very interesting article. My many (partly repetitive) questions and comments should not be perceived too critically, but rather reflect my interest in the topic and the results.

Steven Franke
March 10th, 2023