Response to Anonymous Referee #2 (original comments in gray box)

We thank this reviewer for his or her insightful comments and suggestions which have helped us to considerably improve the paper.

General remarks

In this paper Luckman et al. investigate the origin and properties of linear features commonly observed in imaging satellite remote sensing on the Larsen C ice shelf. In the first part, using a combination of GPR, GPS and MODIS data, they deduce that these features are the surface expression of basal crevasses. The dimensions of the crevasses (along-flow extension at the surface and base and penetration height) is deduced from the GPS and GPR data. The resolution of available imaging remote sensing data lets them conclude that the majority of linear features observed in satellite images cannot be attributed to the presence of surface crevasses, but that they are either surface troughs caused by basal crevasses or rifts.

In the second part, the authors employ a linear elastic fracture mechanics (LEFM) approach to model the penetration height of the basal crevasses into the ice. Although this model generally underestimates the penetration height, the results are decent given the number of assumptions for the shelf's stress regime etc.

Based on their findings they suggest that the presence of crevasses changes the overall rheological properties of the ice shelf and the thermodynamic exchange with the ocean considerably enough so that they should be considered in ice-shelf models.

The paper is well written, data analysis is thorough in general and the structure requires little change. However, most importantly, I miss a more quantitative discussion of the implications of the authors' findings. At several instances they stress that their findings has important implications as "deep faults within ice shelves have implications for both their thermodynamic and structural properties. By increasing the area of interface between ice and water, they may enhance heat exchange with the ocean deep inside the shelf where the ice would otherwise be well insulated from external sources of heat" (Page: 2050:16.12). I would expect at some point in the paper an estimate of the true increase of ice area in contact to water for a representative region compared to the case without considering basal crevasses at all (e.g. given as a normalized estimate). Likewise, the reduction of the effective cross section of the shelf in the presence of a large basal crevasses should be provided as an order of magnitude estimate to illustrate how important the crevasses really are rheologically. Having established the depth and width of the crevasses, with their length taken from the satellite images, both could be easily manageable. Such an estimate could be i) performed for the actual "crevasse area flow lines", that is, the flow width covered by the crevasses and ii) for the complete areas of Fig.2a and b. In this sense the title's promise "implications for their global abundance" is only very tentatively fulfilled.

Since we know only about the penetration height of a few basal crevasses, and an analysis of the number and distribution of basal crevasses would be a substantial piece of work, we believe it would be inappropriate to make such an extrapolation here, rather leaving this to a more considered future paper which will look at such a quantification. Had we focussed on "implications for ice-ocean interactions", then we agree that the work might not have lived up to the title. However, the "implications for their global abundance" arises from the finding that many more features in satellite images of ice shelves are basal crevasses than perhaps were previously supposed, and this focus is not dependent on more widely quantifying the their impact in shelf geometry.

Analysis of near-surface layers

The authors use a the mean wave speed of v_{mean} =0.175 m ns^{-1} to convert travel time to depth. For the deeper features this is fine. However, this is not appropriate for the analysis of the near-surface layers and tentative conclusions drawn on their influence on differential accumulation within the troughs. Any estimate on surface density/firn column density–depth function would do a better job. The justified error of 5% attributed to the crevasse height seems to be implicitly assumed for the surface layers as well. The statement on p.2041:8.1 "would predict surface trough depths approximately 1.5 times as deep as we measure" illustrates that this is not a neglectable issue, as the density distribution of the firn layer influences the stress regime.

We agree that without suitable correction for velocity with depth, it is unwise to comment on the thickness of the near-surface layers. However, the depth of the surface trough is measured from dGPS survey, not from GPR records, so this issue does not arise. Our inferences about the relationship between trough depth and crevasse penetration height rely only on total measured thickness above the crevasses and trough depth measured by dGPS, neither of which depend on a depth-dependent correction for radar velocity. Where we have made a small comment about the near-surface layers, we have qualified them to this end.

Other specific examples where this is important:

5.4: Based on which diffractions? How large would be the effect of velocity pull-up because of different density distribution in the surface depressions? Seems not justified to apply this velocity for uppermost layers (as for instance done in Fig3), which is important to see how the surface structure changes with depths. A higher velocity in the surface depression because of more snow will deepen the trough further in the radar data.

A higher velocity in the surface depression might indeed deepen the apparent depression of sub-surface layers, but not the surface itself, which is measured by dGPS.

6.8 "follows approximately": this seems to be the case because v_{mean} has been used. However, in troughs more snow accumulates, so the density-depth fct. would be different and the shallow internal reflector needs not necessarily to be parallel to the surface but could deviate from it. This is likely most clearly illustrated with a radargram without static correction in the time domain.

7.17 This can only be illustrated with a time-depth conversion that takes the different density-depth structure into account. Deeper (older) layers may show more deformation than the surface layers because more recent layers are likely to be subject to uneven accumulation which would tend to favour the trough.

We chose to use the CMP survey that we carried out to estimate the average velocity and present the data using only this velocity because we make no calculations based on these internal layers.

Figures and space considerations: At two instances they authors argue that because of "space considerations" they choose not to show further illustration. In both cases I think it is necessary to show these figures, e.g. as a supplement if indeed not appropriate for the paper.

We have included the Landsat sub-figure that is referred to here, and now show the location of our CMP survey in Figure 2. However, we choose not to include a figure of the CMP data because this is a standard analysis step, not because of space, and we have corrected the text to reflect this.

Minor remarks

Crevasse dimensions

At several instances I was puzzled what is meant with crevasse width in contrast to crevasse length. This should be defined once in the beginning in respect to along- and across-flow directions to avoid ambiguities. Some instances where this causes an unclear context: 4.8 "10 km long" 6.11 "widths of these surface troughs"

We have clarified this early in the text.

7.10 Do you really mean "width" in terms of the along-flow dimension? The reader could only verify this by looking at migrated radargrams. The unmigrated sections in Fig.3/4 cannot be used to reliably support this statement.

We believe that the approximate crevasse width perpendicular to its faces can be adequately deduced from figure 3, as the change in slope at the crevasse margins does not correspond to a diffraction hyperbola originating from the crevasse tip. We agree that the interpretation of figure 4 is more difficult, and we acknowledge this fact on page 2039, line 20. In all cases we make only approximate estimates of crevasse widths because of the difficulties of identifying in radar data the position of the breaks in slope on the ice shelf base. Our experience shows that this is no easier using migrated data.

Other comments

Having introduced the nomenclature S1, S2, etc., the authors should keep on using it throughout the text. At several instances they switch back to e.g. writing "series 1". Using "series S1" seems more consistent.

We disagree here, the series refers to the set of crevasses, while the abbreviation S1, S2 is only used when referring to individual crevasses C1 or C2. The wording is consistent and carefully chosen in this respect.

Page: 2037

3.20 global positioning system (GPS)

3.22 MOA already contains mosaic: MODIS Mosaic of Antarctica (MOA) Image Map

Both have been corrected.

Page: 2038

4.9 flow speeds: which are? Reference needed.

4.12 long: length of crevasse or spacing?

14.12 10 ... of series S1

4.17 equivalent to a creation every 14 years at a flow speed of ...

4.26 trace spacing of 4.3 m: Uncertainty? GPS position for every trace recorded or interpolated? Where was the GPS base station? (baseline length?)

All corrected of information added to clarify.

Page: 2039

5.5 Indicate on map! How near is nearby? What is the error in the velocity based on this diffraction analysis? I'd prefer to see such a radar image anyway.

The CMP survey was located on the profile and its location is now indicated on Figure 2.

5.9 Figures 3 and 4: need labels on axes

5.18 "Assuming": If you know the GPR track and the crevasse position from RS, you know the angle. Clarify.

Page: 2040

6.8 remove ","

6.16 Rewrite sentence, awkward: "Having established that the features visible in MODIS data (Fig. 1), illustrated using Landsat data (Fig. 2) and investigated using GPR (Figs. 3 and 4) are basal crevasses,. . . "

All done

Page: 2042

8.18 I cannot see that this is a valid assumption, as the water is missing in the surface crevasse and gravity works the other way, so conditions ARE different. Please extend justification of the statement: "If we make the reasonable assumption that basal and surface crevasses have similar width-to-depth aspect ratios,. . . "

Our point here is that we expect the surface crevasses to be narrower than basal features because they penetrate less far, and a first approximation of their width can be found from the theoretical ratio of surface to basal crevasse penetration depths. The occurrence of melt can only make the ration of widths still greater. We have made adjustments to the text to clarify this point.

Page: 2045

11.11 Example required. "We choose not to show them here for considerations of space"

New part added to Figure 5.

11.21 The criticism raised here requires to show other data that clearly indicate that the features cannot be crevasses, neither surface nor basal (see 11.11).

We do not understand this comment.

11.26 "be either the surface expressions of basal crevasses," Simply calling them crevasses in previous papers is all right in my opinion, as long as no distinction has been made between surface and basal crevasses.

We do not claim that previous papers were incorrect, although we imply that they need clarification.

Page: 2046

12.23 LEFM: explain acronym again, has only been done in the intro, 12 pages away.

Page: 2047

13.16 to adapt

Both done.

13.22 What about the lateral stresses/boundary conditions to calculate the effective stress?

The approach we are using here only takes stresses into account which act perpendicular to the crevasse shoulders, as described in the text.

Page: 2048

14.22 Likely not if they all form at the same place for the same reason and are then advected downstream.

In case of Series 2 crevasses it is not possible to say exactly where they originate.

Page: 2049

15.15 This is why the introduction of the physical setting for the LEFM approach (13.22) requires more details on which assumption have been made. Would be good to provide an order-of-magnitude estimate on the effect of shear stresses on the crevasse formation.

Additional information on the relation of shear and longitudinal stresses have been added.

Thanks.