Dear Editor,
we are submitting our revised version hereby.

In the reviews, the topic marine ice was strongly pronounced. We would kindly like to emphasize that the manuscript has the title 'The evolution of the western rift area of the Fimbul Ice Shelf, Antarctica' and not 'Marine ice in Fimbul Ice Shelf' – which would be an interesting study itself. The topic is thus primarily the discussion of the rifts and not the generation of marine ice.

Both reviewers criticized the structure of the manuscript. We have worked on an improvement, but kept the structure 'Intro-Database-Synthesis-Discussion-Conclusion'. Thus we shifted all presentation of data, including the radargrams, in the synthesis section, concentrated the description of the data and figures in this section. Consequently, we reordered the figures as well.
The manuscript covers a lot of details, which we believe is beneficial for the reader. Papers with a structural glaciological approach have taken a similar route with success. We admit that it is at some instances lengthy, but there is not much potential to tighten the description without loosing information. Where we could tighten the manuscript we did that. We did our best to balance between presenting detailed information and focusing on the important issues.

We thank the reviewers for the helpful comments that lead to an improved version of the manuscript that we submit hereby. We answer all reviewer comments point by point below. You will find the reviewer comment in *gray* and our answer in black letters.

Sincerely,
Angelika Humbert and Daniel Steinhage

Anonymous Referee #1
Received and published: 6 May 2011
This paper takes a detailed look at the structure of part of Fimbul Ice Shelf, including an analysis of satellite imagery and airborne ice-penetrating radar data. The study divides the western area of Fimbul into sections, providing a detailed commentary on the structure of the ice thickness and isochrones observed in each section.
My overall impression of the paper is that it probably deserves to be published in The Cryosphere eventually but the current manuscript is an unexciting read and perhaps not the greatest advance in knowledge.
We apologize that we could not enchant the reviewer in the same way as the topic enchanted us and we hope that the efforts we put into the revised version lead to a sharpened manuscript, as well as highlighted more of the crucial points.

I was left thinking that the paper provided neither new theoretical insight into rifts (the final hypothesis for downwarped isochrones, a very interesting observation, was unconvincing) or even a well-motivated and comprehensive overview of this particular location (why should we be interested? what about the east of Fimbul? what about the modelling?).
"What about the modelling?" is a provocative statement at this location. Indeed one of the authors performed extensive numerical simulations, however, we decided that the manuscript would be totally overloaded with a description of the modelling, that would result in about the double of pages than we presented here. Thus we decided to split this and submit the modelling in a separate manuscript. We think that the

reviewer should owe us this freedom of decision.
The east of Fimbulisen is simply not the topic of this manuscript. We picked out the western rift zone, because it is a prominent area in the Fimbul ice shelf, whereas the East is rather unexciting from the structural glaciological aspect. We were particularly interested in understanding how such massive rifts could form and what are the driving factors for that. We hope to satisfy the reviewer with the incorporation of this brief section about the East.
'The eastern part of Fimbulisen, visible in Figure 1 as well, consists basically of ice with a smooth surface structure. The only exception is the area north of Tsiolkovskiy Island, where the ice flow diverges and forms a rifted zone northwards. The flow of the ice shelf in this eastern part is decoupled from the fast central part and does furthermore not affect the western rift area. Therefore, our study does cover this part.'

There are also significant deficiencies in the interpretation of the data, particularly relating to interaction of Fimbul rifts with the ocean.
We discuss this point in detail further below, as this topic appeared also in the section 'Larger points'. Here we only want to state, that we improved the discussion in the manuscript by focusing more on the references provided by the reviewer and took up points made by this reviewer.

The use of English is imperfect.
Obviously both authors are not native speakers. We have worked on an improvement of the English and will authorize the copy-edit service offered by The Cryosphere.

I would suggest major revisions at the very least.
Larger points
The paper contains no motivation. The introduction launches straight into the detail of previous work on Fimbul, without explaining sufficiently well why we might be interested in it, and in particular only its western section.
We have deleted the text about the thermal structure of Fimbulisen (1090/23 to 1091/4), which might have been some kind of distraction. The rest of the introduction guides into the area of Fimbulisen (1090/18-1091/4), explains that the western area is considerably different from the eastern, is untypical and large (1091/4-1091/13), that this rift zone influences calving of giant icebergs (1091/14-24) and what approach we chose to investigate the origin. (1091/25-1092/3). It seems to us that the reviewer's perspective is focused on the marine ice, which was never meant and never claimed to be topic of this paper.

The 'evolution' of the rift area (as alluded to in the title) is not discussed.
We disagree with this statement. A look onto the conclusion proofs that we explain the evolution of the rift zone: the first half of the conclusion summarizes, the second half discusses the evolution and even ends with 'We infer that this process is the origin of the western rift zone.' We would call this a discussion of the evolution of the rift area.

The observation of downwarped isochrones coinciding with basal crevasses is fascinating– I'm not aware of such an observation elsewhere?
We could not find any similar observations in the literature as well.

However, it is not mentioned in the abstract or conclusions and is not satisfactorily explained in the paper. It's not particularly well displayed in the figures either.
This is not true.
Abstract: 1090/8-10 'Downstream of the rumple we found down-welling of internal

layers and local thinning, which we explain as a result of basal crevasses due to the basal drag at the ice rumple.'
Conclusions: 1110/ 7-8 'Although the vertical structure exhibits strong deformation of the internal layers and also hyperbolas throughout the thickness'
Figure 4a+b(new3a+b) shows down-wrapped isochrones.
However, we have highlighted this in the new figures now, so that it is easier visible in the Figures.

The best explanation offered is that there is increased melting within crevasses, but it is an observed fact that the ocean freezes in Fimbulisen crevasses, rather than melting them (e.g. Khazendar & Jenkins JGR 2003).
We like to emphasize, that the area where freezing is observed and modelled is an ice melange (Zone22/new1) and not a zone with single crevasses. It is definitely not comparable to the area where we observed down-welling of the layers. Figure 4b(new: 3b) shows where the down-welling is observed and the comparison with Figure 7(new:5) where the ice melange is located.

It is possible, I suppose, that higher melting occurs on the sidewalls of a rift than under the 'flat' ice outside crevasses, and this might downwarp the isochrones. If this is the case, then 'older' crevasses (further downstream from the rift) should have more downwarped isochrones, since the melting has been going on for longer – it doesn't look like that is the case but it is hard to tell from the figures?
In case melting on the steep sidewalls of a rift would simply remove mass but not alter the position of isochrones in vertical, as the thickness of the ice shelf does not change. Whereas melting on the base would cause a change in the vertical of isochrones, because of the buoyancy of the ice.

It is also possible that simple ice dynamics are responsible for the downwarping – e.g. Leysinger-Vieli et al Ann. Glac. 2007.
This is a very interesting point (whereas 'simple ice dynamics' is a somewhat wide term, it is definitely ice dynamics that is responsible, what else?). A definite answer can likely only be given with a 3D full-Stokes model, including a linear-elastic and linear-elastic fracture model in the vicinity of the rumple, for an area of about 30x30km^2 around the ice rumple. The sliding+melting examples in Leysinger-Vieli and others (2007) are comparable to some extent to the situation here, as the plug flow that these authors denote as 'sliding' is the flow regime in an ice shelf. However, we neither have a pure sliding + melting, nor a pure 'channel', we have 'internal deformation' over the rumple, 'sliding' around and a 'channel' in the lee zone of the rumple (to keep these authors terminlogy). However, profile A to A' is relatively far away from the 'channel' and it seems to us unlikely that the channel effect is relevant at this location. That the removal of material at the base causes the layers to plunge is exactly what we have suggested in the manuscript. This is indeed simply ice dynamics – nevertheless not comparable to Leysinger-Vieli et al., as that ice is not floating, which is an additional contribution here.

Can the opening of a crevasse cause a downwarping due to ice divergence in the lower part of the ice column in the same way as the transition from sticky to slippery basal drag causes a downwarping, I think due to horizontal divergence of ice that increases with depth? That would give a downwarping that happens only on crevasse formation, so the downwarping remains constant with crevasse 'age' (distance downstream) which is agreement with the observations as far as I can see.
In the reference mentioned by the anonymous reviewer, Leysinger-Vieli and others (2007) investigate influence of various scenarios (changes on flow mode, basal melting and flow convergence) on isochrones using a simple model for grounded ice. The model deduces the velocities based on the mass fluxes. As basal melting at the

base of the ice shelf removes mass, the model is not applicable to an ice shelf, even though the model provides a valuable insight on the response of isochrones of grounded ice on varying basal conditions.

Another apparently key observation is that the isochrones converge vertically. I can't see this in the figures.
We provide the reader now in Figure 4b(new: 3b) with two tracked layers in one radargram and display the distance between two layers along the flight-direction.

Similarly, what are the hyperbolae high up in the ice column?
We suppose that they are the tips of basal crevasses.

Do the figures show an example of one anywhere?
Yes, Figure 4a(new: 3a), the radargram from A to A' shows hyperbolas at a high location in the ice column, as well as the map in Fig.3b(new: 4b).

I would suggest that example figures need to be added showing detail of downwarping, isochrone convergence, and hyperbolae at different depths.
The isochrone convergence is now clearly visible in Fig.4b(new: 3b), profile B to B', even highlighted in color. In this figure the downwraping is also well visible, as now two of the layers are colorized. The internal hyperbolas at different depth are superimposed on section A to A' now and a colored bar denotes where basal hyperbolas exist.

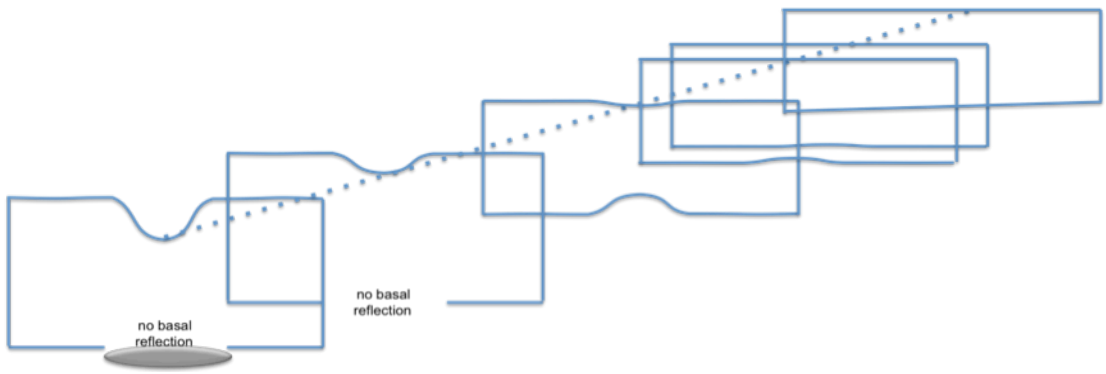
The existence of ocean-sourced marine ice in Fimbul (Khazendar & Jenkins) is never mentioned in the paper, despite its ability to explain many of the observations.
We don't see how marine ice explains our observations and the reviewer does not clarify which observations it would explain neither. It is hard to believe that the reviewer claims that marine ice explains the stress state that creates the rift system, nor the formation of single rifts and surely also not the subsequent deformation and propagation of the rifts. '[...] despite its ability to explain many of the observations.' is thus in our point of view an assertion. Therefore, we would like to emphasize here again, that this study aims to investigate the evolution of the rift system.
Although we did not cite this reference in the original version (we do this in the revised version) Khazendar & Jenkins refer to a site called Jutulgryta (former Zone 22, new Zone 1), an area formed when the ice stream just passed a confining valley and became afloat. This area consists partially in summer even of open ocean, fragments of meteoric ice and sea ice, which was used by Orheim et al. (1990a,b) for an easy access to the ocean, after the first drill attempt through the main, thick, part of the ice shelf failed. This area (#22 in the original version) is not comparable to the area where we observed the downwelling of the layers. Using common terminology (as we did in the manuscript), we would call this area an ice melange.

Although Jutulstraumen has a deep grounding line and was previously suggested to experience high melt rates (for a quick overview see Humbert, 2010 which displays her basal melt rates from steady state assumption, the basal melt rates from Smedsrud et al., 2005 from ocean modelling and Smith, pers comm, 2009, steady state), there is no evidence for a thick marine ice layer like the large ice shelves exhibit. The estimation of the mean density from hydrostatic equilibrium using the ICESat DEM and the various ice thickness measurements, including the extensive survey of Nost, 2004 and the one presented here, give not indication for a considerably thick marine ice layer. If marine ice exists, it is unlikely to play a role in the evolution of the western rift zone, which we investigate here.

However, there is one kind of freezing which we indeed believe to happen and to be

relevant: when basal crevasses form, they cut into cold parts of the ice shelf, which are in case of Fimbulisen as much as tenth of degrees colder than the -2°C at the base (see Humbert, 2010). The crack propagation is a fast process, on the time scale of seconds. We believe that the newly formed walls of the basal crevasse will be covered with marine ice, frozen directly at these walls and also at the crack tip, which reduces the load at the crack tip, prohibiting propagation for some time.
We have included a paragraph about this in the manuscript.

We worked through the radio echo sounding data set again in order to find indications for the existence of marine ice:
The lee-zone downstream the ice rumple (Zone 8) is crossed by several radar profiles (only the ice thickness data is shown in this study, Fig.5b(new: 6b), as they are obtained using a 600ns pulse). The figure below sketches the situation as we found in the radar data. The basal signal is lost in the vicinity of the ice rumple, whereas 14km further downstream the basal signal appears again. A plausible explanation for this is a rough ice base across the ice rumple and in its vicinity, leading to the loss of the basal signal. The downstream retrieval of a basal signal requires a smoothing of the ice base and we suggest that this is due to melting. The surface elevation over and in the vicinity of the ice rumple in Zone 8 is lower than that of the surrounding ice in the west (Zone6) and east (Zone9). Downstream the ice in Zone 6 and 9 thins and the surface over these three zones levels more and more out. However, the base of Zone 8 remains downstream higher elevated compared to Zone 6 and 9. This basal trough is at most in the order of 10-20m. In case this trough is filled with marine ice, the surface should be at the same level as the one east and west of the trough. An accuracy of the surface elevation <0.5m is required to clarify if the hydrostatic equilibrium requires the accretion of marine ice. Our accuracy is unfortunately not sufficient. Consequently, we have to conclude that we don’t find any evidence for marine ice, but a 10-20m thick marine ice layer in the northern part of Zone 8 would be consistent with the observations we made.



In particular, I suspect that units 18 and 16 host large deposits of marine ice. They have no basal radar return and originate in shallow ice downstream of a peninsula between two flow units, which is the classical location for marine ice formation. See e.g. GRL paper by Holland et al 2009.
We agree with the reviewer that unit 18 and 16 could contain marine ice (we actually stated that already in the description of Zone 18 that it is a mélange, a term that includes sea/marine ice). However, we disagree that this is the reason for the loss of the basal signal. Radio echo sounding on the Filchner Ronne Ice Shelf showed a clear reflector at the meteoric/marine ice transition, as this boundary demarcates a jump in dielectric properties. An extremely rough base is the most likely reason for a loss of the basal signal. It might well be that zones 16 and 18 contain marine ice formed by a plume, but we think it is incorrect to infer marine ice from the loss of the

basal signal.

The marine ice couples the flow of Jutulstraumen to that in the west.
This is an assertion and there is no evidence for this. Where the radar retrieved a basal signal, the ice thickness is not much different to that east of this zone. The ~250m meteoric ice that the radar observes is the major part of the ice column and marine ice will amount to much less. There is no reason why the distinctly thinner marine ice (we don't see a surface bump over Zone 16) should be the part of the ice column that couples the flow of Jutulstraumen to that in the west.

P1105, L7-8 mentions that thinner ice implies greater melting – why? I would suggest that basal freezing might be filling in the crevasses.
The reason for this is the following: a crack is a narrow feature. Even wide crevasses are not hundredths of meters wide. If one compares the scale at the x-axis given in km, one sees that this is not the top-down version of a surface crevasse with steep walls. This let us suggest that melting plays a role. However, we neither have a proof for melting, nor for freezing. This is an argumentation based on plausibility. The freezing that the reviewer suggests might play a role, but the modelling of freezing by Khazendar & Jenkins (2003) and Holland et al. (2008) requires ISW, which itself requires melting somewhere. One could consider that the lower parts of the basal crevasses melt (consistent (!) with the findings of Khazendar & Jenkins, 2003) and create small amount of melt water available for frazil ice formation. If the frazil ice rises due to buoyancy, it is very likely that it will be transported along the main axis of the basal crevasses towards the lee zone of the rumple, Zone 8, which is the thinnest and thus highest location of the base. In this zone we could not find evidence for marine ice, as described above.

I don't really see the point of setting up hypotheses i-iv on page 1104 when it is obvious from the start that most of them cannot work. The hyperbolae imply crevasses, which rules out ii and iii immediately. I don't understand how adjustment to hydrostatic equilibrium would make layers sink, since surely a region thinned equally from the top and bottom would move up, not down? That rules out hypothesis iv so I think you could just state your final hypothesis to begin with.
This was ment to be some kind of wrap up of all hypothesis one could consider. We are flexible and shorten this if the editor requests, but if we are not short in space, we probably won't loose much if we keep it.

Smaller points
Throughout:
looses -> loses
builts -> builds
hyperbolas -> hyperbolae
alteration -> alternation
downwrapped -> downwarped
All these corrections were made.

bended -> bent
Done.

Page 1090, Line 4: Fimbulisen
Done.

P1094 L12: why use 8.8m? At the very least a reference is needed.
The transformation from two way travel time to ice thickness was made by using a radar velocity of 0.17 m ns-1 plus 8.8 m in order to account for the higher velocities in

firn, following the approach by Blindow (1994) and adjusted for the relatively high velocity reported by van Autenboer and Decleir (1969).

P1097 L20: why leap to figure 7(new: 5) here? Please re-order figure numbers to be in the order they are referenced in the text.
We will do this for the final version in coordination with the editorial team.

P1097 L26: I think you mean perpendicular to the flow?
Yes, indeed it is perpendicular to the flow. Changed accordingly.

P1098 L7-12: I think there are 4 inaccuracies here. The ice thickness does vary across the grounded area in your figure, but this doesn't mean it isn't an ice rumple! Similarly, an ice rumple is overflown by ice, and your cracks do not imply that it is not overflown.
We wrote that the ice thickness does not vary *significantly*. We wrote that the small cracks indicate that it is overflown. We don't see how the reviewers argument match the text of the manuscript.

P1098 L20: alternate
Done.

P1099 L13: what is the relevance of tidal displacement?
The tidal displacement forms a hinge zone. However, the formation of the zone is due to outflow and not due to the tidal displacement, thus we have reworded the sentence to 'The second zone is most likely originated from outflow from Novyy Island into the ice shelf.'

P1101 L8: what does 'is adjoining northwards' mean?
We have reworded this to 'North of Zone18, a region representing a typical shear-margin begins.'

P1104 L13: becomes critical
Done.

P1106 L20: parallel to the flow?
Yes, indeed it is parallel to the flow direction. Changed accordingly.

P1107 L14: surely the difference between properties east and west of the ice rumple is just due to the different stress regimes, not to any difference in the homogeneity of the flow units?
In the line the reviewer mentions the lateral extent of the western and eastern zones adjacent to the ice rumple is discussed. We stated that the eastern zone is smaller because an inhomogeneity arose at the grounding line. This inhomogeneity blocked the propagation of the basal crevasses and is similar to the findings of Hulbe et al., 2010, which the second reviewer proposed for citation. We are confident that the width of the eastern zone is not determined by the stress regime.
Nevertheless, the text has been extended to highlight the connection to Hulbe et al., 2010.

P1108 L1-3: I can't see this in the figures.
The shear wing cracks are the most prominent features and are surely a kind of eye-catcher in Fig2a and Zone10 also highlights the shape in Figure 7(new: 5) (numbering according to the TCD article).

P1108 L9-11: why would lateral stretching couple the flows?
Because it builds up compressive stress.

Figure 2b: The colouring of ice thickness is very poor and as a result it is impossible to tell whether the arguments made in the text are true. Ice thickness is highly relevant for some of the arguments, and the airborne data are quite dense, so I think the authors should produce a grid of ice thicknesses and include a figure showing it with a full colour scale.
The figure is now provided with a rainbow color scale.

Figure 7(new: 5): It is very hard to make out the flow units, so this figure should be replaced with a zoomed version, like in figure 2a. Also, the numbering is strange – what happened to units 1-3, 23?
We have renumerated 22 to 1 and 24 to 2, as well as introduced the rumple as Zone 7, thus the numbering is now continuous.

Referee #2 D. Jansen

Structure
I think by giving the manuscript a clearer structure the authors would make it easier for the reader to access all the information. The parameters on which the classification is based are described in detail in the text. A table or a structural diagram of the single classes/areas and their properties would help to visualize this information and keep the text concise.
We discussed that suggestion lengthy and also talked to the reviewer. It appears that whatever kind of visualisation one chooses, a table with small images of the features, a map with small images of the features and annotations, it will either be lengthy, e.g. the table over two pages, or requires a large map, definitely larger than A4. We have prepared for ourselves during the data analysis a catalogue like this, which is four pages long. If the editor insists, we will provide such an overview, but we would prefer the strategy to add annotations in the radargrams, as we did for the new version.

The manuscript would also benefit from focusing on the zones which are discussed later as the key areas for the western rift system. I appreciate the completeness of the presented approach, but it is at times difficult to link the discussion to the figures.
We see the point the reviewer makes here and partially agree. This manuscript was meant to provide comprehensive information on all these areas, as potential readers could access by this information over all areas without having themselves access to the database. One of the authors (A.H.) made the experience, that modelers' for example benefit from the wealth of information experimentalists have access to, but often do not incorporate in the papers. The information remains accessible for the observer only. Therefore this author was the driving force for the compromise of describing and at the same time avoiding detailed figures. This leads us to the suggestion to the editor to keep the entire description in the manuscript. We are however flexible and would select key areas on editors advice.

Radargrams
It would be helpful to highlight one or two of the layers in colour to visualize the amount of distortion. It is difficult to follow the single layers in the figures with the resolution provided.
We have have highlighted two layers in a section of profile B-B'.

Arrows could highlight the features discussed in the text in all radargrams and be

linked to distinct marks on the satellite imagery, for example one particular dark stripe.
We pointed out certain features in the radargrams and added explanatory text to the figure captions.

Page 1106, second paragraph. To me it is not obvious from the figures that strain thinning is different above crevasses from figure 4a(new:3a). It would be helpful to highlight this or zoom in on one region.
We highlighted sections of two layers in a section of profile B-B' to point out the thinning of the ice between layers.

The radar profile A to A' cannot be 120 km long. Is there a factor two in here? The same is true for the profile from B to B'. This is essential because it appears to have lead to a misinterpretation of the data:
The reviewer is right, the length of the profiles are stated incorrectly in the figures due to a mistake by the calculation of the distance between the respective geographic coordinates for the three examples presented in the paper. We have corrected all three figures. The interpretation of the data has been done with proper length scales.

Page 1105, last paragraph: To me it seems as if the hyperbolas start right at the position where the stripy features are visible on the imagery. Again, it would be very helpful to indicate examples for the discussed features in the figures. Profile C seems to be ok.
We have plotted the profile now in Figure 2a and marked the hyperbolas in the radargram. However, the location of all(!) hyperbolas are shown in Fig. 3b(new: 4b).

Down-welling of radar layers
Most importantly: How is the radar data corrected for topography?
If the hyperbolae are caused by basal crevasses, the dark stripes which are also visible in optical remote sensing data could indicate surface troughs, which can amount up to several meters in depth. Ignoring this might lead to an apparent distortion of layers which becomes stronger with depth.
The RES data were recorded at a constant flight level. Therefore the profiles show the ice shelf in its correct position and no additional static corrections are required except of leveling of the profiles to a common reference height. But a constant shift a whole profile does create any artifacts, which could be misinterpreted i.e. as downwelling layers.

The B profile cuts across the smooth ice zone downstream of the ice rumple (zone nine). To me it looks like if the extreme disturbance of the layering in the first half of the profile coincides with the boundaries of this region. On optical remote sensing data (e.g. Moa) this area appears to be under compression and is narrowing (distance between stripes on either side gets smaller). Could lateral compression perpendicular to the flow direction be a reason for the strong distortion?
In order to assess if this could be due to compressive stress, one has to take Zone 15 (and even 17) into account. In Zone 15 compressive stress leads to the bending of the fibre-like blocks and becomes a shear stress dominated area at its northern end (which is continued in Zone 17). Profile B cuts from the west through Zone 6, 8, 9 and 15, but 9 and 15 are from kilometer ~16-17.5 and ~17.5-22 respectively, thus a short section. Zone 9 is indeed slightly narrower northwards, however Zone 8 keeps its width, with even a tendency to widen (maybe the SAR imagery is required for this). This let us assume that Zone 6, which shows is not affected by compressive stress arising from Jutulstraumen and that Zone 15 is the zone that experiences the compressive stress in the south and shear stress in the north. It is worthwhile to

mention that Zone 9 is not much smoother than Zone 8, it is just narrower.

I also do not understand the argument that basal melting could occur in the crevasses, deepen them, and lead to further distortion of the radar layers. The pressure dependence of the melting point would rather suggest freezing under thinner ice.
The melting/freezing issue is discussed above intensively, so we answer here only the question about the distortion of the layers. The distortion of layers can have to processes to contribute: strain and thinning. We referred here to thinning. Let us conduct a gedanken experiment in which we have equal distance between layers in the vertical and no strain. If you melt away ice from the base, the surface and the layers sink – just due to hydrostatic equilibrium – the distance between them would remain constant.

Additionally:
A rough ice shelf base is not the only explanation for the lack of a basal reflector in the radar data. Especially downstream of the Ahlmannryggen the reason could be accumulated marine ice (Holland et al, 2008).
We don't see the point how marine ice could lead to loss of the meteoric ice/marine ice transition. In the Filchner-Ronne Ice Shelf and in the Amery Ice Shelf the meteoric/marine ice transition surface was retrieved with the radar. We don't doubt the existence of marine ice in the zones mentioned in Holland et al. 2008, but we believe that the basal roughness is the reason for the loss of the radar signal. If one looks carefully into Fig1 of that paper, one find that there are locations where the ocean modelling suggested marine ice is not uniquely coincident with loss of the basal signal and additionally, the basal signal is lost in areas where no marine ice is suggested, but the satellite imagery exhibits rifts.
Furthermore, we have profiles across Zone 18 and 16, where we have a basal reflection – the area where we have most likely marine ice exists and even in Jutulgryta, where the modelling (Khazendar and Jenkins, 2003) and observations (Orheim 1990) proofed marine ice to exist. These findings are inconsistent with the assumption that marine ice leads to the loss of the basal reflection.

Page 1107, second paragraph: See Glasser et al. (2008) and Holland et al. (2008), also Hulbe et al. (2010), might be worth citing one of these.
We have included these references in the new version of the manuscript. Holland et al. (2008) and Khazendar & Jenkins (2003) is cited in the new paragraph about marine ice. Hulbe et al. (2010) is included in the discussion of Zone 9.