Response to Reviewer 1 for: "Large-scale englacial folding and deep-ice stratigraphy within the West Antarctic Ice Sheet" (MS No: tc-2019-245)

We are grateful to both reviewers for their constructive and helpful reviews of our manuscript. Below we respond (non-highlighted text) to the comments of reviewer 1 (highlighted in grey).

Anonymous Referee #1

General Comments:

This paper aims at explaining observations of englacial folding in the lower ice sheet column obtained from radio echo sounding. The deep-ice unit is analysed by mapping anticlines using a high-resolution grid of radio echo sounding data from 2010/11. Analysing the returned radar power at certain cross-overs allows to evaluate if englacial layers show signs of anisotropy. Evaluating different hypothesis explaining the formation of such complex near basal structures such as subglacial freeze-on, varying ice rheology and entrainment of basal material, the authors conclude that the deformation and folding of the near basal unit is related to convergent ice flow and layers of anisotropic ice. The paper shows a nice and extensive data set of radargrams and returned radar power at cross overs. However it is rather difficult to orient oneself between the different figures to really understand how the data is related to the ice flow. The different hypotheses are discussed but I feel that the evidence to choose one hypothesis over an other is lacking in parts. I'm not sure what the goal of the paper is - presenting the large-scale englacial folding or resolving how such a structure is obtained. I don't think the latter point is achieved.

We thank reviewer 1 for their review and helpful suggestions to improve the manuscript. We very much appreciate the description of our "nice and extensive dataset of radargrams". We believe that this dataset is unique for a modern SAR-processed radio-echo sounding survey in Antarctica. There are certainly few airborne RES surveys of an appropriate orientation and such closely spaced survey lines across the onset zone of a major Antarctic ice stream system like the Institute-Möller ice stream complex.

We would disagree with the comment that it is "…rather difficult to orient oneself between the different figures to really understand how the data is related to the ice flow…", as we have provided maps of ice flow in four of the six figures, whilst the location of figure 3d (figure 3 is one of the two figures that does not have its own ice flow map) is shown on figure 1d, where ice flow contours are shown above a map of bed elevation. However, we are happy to adopt many of the later specific suggestions from reviewer 1 for our figures (i.e. labelling tie and across lines, adding the location of radargrams in figure 2 to figure 1a, and having additional area boxes in figure 1). We hope that accepting these should also address reviewer 1's general comment.

Reviewer 1 states that they are "not sure what the goal of the paper is - presenting the large-scale englacial folding or resolving how such a structure is obtained". The primary purpose of the manuscript is to report the discovery of the large-scale englacial folding AND the unusual deep ice stratigraphy (i.e. extensive thick high reflectivity layers at depth) across the study area. Secondary goals of the manuscript are to infer the physical properties of these folds and layers; to develop hypotheses for how the layers, and associated structures, might form; and to assess any potential glaciological impact. Whilst our "nice and extensive dataset" can be used to identify the englacial structures, and, in combination with ice velocity datasets, can be used to describe its spatial relationship with ice flow, we acknowledge that it does have its limitations. Whilst we can infer some physical properties (i.e. strong preferred crystal fabric) from the data, we lack direct measurements of physical properties (e.g. from ice cores corresponding to the airborne RES survey) to 'calibrate' the radar data, so we are unable, at present, to resolve "how such a structure is obtained". However, from the available datasets, and with reference to glaciological theory, combined with existing observations elsewhere in Antarctica and Greenland, we have been able to develop initial explanations for "resolving how such a structure is obtained". Full determination of the processes responsible for the englacial structures is beyond the current dataset and therefore this paper, however, and will need to await future direct investigations and observations (e.g. coring and geophysical measurements) designed specifically for addressing this question. We note that the current RES dataset was not designed, or acquired, to answer such a question; it was designed to test for the presence or absence of subglacial sediments beneath Institute Ice Stream, and the discovery of the englacial structures and layers was serendipitous. Given this context, we would argue that we have successfully reported the englacial layering and structures, inferred possible physical properties, proposed possible processes responsible for fold formation, and assessed the glaciological implications of the folding. That we have not fully resolved "how such a structure is obtained" is not, in our opinion, a major issue. We have undertaken the best assessment and interpretation of the evidence possible, given the data currently at our disposal.

Specific Comments:
- Line 4: this process might also involve freeze-on of basal water?
We acknowledge that local freeze-on of basal water is possible, but the widespread and extensive nature of the deep-ice facies we report is inconsistent with the freeze-on of basal water being an important process in its formation. The sentence in line 4 reports that we observe little evidence for basal water within our study area (e.g. there are few definite subglacial lakes) as detailed later in the manuscript. We therefore propose no change here. Additional discussion of this issue is outlined in our comments on lines 134-140 below.

- Line 18: the citation of Dow et al. 2018 is not entirely correct in this context - as the impact of freeze-on units on ice-sheet flow and dynamics has not been investigated by that paper as they use a subglacial hydrology model and not an ice-flow model.
We propose that we simply remove the reference to this paper at this location (though we retain it elsewhere in the manuscript).

- Line 27-28: This sentence 'Like a structural geology problem, ...' is very assertive statement and I wonder why you are so sure about this. Does this come from the literature (but then references are missing) or does it come from your own findings (then say so)? Here in the introduction it seems to be at the wrong place.
We propose inserting references to papers by MacGregor and others, JGR, 2015 https://agupubs.onlinelibrary.wiley.com/doi/10.1002/2014JF003215 and Hudleston, Journal of Structural Geology, 2015 https://www.sciencedirect.com/science/article/pii/S0191814115300365 here. Both these papers draw attention to similarities between ice sheet englacial structures and structural geology, and their inclusion should hopefully address reviewer 1'a comment.

- Line 57: You mention the ice thickness in this sentence but not how thick the lower-ice column is. It is mentioned in the abstract but here would be a good place too.
We will insert a value or range for the thickness of the lower ice column here.

- Line 59: I think not only Figure 3 but also Figure 6 could be referred to.
We agree. We will add the reference to figure 6 here.

- Line 60-61: R2 is referred to as a band of prominent reflection that "sometimes diverges and bifurcates, becoming a series of 3-4 layers". Is it the band that diverges and bifurcates or rather the series of layers within? I suggest a clearer wording.

We propose rewording lines 59-61 to: "Beneath R1, within the low-reflectivity lower-ice column, a second highly reflective englacial reflection (R2) is observed. R2 sometimes diverges and bifurcates, becoming a series of 3-4 layers (Figure 2b)."

- Line 64: I'm not sure if instead of Figure 2a you rather mean Figure 1d? It is not clear where exactly you refer to in Figure 2a - which tributary and wich lateral boundary (I guess in the center - parallel to K'K and J'J)?

We will refer to figure 1d instead of 2a.

- Line 67: Can you give examples where the amplitudes of up to 40% are seen? Maybe highlight in one of the Figures so that it is clear to what you refer to.

We will annotate this on figure 5a.

- Line 69: For the statement that rather bed conformable undulations than folds are found along flow Figure 2b-c might not be the best example. Comparing the transects where the anticlines are marked (Figure 1d and 3a-b) it is obvious that the chosen along profile in 2 is not crossing many marked anticlines. Along flow one would expect not to see much of a fold that is oriented along flow (e.g. as shown in Figure 4 it is difficult to see the along flow pattern).

The purpose of referring to 2b-c here was that these radargrams demonstrate that there are no folds or overturnings in radar data acquired in that orientation. They were presented as typical examples of the survey data and show that the deep reflective layers are very extensive (i.e. over distances of ~250 km or more) and bed conformable. If referring to figures 2b-c in line 69 is an issue we can simply remove reference to 2b-c, as 3c demonstrates the same point just not over such a large spatial area. However, we would prefer to leave line 69 as is. The evidence for bed conformable layering down flow is inconsistent with localised basal freeze-on being behind the formation of the deep ice units. Such a process could not explain this layer stratigraphy and structure.

- Line 93-94: This surface flow stripe is not visible in any of the figures (neither main text nor supplement). It would be nice to see the mapped fold together with the flow stripe.

The position of the surface flow stripe is mapped in figure 5a, but we propose adding an additional subplot (comparable to size and area of 5a) to figure 5 that shows surface imagery of ice stream trunk (e.g. RADARSAT) with the flow stripe visible. This will provide the 'raw data' of the surface flow stripe requested by reviewer 1.

- Line 97: Could you highlight where the "significant shift in basal reflectivity" is seen? This would make it much clearer for the reader what you mean.

We will mark/annotate 'bright' and 'dim' areas of the ice sheet bed on the radargrams in figure 5.

- Line 98: Same as above - highlight the onset zone of IIS so that it is very clear to what you are referring to.

We will annotate figure 5a with this information.

-Lines 102-103: From the text/figures it is not clear to what exactly you are referring to. E.g.: - where is "grid-SW margin" to be found? - where is the ice plain? Mark/label it so that it is clear. I can guess but rather would like to be sure. Add these explanations to the overview Figure 1.

The ice plain is currently located just beyond the grid-north limit of 5a. We will expand the area of figure 5a to include it. We will also annotate/map the ice plain on the figure.

- Line 113: I'm not sure that the observation of no high relief in basal topography is enough to say that anisotropy is not forming in soft layers due to enhanced stresses on the stoss-face of basal hills. One

can imagine that if locally basal freeze-on exists, then the plume of accreted ice pushing into the ice columne might act as an obstacle to the meteoric ice flow.

We propose to simply delete the sentence at the end of the paragraph that dismisses this idea (i.e. lines 112-113), and that starts "We do not observe…..".

- Lines 133-134: The usage of deep-ice units in this sentence is not clear as it seems to be used in two different contexts: 1) as the basal freeze-on units of accreted ice from the bed raising into the ice sheet as hypothesised by Bell et al, 2011; and 2) as the unit of ice between R1 and the bed. So I wonder what in this paper is understood by the "deep-ice units". It is defined as the ice between R1 and the bed. I don't think you expect the whole unit to be frozen on by basal water. With basal freeze-on one would expect the to have some local areas where "material" is rising into the ice sheet and advecting downstream - shaping the meteoric layers above.

We confirm that for our study area, the "deep-ice unit" is the body of ice between R1 and the bed. To limit confusion in lines 133-134, we propose to substitute "deep-ice unit" with "basal ice" when referring to the paper on basal ice features in East Antarctica by Bell et al. 2011. The sentence would then read: "The 'freeze-on' hypothesis for the formation of basal ice (Bell et al., 2011) cannot explain the deep-ice unit and incorporated layers (i.e. R1 to the ice sheet bed) for four reasons:…….".

- Lines 134-140: The arguments against basal freeze-on, of slow-flowing and thin ice likely frozen to the bed, as well as fast-flowing ice over a wet bed, do not entirely exclude the formation of basal freeze-on plumes. In order to exclude basal freeze-on the possibility of water needs to be excluded. Where is the evidence that there is no water at the base of the ice-sheet? Is it visible in the radar data? It would make sense to map the anticlines together with the main topographically-constrained subglacial drainage network to evaluate if basal freeze-on is possible. Further the ice velocity itself cannot exclude the formation/existence of substantial freeze-on units.

We can include an additional figure of the mapped anticlines with the subglacial hydrological drainage network in the manuscript if required. However, even if we did this it could not explain or dismiss the widespread presence of the strongly reflective deep ice layers across the study area (see lines 52-55 of the manuscript), even in areas that are clearly underlain by cold based ice.

With regards to the questions "Where is the evidence that there is no water at the base of the ice-sheet?" and "Is it visible in the radar data?" we refer to lines 139-146 of the manuscript, where we describe the limited evidence (in our, and other, RES data) for "significant ponding of subglacial water" in the study area.

- Lines 147-149: I don't understand the argument that because R1 and R2 have a consistent stratigraphic position "the structures and extent of the deep-ice structures must be the result of the deformation and localized folding of meteoric ice". How is this meant - a stratigraphic position in the vertical ice column? They will always be on top of each other - unless one is melted away. E.g. basal freeze-on would lead to the same result just pusing up the entire ice column - with the vertical distance between the layers diminishing.

It is true that basal freeze-on would lead to the same result by pushing up the entire ice column, but it is physically unrealistic for this to have occurred over such a widespread area as we observe R1 and R2. The volumes of water required would be, quite simply, enormous and freeze-on rates would have to be consistent right across the study area. We might expect freeze-on locally (and we do not dispute the possibility of localised basal freeze-on), but not over the extensive area that we map the layers (see Lines 52-55 and Figure 1 – noting that there is an error in the mapping of R2 in figure 1b which significantly under-reports its spatial distribution. We will rectify this in any revised version of the manuscript). In addition, freeze-on cannot be responsible for the formation of the basal ice layers over high subglacial mountain ranges, e.g. the Ellsworth Subglacial Highlands, where the ice is cold based and has likely been for millions of years. Therefore, the only physically realistic explanation for the consistent stratigraphic position of R1 and R2 in the ice column, over such a vast geographical area, is that they reflect layers that were initiated at the surface by meteoric deposition (i.e. of snow and/or volcanic ash), but that will have undergone physical modification (i.e. to crystal fabric orientation) in the ice column during burial and strain. We do propose that figure 1 (in addition to figure 2) is referenced in line 147 however, to direct the reader to the extensive geographical distribution of the deep ice layers.

- Line 148: What exactly is your deep ice structure? In parts it sounds that it is all the ice below R1? Or do you mean the layer pattern at depth? Is there a difference between units and structure?

By deep ice we mean all the ice between R1 and the bed. Units are the layers of ice (like geological units), the deep ice structure refers to the geometry of the units (e.g. folds) in the deep ice.

- Lines 149-151: It is not clear to me how the anisotropy observed in a layer leads to the folding of the ice unit below. In the paper by Bons et al., 2016 it is not one anisotropic layer that leads to folding but a body of anisotropic ice.

In response to a comment from reviewer 2, we propose to amend these lines to: “Further, given the radar anisotropy observed, the most likely explanation for the folds is that they are caused by a combination of convergent ice flow and the distinct physical (i.e. crystal orientation fabric), and consequent rheological, properties of the band of ice associated with R1.”. As is clear from figure 6 (and multiple other examples in the supplementary information) R1 is a broad band (described in line 58 as “up to 200 m” thick) that is rather different from the narrow reflections we typically think of as englacial layers observed in RES data. As such, R1 is more a body or package of ice than a single layer (and therefore potentially like Bons et al, 2016). Furthermore, we do not know the properties of most of the ice below R1 as, unlike in Greenland, as we do not have direct observations (e.g. from ice cores) to calibrate against. We can infer from the RES data that R1 is anisotropic, and that R2 is likely isotropic. However, we do not know the physical properties of the rest of the deep ice units as it is typically an echo-free zone below R1. It may be anisotropic, either in places or throughout, but we cannot determine that from the currently available data.

- Lines 169-171: Why would the spiraling flow of basal ice only happen at that specific location and why only on one side of the ice stream onset?

We propose the deletion of lines 169-171 and the reference to Schoof and Clarke 2008.

- Lines 173-176: "strongly contrasting physical and rheological properties of glacial and interglacial ice" might not be the only explanation that basal units are" folded, sheared and overturned".

We accept that englacial layers in Greenland and Antarctica can be deformed by the freeze-on of basal ice (which we assume reviewer 1 is alluding to here), but based on the currently available literature for Greenland (which we cite in lines 175-176) the most widespread accepted explanation for such folding is the strong contrast in physical and rheological properties between glacial (‘soft’) and interglacial (‘hard’) ice. We do not see the need to alter this sentence.

- Line 203: I do not agree that you "demonstrated" that the deep-ice units have different physical properties than the ice above. You showed that one band does show in some places anisotropic behaviour.

We propose rewording this sentence to: “We have demonstrated the presence of an extensive package of deep-ice units beneath the Institute and Moller ice streams. At least one layer in the deep ice has physical properties (i.e. ice crystal orientation fabric, rheology) significantly different to the upper ice column.”

- Line 204-206: I'm not happy with the statement in this sentence. The paper does not show that convergent flow heavily deforms deep-ice units, that this process leads to the formation of large-scale englacial folds and that these folds modulate ice-stream position structure and dynamics.

We propose rewording this sentence to: "At the lateral boundary of the onset of enhanced ice flow of IIS, where ice flow is convergent, these deep-ice units have been heavily deformed. Deformation has led to the development of large-scale englacial folds that may modulate ice-stream position, structure and dynamics."

- Conclusions: The conclusion seems to me in parts too assertive. In my view the study is not really conclusive how this structures are really formed. I agree not so much with the top part but agree with the bottom half. It is still not clear what exactly is meant by deep-ice unit.

We suggest that we have addressed this issue with changes we propose to lines 203, and lines 204-206 (see above) that are less "assertive" than they were. We acknowledge that our study may not be conclusive as to how the structures formed, but that reflects the serendipitous nature of the discovery and the datasets currently available. More conclusive understanding will require additional work (i.e. new geophysical/ice core observations and/or numerical modelling) beyond this 'discovery' paper. As stated previously, the deep ice unit is all the ice between R1 and the bed.

- Figures 1: The figures in the paper are not very well related to each other. It is difficult to see where the transects are taken relative to ice velocity, bed topography surface slope and the mapped anticlines. I suggest that the boxed area of 1d is not only shown in 1a but everywhere in 1. Ideally 1d would show bedtopography upstream of the section in 1d. Help reader to orientate by defining the orientation of the grid used in the figure.

We can certainly add the boxed area shown in 1d in 1a-c. We are also happy to expand the extent of 1d up-ice. We are unclear what the reviewer means by "*Help reader to orientate by defining the orientation of the grid used in the figure."*. The orientation of the 1a-d should be clear from the inset figure in 1a, but we are happy to modify the figure in response to a specific suggestion as to how to address this comment.

- Figure 2: Where is this line (2a) in Figure 1 - maybe useful to show the tie line 1 and the across line 1 (or other number) as to allow reader to orient oneself in the overview figure. An overview of the shown radar profiles would be nice (see in Figure 1).

We propose to add the location of the radar data shown in 2b-c to figure 1a as well as showing it in 2a.

- Figure 3: What is the criteria for a fold - in 3b I can't see a fold in the radargram where the third "A" from left (from Y1 towards Y2) is. Further comparing the mapped "A" with Figure 1d it seems to me that sth. is wrong. In 1d "HSR" is the second from right and not the third as shown in radargram and the spacing between the folds is different than in overview Figure 1d.

Folds were mapped where the englacial layers were folded and were not bed conformable. In the case of 3b, we may have been slightly overenthusiastic in our identification of the third A from the left, based on our mapping of the fold in parallel radar lines (i.e. we tracked it back to that location on the radargram from down-ice radargrams). We will review all our picks of englacial layer folds to ensure that only folds that we can be 100% confident represent folds are mapped. With regards to the 'HSR' we have clearly made an error in the manual labelling of figure 3b or the plotting of the fold axes in 1d. Further up-ice (i.e. where 3b is located), it does become harder to confidently identify the folds. We will address the discrepancy in any revised version of the manuscript after a comprehensive review of all the picks of the englacial folds.

- Figure 4: Numbering the radar lines would be helpful (eventhough one can deduce the numbering) especially in context with Figure 1 (if there is some numbering).

We can add numbering (perhaps every second or third across line) to figures 1 and 4 to aid with this.

- Figure 5: Distance taken from where for Figures 5b and c? Why not just show the "length" of these two radargrams (as a distance is o.k.). Why is the "signature of englacial folding" (caption line 5) not mapped/marked? What is the criteria for mapping these folds?
The distance is from the start of the radargram. This is an error and can be corrected. We also need to amend Figure 3 x-axes so that distance is reported in km rather than m. We can add annotation to the englacial folds that are apparent in figure 5. Mapping of the folds is based on obvious deviation of englacial layering from the bed profile, and identification of folding in up-ice radargrams.

- Figure 6: in (b) why not mark in the white circle the along line in blue and across in red. In (c) and (d) it's not entirely clear if the colour of the vertical line represents the colour of the crossing transect. Maybe it makes more sense to mark in (c) in red where the across transect crosses and vice versa in (d). The caption explaining this is slightly confusing. For (c) and (d) it's not clear where -5 and 5 is e.g. is (c) going downstream from -5 to 5 or upstream? Are the across profiles (d) always oriented the same way? An orientation would help.
Because of the scale of the figure we were concerned that two 10 km-long plots of the radargram locations would be lost, hence why we opted for the circle and cross to demonstrate location. We will explore options for plotting the XY positions of the radar traces over the white circles to address the reviewer's comment.
We are happy to swop the colours of the vertical lines in the radargrams. This makes sense.
The radargrams in c and d are oriented along the direction of the survey flight, so are not always oriented the same way as different parallel survey flights will have been flown in different directions. We can either: (a) orient all the radargrams in the same way; or (b) leave them as is, and simply label the orientation.

- Figures in Supplement: I'm not sure if the across profiles all (d)'s are always along the same orientation. Especially when looking at "HSR" in along tie10 it seems to me that it "jumps" from one across profile to the other. It would be nice to have the same geographical orientation for all along and across profiles at all times. As mentioned above it would be nice to know where the tie lines are.
No, the across profiles are not always in the same orientation, as they are oriented in the direction of the survey flight, and different survey flights were flown in different directions. As stated in the previous response we can either: (a) orient all the radargrams in the same way; or (b) leave them as is, and simply label the orientation. From this comment, it appears that reviewer 1 would prefer the former of these two options.
Tie and across lines will be labelled on figure 1 and 4 (see response to comment on figure 4).

Technical corrections:
- Caption 2: Line 2 - "ubiquity and widespread" is this not the same message?
We propose deleting "ubiquity"

- Caption 2: Line 2 - band of ice-layers rather than "deep-ice layers"
We are happy to make this change.

- Caption 2: Line 4 - "thin black line" of grounding line is not easy to differentiate from the "thin black lines" of the survey grid.
Agreed, we will change the colour of the grounding line.

- Caption 2: Line 5 - "bifurcation of R2 into 3-4 layers" happens also earlier around 50 km.
Agreed. We had not spotted this. We will amend the caption accordingly.

- Caption 2: Line 5 - in (c) mention tie line 9 as used in Figure 4 and supplementary figures.
Agreed. We will mention tie line 9.

- Caption 2: Line 6 - replace "show" with "shown".
Agreed. We will amend the caption accordingly

- Caption 5: Line 4 - the transects are not "perpendicular" to the ice flow rather at an angle of 45 degrees oblique to ice flow.
Agreed. We will amend the caption accordingly.