We thank the reviewers for their constructive comments on this manuscript. In the response below, we address the comments made by Referee #1 (David Sugden) and explain the changes we have made to the manuscript. Reviewer comments are in *black italics*, and our responses are in red.

RC1 (David Sugden)

This is an excellent paper adding new insights into the bed of a significant part of the West Antarctic Ice Sheet. The paper describes, analyses and interprets the bed topography extending across parts of the southern Antarctic Peninsula, the West Antarctic Rift System and the Weddell Sea Rift System. Better knowledge of the bed of Antarctica is important both for understanding current ice sheet dynamics and in elaborating geological evolution. The scientific background to this is covered effectively.

What is exciting is that the authors discover two extensive and coherent flattish surfaces that are incised by steep glacial troughs. They argue that the surfaces are relict and have been protected beneath cold-based, slow-moving ice and that glacial erosion has been selective and largely confined to the troughs. The subglacial landscape predates ice-sheet glaciation in Antarctica and is reminiscent of similar landscapes studied in the formerly glaciated areas of the Northern Hemisphere although on a grander scale. The authors put forward two interpretations. First, the surfaces represent fluvial landscape evolution following the break-up of Gondwana, similar to that for example in Southern Africa. In such a passive margin environment, the high surface is the former interior surface of Gondwana and the lower surface is related to fluvial erosion to the new lower sea level following break up. The second hypothesis for flat surface formation is through the planation of an extensive surface throughout the region by marine erosion. They favour the first explanation.

The paper brings together new results from three extensive radio-echo sounding surveys and analyses the topography along the radar lines in order to pick up sharp contrasts in slope, such as an escarpment or steep trough wall. This approach is highly effective in identifying coherent landforms. Overall, the paper relies on quantitative analysis and the hypsometry results are particularly striking, especially when applied to an isostatically rebounded topography. The figures on both methods and the results are excellent and allow a reader to follow the approach clearly and in detail.

An additional point of interest is to find that a similar pattern of two surfaces of similar elevations have been observed 400 km away across a micro plate boundary on the flanks of Institute and Möller ice streams. The similarity supports the passive margin fluvial origin for the surfaces.

We thank the reviewer for their kind and positive comments.

I wondered whether they might comment on another way of establishing fluvial activity by the recognition of river valley patterns. I was struck by the dendritic pattern at the head of Evans Glacier (Fig 2c) and the angles of the confluences that are characteristic of fluvial activity. This pattern also features on either side of surfaces 7 in Fig 5. Also suggestive is the sinuous talweg of the valley between surfaces 3 and 4, a typical fluvial signal. Perhaps the crenulated margins of surface 6 is best explained by fluvial activity? Are any of these points worth a mention?

At the beginning of Section 5.3., we have added some discussion on the likely inherited fluvial network. This introduction now reads:

"The current ice flow patterns are controlled by the inherited drainage patterns imposed by Gondwanan tectonic processes and by the original fluvial network that would have formed prior to Antarctic glaciation. We note the distinctive dendritic pattern and confluence angle of the valleys, particularly in the upper reaches of the Evans Ice Stream. The ice flow would likely have inherited the fluvial network and exploited it via selective linear erosion (Sugden, 1968, Sugden et al., 2014) leading to trough formation and flat surface preservation. Erosion of the structurally- and fluvially- controlled troughs lying between the flat surfaces is likely to have occurred after surface planation, forming conduits for the ice streams observed today. After onset of the WAIS, incision would have followed the structural weaknesses in the bedrock and pre-existing fluvial network, which both happen to be largely aligned with radial ice flow from the central WAIS (Bingham et al., 2012; Jamieson et al., 2014). Therefore, these troughs would act as conduits for ice during the initiation of Antarctic glaciation, providing topographic steering (Bingham et al., 2012; Kessler, 2008) and promoting fast, warm-based ice flow that would have strongly influenced the long-term structure and evolution of the ice sheet (Aitken et al., 2014)."

The Discussion is full and careful in considering the two hypotheses.

As a geomorphologist, I find the marine hypothesis difficult to believe. Inherited from the mid-20th Century, there has been a view that marine erosion can erode extensive surfaces. But the problem is that the erosion is attributed to wave action and that on a slope from the coast to the sea, wave action is unlikely to be able to erode a platform more than a few 100s of metres across. Here we are talking of gently sloping surfaces measuring tens by hundreds of km. Marine planation as an extensive process of erosion would have to rely on an unlikely relationship with relative sea level over millions of years. Perhaps it would be clearer to argue that the change of relative sea level may allow rivers to erode a landscape to near sea level. Indeed, following plate tectonic separation, there are new coastlines and a passive margin situation is perfect for low-relief plains to form inland of and parallel to the new coast, as in Namibia which they quote. Having said this, I am all for the authors keeping discussion of both hypotheses in the paper – if perhaps nuanced.

We have inserted and edited sentences at the end of the first paragraph in Section 5.2., which reads:

"We regard marine planation at this scale as unlikely, as there is no modern analogue for how surfaces of such breadth could be produced by wave action. For marine planation to have formed these surfaces as an extensive process of erosion, it would require an unlikely relationship with sea level over millions of years, in order for surfaces of this scale to have been formed.

Similar, but not as extensive, low-relief erosion surfaces occur in West Antarctica and New Zealand, representing prolonged intervals of erosional levelling in a stable tectonic environment (LeMasurier and Landis, 1996)."

More details.

Line 134: I do like the way you deal with the difference between Bedmachine interpolation and your radar line approach.

We thank the reviewer for their positive comment.

Line 228: Floors not flows?

Thank you for catching this; “flows” has been amended to “floors” here.

Fig 8: Really interesting to see the effect of isostatic rebound on the hypsometry.

We thank the reviewer for their positive comment.

Line 338: You seem to push the wave cut hypotheses over large areas.

Here we have rephrased the sentence at line 338, which now reads:

“Interpretations of the formational processes of these surfaces have involved passive margin evolution with a retreating escarpment dividing two surfaces following the Gondwana break-up (Paxman et al., 2018), or marine erosion or wave action into the basement in the absence of glacial ice (Wilson and Luyendyk, 2006).”

Lines 416-418: The coastal plain here today is nearer 10 km wide rather than 100km. I would be tempted to add the reference to Fitzgerald, 1992 instead of Sugden and Jamieson, since this is the main source of the figure of 4 km, and cut the last sentence. The reference is: Fitzgerald, P.G. 1992. The Transantarctic Mountains of southern Victoria Land: the application of fission track analysis to a rift shoulder uplift. Tectonics,11, 634-662.

We agree with this; the reference has been amended and the last sentence has been removed.

Line 472: After Ollier. Add Summerfield, M.A., (Ed), 2000. Geomorphology and global tectonics. Wiley, 367 pp. for a comprehensive review?

The reference suggested by the reviewer has been added here.