Reviewer #2

First, we would like to thank Prof. Stroeven for taking the time to review this paper. We appreciate his thoughtful comments, which have helped us to improve the paper. In particular, we note that he 'quite [likes] the manuscript' and he recognises that it represents the combination of 'an abundance of excellent data'. Please note that our response to the reviewer's comments are in blue italic font throughout. Where page, line and figure numbers are preceded by 'ESurfD' they relate to the original submitted manuscript, otherwise they refer to the newly revised manuscript.

Thank you for this opportunity to review the manuscript by K.C. Rose and co-authors entitled "Ancient pre-glacial erosion surfaces preserved beneath the West Antarctic Ice Sheet" for consideration in Earth Surface Dynamics. I quite like the manuscript, how it sets out to document an enigmatic upland underneath the West Antarctic ice sheet in the Institute and Möller Ice Streams (IMIS) drainage area. However, whereas I concur entirely with the title that what they document is a "pre-glacial erosion surface", the data of the authors cannot with certainty attribute the erosion surface to a particular erosion regime (fluvial, glacial, marine) or for that matter to a particular time. The authors do, however, both, and in that respect I think they do their data injustice. The paper should not be published in its present form, but could be published almost in its present form if it were more balanced in its discussion and conclusion.

In this paper, we did not intend to argue that we know the formation mechanism and timing of origin for the erosion surface with absolute certainty. It was our aim to present a range of possible processes capable of forming such a surface and to convey which one we believed was the most likely mechanism of formation based on the evidence available. Similarly, our intention was only to hypothesise a likely time in the past when conditions would be suitable for such processes to operate.

It is evident, however, that these intentions were not conveyed satisfactorily, so that the reviewer had concerns regarding the extent to which we interpret the data. We have, therefore, decided to significantly restructure the paper in order to provide a more balanced discussion and conclusion to the data set. These changes address the primary concerns of both Prof. Stroeven and Reviewer 1 (see point 3 of our response).

Point 1: *We have amended manuscript to limit the scope of our interpretation to only characterise the bedrock surfaces as erosion surfaces (**Section 5.1**). We then present a short* *discussion of a range of different mechanisms that would be capable of forming an erosion surface (**Section 6**). We thereby establish this paper as a foundation from which further investigations may be carried out and data collected in order to determine the true mode of formation of this surface. We also more clearly acknowledge that the mode of formation of such surfaces is controversial, complex and largely unknown (**Section 6**). We have removed the section hypothesising the potential timing of formation of the surfaces.*

The authors are to be congratulated in bringing together such an abundance of excellent data from one of the more hostile environments on Earth. The paper is preceded by half a dozen articles or so, in which some of the data has been described and analysed (such as bed roughness): however, none have attempted to deduce the long-term history of the investigated bedrock surfaces beneath the investigated WAIS sector in the Weddell Sea.

The inference of marine erosion surfaces, or strandflats, is not a new one to Antarctica, and indeed they also occur along many of the glaciated and formerly glaciated margins. However, there has been little progress in understanding the formation of such exceedingly even bedrock surfaces close to contemporary sea level. Fredin et al. (2013), which the authors refer to, credits their formation in Norway to "A wide range of [possible] processes", including marine abrasion (wave action), erosion by sea ice and frost weathering, and glacial erosion. Hence, even if we knew that the identified surfaces are strandflats, we still don't know how they form in detail. However, the landforms described could perhaps also be remnants of fluvial planation surfaces (of which there are also a host of varieties described in Scandinavia; see papers by Lidmar-Bregström on the subject) or remnants of true peneplains. In my mind, the surface characteristics described and the imagery provided (1B, 3A, 4A, 5A) could also be consistent with a surface of areal scouring, or, as the authors themselves acknowledge "In North America, for example, glacial erosion was responsible for the removal of sedimentary deposits and the development of a "knock and lochan" style landscape, but the underlying Canadian Shield geology was the dominant control on the gently undulating bedrock surface retained (Embleton and King, 1975)."

The reviewer highlights that there are a range of possible surfaces with different origins that have characteristics that are comparable with the surfaces we describe. The reviewer demonstrates that we have included reference to several different possibilities in our original submission (ESurfD, Section 6). We have, however, now amended the manuscript to present *a more even discussion on the possible origin and mechanism of formation for the surface (see point 1 above, **Section 6**).*

What I would advise the authors to do, is to set-up a set of multiple hypotheses for the formation of the relict bedrock surfaces and discuss the merits of marine abrasion, fluvial planation, and areal scouring as the three dominant explanatory lines, and, perhaps less confidently, conclude that marine abrasion as their preference, but that it is hard to discount entirely the merits of the other two. This would suit the title a lot better as well!

*We thank the reviewer for this useful suggestion. We have restructured the manuscript in accordance with point 1 above (see **Sections 5 & 6**).*

The marine abrasion surface explanation hinges heavily on the inferred rebound bringing the depressed surface to sea level. This appears to be a most generalized model which may or may not be realistic. It would have been good if the authors had indicated how much uncertainty they expect their reconstruction was prone to. If one attempted to do a better GIA reconstruction, it would include (i) the effects of ice sheet change since the LGM and (ii) the effect of WAIS and EAIS remnants on a situation where the study area became ice free. Is it necessary to remove all ice to get a good solution, or is it perhaps better with some remnant ice? How long does it take to come to a good solution?

*Our intention, in regard to the inferred rebound, was only to present a generalised model or estimate of pre-glacial bedrock elevations across the IMIS survey area, to see if they were anywhere in the region of sea level (ESurfD, p.686, L.17-23). The suggestions made by the reviewer are very sensible but they are currently beyond the scope of this paper. Here, we wish to present the data collected, as we believe it is important that these data should be available to the community, and we wish to present some interpretation of what these data represent. However, a full modelling account on all possibilities in not feasible. Therefore, we wish to use this paper as a platform to encourage future work, such as a full GIA reconstruction, as suggested by the reviewer above. This aim should be now reflected more satisfactorily as a result of the amendments made to the paper in accordance with point 1 above (see **Sections 5 and 6**). These changes also ensure that the estimated rebound elevations are not used to favour any one particular mode of surface formation over another.*

If the surface were formed by the ice sheet plucking the regolith of a weathering surface (scouring), but not by much more in those regions where U-shaped valleys are absent, then the underlying bedrock structure dominates the roughness signal. Where subsequent streamlining occurs through glacial erosion, this then changes the roughness values.

Below, I indicate some comments to the ms by references to page numbers and line numbers:

Page 864, line 7: Change “LeBrocq” to Le Brocq.

Changed

Page 864, line 9: Remove “, TCD”.

Reference now updated following final publication in The Cryosphere in 2014.

Page 686, lines 20-23: “We appreciate that this does not take into account the full complexity associated with glacio-tectonic interactions. However, it does provide an indication of pre-glacial elevations across the region, offering insight into the landscape setting prior to glaciation.”

As I indicated above, this is insufficiently-well constrained if this is used to dismiss other explanation models.

C*hanges to the manuscript (see point 1) ensure that the estimated rebound elevations are not used to favour any one particular mode of surface formation over another.*

Page 688, lines 20-22: “The long-profile radar echogram shows that there is a pronounced break in slope and change in elevation along this profile, approximately 80 km inland from the edge of the Robin Subglacial Basin (Fig. 2c).”

It is also here insufficiently clear why the break in slope is at c 140 km whereas a more natural break in slope appears to occur 25 km further upstream at around 115 km.

We note that the reviewer has identified an area at ~115 km (ESurfD Fig. 2c) where a change in topography may occur. We would argue, however, that the feature we identified at ~140 km represents a significant break in slope across this region as it corresponds with changes in the ice sheet surface (ESurfD Fig. 3e & f), a significant change in slope (ESurfD Fig. 3c) and it approximates sea level using the generalised isostatic rebound model (ESurfD Fig. 3b).

Page 689, lines 8-12: “Steep slopes (> 7°) are associated with […]. The block itself has generally low slope gradients (< 4°), reflecting….”

The figure is rather useless in showing these differences due to the sliding scale. Also, if one enlarges the figure digitally to see anything, it appears as if the scale is inverted with white colors as most steep (16°) and black as flat (0°), quite contrary to what the map shows. Perhaps the solution to showing regional differences is to use classes of slopes rather than a sliding scale.

We have found that it has been particularly difficult to display changes in slope in a more satisfactory manner. The figure presented represents what we found to be the best of many different iterations, using a variety of different colour schemes and scale types. We believe, however, that the black colour scheme does help to show those regions where the slopes are most steep. We agree, however, that the colour scale legend appears to have been inverted – this has now been changed.

Page 689, line 14: add “s” to divide.

Changed

Page 690, lines 27-28: I have a hard time seeing it differently than that “58% of the landscape area lies within 100 m of glacial-isostatic sea level (between 100 and −100 m)”.

The elevation range is 200 m, but we appreciate the reviewer’s point that either side of sea level (0 m) the land is within 100 m – now changed.

Page 691: Roughness: there are probably some other, older, studies of subglacial roughness and what they imply than Rippin et al. (2014) and it would be prudent to dig some out. One study that comes to mind is that by J.-O. Näslund from Dronning Maud Land (1997?).

Further references on subglacial roughness have now been incorporated (Section 3.3).

Page 695, lines 14-18: “Fluvial erosion processes erode towards a base level, typically at sea level. However, given the broad extent of the surfaces and their setting in a marine embayment, we consider destructional marine terrace formation (Burbank and Anderson, 2012) to be the dominant erosion process in this case.”

This is hardly self-evident for the reader. Why would fluvial-erosional surfaces (peneplains?) be more restricted than marine terraces?? Some of the largest “flat” surfaces on earth are considered peneplains. How much does the current "setting in a marine embayment" of Miocene or even Eocene erosional bedrock landforms inform us about their fluvial or marine (or even glacial) history? This can never be a sweeping statement (as it is here) but needs some logical back-up. Because the coupling between landscape size and process regime remains unclear, the following sentence, therefore, makes no sense at all: "This is particularly applicable given that the erosion surfaces may have been much more extensive…"

*We have reconsidered our arguments here in light of the amended structure of the paper (see point 1). Greater consideration is now given to the role of fluvial processes as a possible mechanism for the formation of the surfaces (**Section 6**).*

Page 695, lines 20-23: the importance of glacial erosion is also done with through some head-on dubious statements which need backing-up with data:
"We consider that a glacial origin for the erosion surfaces is also unlikely. Under present-day ice sheet conditions, the comparatively thin ice and low ice flow velocities (< 50 ma−1), coupled with low roughness values for this region (Figs. 3d and 5), are consistent with low rates of erosion."

In any other context than Antarctica, perhaps, 1500 m of ice is not "comparatively thin", and certainly can do a lot of harm: hence, also this needs some qualification. It remains also unclear why "low roughness values" are consistent with low rates of erosion. How much is high rates of erosion, and what are typical roughness values for these landscapes? At large, the landscape has slope values between 0° and 16°, according to the scale bar in Figure 3c…

*In the study region, climatic conditions mean that 1500 m of ice is insufficient to cause pressure melting at the ice-bed interface. Similarly, ice flow velocities are insufficient to cause frictional melting. Therefore, cold-based conditions may be sustained, under which subglacial erosion rates are typically low (Benn and Evans, 2003). The text has now been clarified to reflect these ideas (**Section 5** - preservation).*

The reviewer makes a good point in terms of the 'low roughness values'. This sentence has now been amended as it should reflect that the thin (i.e. cold-based) ice and low ice flow velocities (i.e. no frictional melting) are consistent with low erosion rates. The low roughness values determined for this region are unusual and relate to the preservation of the low-relief block rather than the ice dynamical regime.

High slope values (>7°) are associated with the slopes of the Ellsworth Trough and the Pirrit and Martin-Nash Hills and particularly values >10° are spatially localised. The majority *of the region is dominated by low slope values of <4°. This is outlined in ESurfD section 4.1.2. The slope colour bar legend has now been edited.*

Page 696, lines 3-6: “Therefore, whilst processes of areal scour may have modified the landscape on the micro-scale, at a macro-scale the dominant mechanism generating the gently-sloping surfaces identified is likely to be marine-erosional processes.”

I agree that large flattish surfaces don’t probably come about by glacial erosion, and that their effect is more “cosmetic”, even though I wouldn’t call it “micro-scale” myself. If it created barren bedrock surfaces out of what may have been a regolith covered bedrock surface, then the influence is rather substantial.

*We were referring to the scale of features that develop through erosion, for example striations (micro-scale features) versus glacial troughs (macro-scale features). However, we appreciate the reviewer’s point that the term ‘micro-scale’ may be confusing to the reader in this context and would agree that stripping regolith from bedrock of a large area can result in a macro-scale signal of glacial erosion. The text has now been edited to qualify these statements and in response to point 1 (see **Section 6**).*

Page 696, lines 24-27: “Marine erosion processes are concentrated at the interface between land and sea through the constant action of waves impacting a shoreline, often during a period of tectonic quiescence (Burbank and Anderson, 2012).”

It seems to me that what was required for the formation of such extensive marine erosional landforms is not only tectonic quiescence, but also sea level quiescence (over hundreds of thousands of years). Is this reasonable? I would like to see more thinking around this topic than what has been presented in the paper.

We would agree that sea level quiescence would also be required and I think that this is reasonable and has occurred in the past. This is mentioned in the original manuscript under the section on the timing of formation (ESurfD p.700, L.9-13). However, with the amendments made to the text with regard to point 1, this section has now been removed and we have not, therefore, expanded on these ideas here. This would, however, be a suitable area for future study.

Page 697, lines 12-13: “In order to form these surfaces, the coastal region must have been relatively free of sea ice, to allow wave erosion to occur at wave base over a period of several 100 kyr."; and page 700, lines 15-16: "In order to form, ice free conditions must prevail at the coast during periods of ice retreat to allow wave action to occur."

Another set of sweeping statements which I believe merits some discussion (especially since it inflicts far-reaching conclusions about the age of glaciation) – certainly given the hypothesized range of formational processes lifted by Fredin et al. (2013), including erosion by sea ice?

These statements are factual in that sustained erosion at wave base and therefore ice free conditions are required for formation of a wave-cut platform. However, we no longer infer the formation, or timing of formation, of the erosion surfaces based on these mechanisms, in accordance with point 1 (ESurfD section 6.3 is removed, see new ***Section 6****).*

Page 698, lines 5-13: "Despite the present-day setting, it is evident that the surfaces have been subject to some degree of glacial erosion following formation. A few larger (often U-shaped) valleys are visible in cross-profile A, particularly in proximity to the Ellsworth Trough (Fig. 2a); whilst further inland cross-profile B has been more significantly dissected by broader U-shaped valleys (Fig. 2b). These intermittent, U-shaped valleys are suggestive of selective linear erosion by small- to regional-scale, warm-based, ice masses (Sugden and John, 1976; Hirano and Aniya, 1988). The location of the valleys may reflect pre-existing fluvial networks that have been exploited (e.g. Baroni et al., 2005; Rose et al., 2013; Ross et al., 2014)."

Here is some of the reasoning I don't understand. If there are precursor fluvial valleys – would this not be a good argument for the consideration of these surfaces as fluvial in nature? After the growth of ice sheets across these features, they would then be preferentially deepened. If there were fluvial valleys (above sea level) and the landscape was subsequently levelled-off by marine erosion to a lower level, why would there then be fluvial precursors? This reeks to me as special pleading (which the authors don't even bother to do). In any case, I invite the authors to be more generous with their ideas on the complexity of landscape development.

We acknowledge that the reviewer makes some logical comments here. We have now amended our arguments regarding fluvial erosion processes (see point 1) and do not dismiss any potential mechanisms of landscape development in relation to the formation of the surfaces (see ***Section 6****).*

Page 698, lines 13-17: "The scale and style of this glacial erosional overprinting is characteristic of warm-based, outlet glaciers, prior to the onset of extensive West Antarctic glaciation. These ice masses would be subject to topographic steering and could therefore flow around the Pirrit and Martin–Nash Hills, enabling glacial incision of the erosion surfaces to occur."

Whereas I take no argument against the inference for warm-based ice, I see no reason to assume that these were "outlet glaciers" prior to the onset of extensive glaciation. If extensive glaciation, at least initially, was warm-based over the entire region (plucking whatever loose material there was on the "relict" surfaces as well as deepening the valleys), glacial erosion over the fluvial precursor valleys would quickly increase local relief, thus more and more inhibiting the conditions for erosion over the intervening plateau surfaces.

We infer the presence of outlet glaciers due to the presence of the troughs which typically result from selective linear erosion by outlet glaciers. When ice was of sufficient volume to encompass more of the Pirrit and Martin-Nash Hills and extend in to the region behind these hills, the outlet glaciers would become incorporated into the ice sheet and the flow regime of the ice mass would change allowing for preservation of the surface in the lee of these hills. In this location, even if the rest of the ice sheet was warm-based, the block is protected as ice velocities and therefore erosion rates are reduced in the lee of the hills.

Page 698, line 28: "…thin, cold based ice." See my previous comment about "thin" ice.

See response above.

Page 699, line 16: "with" should probably be "within". However, even so, this sentence should be rewritten for clarity.

Section 6.3 has been removed.

Page 700, lines 18-19: "This makes it unlikely that a major ice mass was established in the region at that time". Specify "major ice mass" and "region".

Sweeping statement, without precision.

Section 6.3 has been removed.

References are complete. I would, however, invite some additional references to underpin requested clarifications.

Additional references have been added where requested – see responses above.

Figure 1: panel (a), left-hand-side 70°S should be 60°S. panel (b), upper-left-hand 84°S should be 80°S. Perhaps one could add “S” (=Skytrain Ice Rise) in the far upper-left corner of panel b?

Changed as requested.

Figure 3: panel (c), scale is inverted. panel (d), perhaps the subglacial topography can be depicted in gray-scale? It is somewhat confusing to find “red” hues in the area of low velocities. The color-scale does not refer to this!

Changes made to the colour-scales in panels c and d.

Figure 4: panel (a), legend for “Upper Surface” and Main IMIS Survey Grid” are too similar!

Legends changed to differentiate between the two items.