

Dear Frédéric Herman,

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 The paper should not be published in its present form, but could be published almost in its present form if it were more balanced in its discussion and conclusion.

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).*”

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!

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?

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.

**Page 864**, line 9. Remove “, TCD”.

**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.

**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.

**Page 689**, lines 8-12: “*Steep slopes ( $> 7^\circ$ ) are associated with [...]. The block itself has generally low slope gradients ( $< 4^\circ$ ), 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 ( $16^\circ$ ) and black as flat ( $0^\circ$ ), 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.

**Page 689**, line 14: add “s” to divide.

**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)*”.

**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?).

**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...*”

**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 ( $< 50 \text{ m a}^{-1}$ ) 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  $0^\circ$  and  $16^\circ$ , according to the scale bar in Figure 3c...

**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.

**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.

**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?

**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.

**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.

**Page 698**, line 28: “...**thin**, cold based ice.” See my previous comment about “thin” ice.

**Page 698**, line 16: “with” should probably be “within”. However, even so, this sentence should be rewritten for clarity.

**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.

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

**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?

**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!

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