Anonymous Referee #1

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Review: Ancient pre-glacial erosion surfaces preserved beneath the West Antarctic Ice Sheet

Rose et al.

We thank this anonymous reviewer for their helpful comments regarding this paper and we note that they find the paper 'interesting', 'important and exciting'. They also comment that it provides images, as well as descriptions and analyses, which are 'as good as possible with the currently available 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 giving me this interesting paper to review. The identification and the production of digital terrain models of land surfaces beneath the West Antarctic Ice Sheet are important and exciting, as they provide us with the possibility to investigate long-term landscape evolution and ice sheet erosional impact in non-accessible areas, beneath an active ice sheet. This paper provides images that are as good as possible with the currently available data for this sub-ice topography, as well as descriptions and GIS analyses of this terrain. Significantly, the paper identifies erosion surfaces that are part of this terrain and discusses the age and possible processes involved in the formation of these ancient surfaces. Despite the interesting setting of this paper, a major revision with re-submission is recommended, due to a lack of references, data and convincing arguments in the authors' discussion of these formation processes and their timing.

In summary, my suggestions for improving this paper are:

- Clearly separate the identification and description of the sub-ice land surface from the much more difficult discussion of formation mechanisms and timing
- Provide a map of known geology and structure for the study area to exploit possible influences of geological structure on surface morphology, e.g. to exclude exhumation of the surface.
- To a high degree sharpen the discussion about formation mechanisms of the erosion surfaces. Include the relation of geology to surface topography. Provide more convincing evidence for the shore platform theory and consider alternative formation possibilities for broad low relief surfaces with appropriate references.

We have taken the three points stated by the reviewer above (hereafter, 1, 2 and 3, respectively) as guidelines by which we improve and restructure the manuscript. We have addressed each of these points as follows:

1. *We have separated any descriptive elements of the landscape (see* ***Section 5****) from the discussion on formation mechanisms (****Section 6****). We have also have removed any discussion on timing of formation.*

2. *We have included a map on the geology and tectonic structure of the study area (****Figure 2****) and discuss the regional geological setting in more detail (see* ***Section 2****).*

3. *Although our paper does not state conclusively that our favoured explanation for the likely formation of the surfaces was the only one or a definitive solution, we have now limited the scope of our discussion by removing our arguments suggesting the most likely mechanism of formation for the erosion surfaces, in order to address the reviewer's concerns. The discussion has been restructured to encompass more fully 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 erosion surfaces in general is controversial, complex and largely unknown.*

The identification, description and visualization of the sub-ice topography. From my view this is good work and no major revisions are needed, even though there are numerous papers dealing with the identification of erosion surfaces in digital terrain models, using the hypsometry and other criteria, including stepped erosion surfaces, which the authors should be aware of and consider including in their paper. Erosion surfaces have been identified and analyzed in DEMs amongst others in Zimbabwe (Römer), southern Norway (Etzelmüller), Wales (Rowberry), Sweden (Lidmar-Bergström), and northern Sweden (Ebert). In these papers, often the timing and possible formation processes are discussed. Below I give further names of authors dealing with erosion surfaces.

We thank the reviewer for their comment that the identification, description and visualization of the sub-ice topography is 'good work and no major revisions are needed'. We are aware that there is a great deal of literature regarding the identification and analysis of erosion surfaces and we appreciate the reviewer bringing to our attention further examples. However, this paper is not meant to represent a literature review and so we do not attempt to cite all of the many relevant papers in this field of study.

The second aspect concerns the authors' conclusions about the formation mechanisms and age of the erosion surfaces. The conclusion of the authors that these surfaces are shore platforms (is the term wave-cut platforms still used? The term shore platform includes all marine processes involved in platform formation, not exclusively wave action) are insufficiently substantiated. In addition, within this discussion, other possibilities like long-term differential weathering and erosion to base level are not mentioned at all, despite a considerable number of papers in the literature that deal with erosion surfaces in all kinds of settings, more or less comparable to the authors study area, within and outside Antarctica. The authors basically give glacial erosion and wave erosion as the sole alternatives, with a weak consideration of fluvial processes, and no mention of weathering processes. Also, the relation of landforms to geology and structure, which are at least roughly known, is not discussed. The processes involved in the formation of wide erosion surfaces cut in basement rocks are subject to long standing debates and uncertainty. The formation of huge shore platforms is an exciting possibility that currently lacks support. However the authors come to a rather quick conclusion based on reasons that do not convince me.

In our original discussion, we provide several different options for the possible formation of the erosion surface. In this way, we do not completely dismiss any other mode of formation. Our intention was simply to give more space to the mechanism which we think is most likely to have formed the extensive erosion surface.

In specific response to the reviewer's comments we note that:

- *We have now changed 'wave-cut platform' to 'shore platform' where appropriate.*
- *Contrary to the reviewer's assertions, we did mention the mechanism of erosion to base level (ESurfD p.695, L.14-15). However, greater consideration is now given to the role of fluvial erosion as a possible mechanism for the formation of the surfaces (**Section 6**).*
- *We have amended the text (in accordance with point 3) to give greater consideration to a range of different processes as possible mechanisms for the formation of the surfaces (**Section 6**). However, it is not the aim of this paper to discuss every single potential mechanism in detail. Therefore, we have focused the paper on processes that would be most likely to result in the formation of the erosion surfaces described.*
- *Contrary to the reviewer's assertions, we did mention weathering in relation to shore platform formation (ESurfD p. 692, L.15-17). However, this concept is now given greater consideration in the text (**Section 6**).*
- *Contrary to the reviewer's assertions, we did discuss the relation of landforms to geology and structure. We discussed the presence of the tectonically controlled Marginal and Transitional*

*Basins in relation to the erosion surfaces (ESurfD p.685, L.1-11) and the long-term tectonic setting of the region (ESurfD p.699, L.6-15). Further information regarding the tectonic setting of this region is now given in **Section 2** and **Figure 2**.*

- *We agree that there is debate around the formation of these surfaces and the manuscript did not intend to present a definitive conclusion on the origin of the surfaces. Our intention was only to present the mechanism that we thought most likely, from amongst a number of possible processes. Whilst we do not agree that our arguments are unconvincing, we acknowledge that more consideration to the wide range of possible processes may be required. We have now addressed these concerns by restructuring the paper according to point 3 above (**Section 6**). In doing so, we aim to demonstrate that there is a lively debate around the formation of such surfaces.*

The author's arguments for interpreting the erosion surface as a shore platform are dispersed in the paper. In general, the authors argue that

- morphological characteristics like the fact that the surface is 150 km wide, comparatively flat, has slope values of 4-7 degrees,
- the surface is situated in a marine embayment,
- the surface is at sea level after calculation of isostatic rebound,
- that conditions at 15-17 Ma were ice-free,
- comparison to the Norwegian strandflat,
- other authors (Wilson and Luyendyk) who came to the same conclusion, proves that this surface is a Miocene shore platform.

*We have now restructured the paper so that the arguments for interpreting the formation of the surface are no longer dispersed within the paper (see **Section 5** and above responses).*

To the reviewer's knowledge, no shore platform on Earth cut in basement is known to be 150 km wide – most Late Pleistocene shore platforms are a few hundred metres wide at most. Examples for extremely wide shore platforms on other locations on Earth would support the authors' argument for a marine origin.

We acknowledge the reviewer's point that the width of the erosion surface is substantial. However, we find that Antarctic geomorphology is often exceptional. The size of glacial landforms is often considerably larger in scale than comparable features elsewhere in the world. For example, the cirques and features of alpine glaciation discussed by Haynes (1995); the glacial troughs of East Antarctica mapped by Young et al. (2011); and the meltwater flood networks found in the Dry Valleys *(e.g. Marchant et al., 2011). Therefore, although these features may be exceptional in scale, this is no reason to dismiss the ideas presented.*

Shore platforms in continental settings are generally near-horizontal, with inclinations of < 2 deg. Can the comparatively steep angles of 4-7 degrees of the erosion surface in the authors' study are be accounted for by tilting, faulting or warping after formation?

We suggest that subsequent regional tilting may be the cause of this (ESurfD p.697, L.17-21).

The fact that the erosion surface is comparatively flat is difficult to assess – shore platforms typically have a relative relief of a couple of meters; the relief of the identified surfaces seems higher.

We would argue that the relief shown in the radar echograms presented, in particular as highlighted in the regions with the horizontal red lines in ESurfD Figure 2a and b, demonstrate remarkably well the summit elevation consistency and low relief of the surfaces.

Did the calculation of the isostatic rebound take into consideration Miocene Sea level elevations? An interesting point would be a calculation of how long sea level must be slowly rising to produce a 150 km shore platform; given the step in the authors' erosion surface, two periods of sea level rise are needed. What timeframes are needed and is this feasible? Are there other landforms witnessing about this event? Can we exclude with all certainty that the step is a geological feature, a tectonic line or a change in bedrock type?

We agree that the reviewer makes some good suggestions here for investigating the long-term isostatic history of the region. However, such work is beyond the scope of this paper. The suggestions made would be sufficient for a new paper in itself. Therefore, in light of the changes made to the paper with regards to point 3 above, we are happy that our work could and should lead others to continue the investigation in the way described.

The Norwegian strandflat, given as an analogy by the authors, is found along the whole coast of western Norway, with an abrupt break in slope from the mountain chain, and is generally much narrower than the author's erosion surface, with a maximum width of 60 km.

We refer the reviewer to earlier comments regarding the scale of landscape assemblages in Antarctica. In addition, the scale and duration of ice sheet cover and marine exposure in our study region may differ from that in Norway. Furthermore, the break in slope between the surfaces may *represent a different stage of formation for each surface. In which case, the lower surface is 80 km wide, which is in the same order of magnitude as the Norwegian strandflats.*

The authors should read and take into consideration erosion surface literature, which exists for all kinds of settings. Some (few) examples: Antarctica (Sugden, Isbell (the Kukri surface)); Australia (Twidale); Greenland (Bonow, Japsen); Norway (Fjellanger, Etzelmüller, Bonow); NE Brazil (Peulvast); NE Scotland (Hall); Sweden (Lidmar-Bergström, Ebert). The literature check should be done with particular attention to the shaping of South Hemisphere continental margins in the Miocene.

Glacial erosion is selective (Sugden, Kleman, Hall, Staiger) and low-relief surfaces and areas on low elevations can be preserved under cold-based ice as e.g. has been the case on the northern Scandinavian shield (Kleman and Hättestrand, Fabel, Hättestrand, Ebert, Hall).

We thank the reviewer for the example papers that they suggest. We are aware that there is a great deal of literature regarding the identification and analysis of erosion surfaces. This paper is not meant to represent a literature review and it would be impossible to cite all of the many relevant papers in this field of study. In light of the new structure of the manuscript (see point 3 above), we have used relevant suggested references where required (see ***Section 6****).*

<u>Detailed comments about the text:</u>

Abstract:

- Here you use the term planation surface, otherwise erosion surface. Be consequent in your terminology and give a definition of the term you chose to use.

Changed 'planation' to 'erosion'

- The genetic term wave-cut platform is not in use any more, use shore platform instead.

Changed 'shore platform' instead of 'platform'

- Sharpen the arguments or reconsider the formation process and timing of the erosion Surface

*See point 3 above (****Section 6****)*

Introduction:

- You state that you address "this issue". Which issue – the glacial history of the region? That is not really the main issue you address in the paper.

Yes, we are referring to the glacial history of the region (ESurfD p.683, L.24-25). We present the motivation for the airborne geophysical survey across this region in the first place. Once we have introduced this idea we go on to introduce the resultant survey data and show that the macro-scale geomorphology of this region had not yet been assessed (ESurfD p.684, L.9-10). Here, we assess that macro-scale geomorphology and in doing so reveal the presence of the erosion surfaces, which are the main issue addressed in the paper. This is a logical chain of thought for the reader that is appropriate in the introduction section of the paper.

- There are more papers you can refer to that state that topography exerts a strong control on ice dynamics, e.g. Sugden's paper about selective glacial erosion

We agree that there are many papers available that attest to this, including Sugden et al. (2005). We chose a recent paper derived from work in Antarctica that also shows topographic control on ice dynamics. We have now prefaced this reference with 'e.g.' to demonstrate to the reader that this reference is an example and exhaustive.

- Last sentence: You discuss the likely evolution of the landscape in relation to longterm GEOMORPHOLOGICAL history (not solely glacial history)

Now included the term 'geomorphological'

Chapter 3.2

- In your calculation for isostatic rebound, do you take into account sea level changes?

No, but we acknowledged that this was a simplified approach (ESurfD p.686, L.20-23). With regards to Miocene sea level rise, this may have been up to a maximum of 25 m at intermittent times during the Miocene, which would be within the error margins of the isostatic rebound correction. Given the restructure of the paper (point 3), less emphasis is now placed on the calculation of isostatic rebound so that elevations are not used to favour any one particular mode of surface formation over another.

Chapter 3.3

- There are many relevant papers to refer to when it comes to DEM-analyses of erosion surfaces, see above.

We agree and we have chosen to reference papers that are relevant and relate to the techniques we have used. The text does not indicate that the references used are exhaustive or exclusive, neither is this meant to be a review paper.

- The roughness characteristics are interesting but it is not entirely clear what you use it for. Please clarify.

Roughness is used as a means to characterise the texture of relief of the erosion surfaces so that they can be distinguished (if appropriate) from the surrounding topography as a unique land type. It is also possible to make inferences about ice dynamical regimes based on such information (e.g. Bingham et al., 2007; Bingham and Siegert, 2007, 2009; Hubbard et al., 2000; Rippin et al., 2014; Siegert et al., 2005). We use this information as a supplementary piece of evidence to help distinguish the erosion surfaces from the surrounding topography (ESurfD p.687 L.26 to p.688, L.1; p.688, L.5-7). Further information has been added to Section 3.3.

Chapter 4.1.2

- You describe a huge block with rather steep slopes and considerable relief. Even if there is a summit elevation constancy, and even when you ignore the glacially deepened landscape elements – does this block have the characteristics of a typical shore platform?

As described in the text, the block has a gently sloping surface, it has a very low surface roughness, in ice free conditions it rebounds to elevations around sea level and it is a marine embayment setting. It, therefore, has characteristics that are comparable with a shore platform. The 'considerable relief' only relates to the steep slopes at the margins of the block, which are the result of overdeepened glacial troughs formed along ice stream tributaries.

- Given the uncertainties in your rebound calculation, you give rather detailed numbers for mean elevations of the elevated block. Is there no possible error scale in the calculation?

We do not put undue emphasis on the mean rebound elevations, in fact we try to highlight that this is a general estimate and acknowledge the limitations to the calculation (ESurfD p.686, L.20-23). Given the restructure of the paper (point 3), less emphasis is now placed on the calculation of isostatic rebound so that elevations are not used to favour any one particular mode of surface formation over another. This opens up the possibility for others to perform a fully modelled GIA investigation of the region. We did state RMS error values for ice thicknesses in the paper (ESurfD p.686, L.8).

Chapter 4.1.3

- To get the hypsometric curve, would it be possible to subtract the clearly glacially eroded areas and give a curve for the preserved areas only?

The red and blue elements of the bar graph show the preserved components of the shore platform. We believe that these stand out well against the grey background, which represents the overall hypsometric signal for the region (a signal of glacial erosion).

Chapter 4.1.4

- Not entirely clear what you use the roughness index for, and the different way of calculating roughness as presented in figure 5.

Roughness is used to characterise the erosion surfaces (see earlier comment). A description of the different roughness indices was given at ESurfD p.697, L.26 to p.688 L.7. We specifically direct the reader to the paper Rippin et al. (2014) because the methods are relatively involved (ESurfD p.691, L.2-3) and we believe that in this paper the reader benefits from the concise description rather than having to read through a more detailed (and therefore lengthy) outline of the methods involved (see Section 3.3). If they wish to explore the method in more detail, they have been given the appropriate reference to use.

Chapter 5

- This chapter should be part of the discussion.

*The paper has now been restructured according to point 3 (**Section 6**).*

Chapter 6.1

- "Fluvial erosion processes erode towards a base level, typically sea level. However, given the broad extent of the surfaces and their setting in a marine embayment, we consider destructional marine terrace formation () to be the dominant erosion process".

I do not see the logic in this sentence – consider all erosion surfaces with broad extents in non-marine environments.

*It is logical, given the setting of the surface in a marine embayment environment to consider that marine processes are likely to have been the dominant process influencing the formation of the surface. However, the discussion has now been edited to give greater consideration to the role of a variety of processes as possible mechanisms for the formation of the surfaces (**Section 6**).*

- Glacial erosion is more commonly associated with widening and deepening existing valley features – yes, see references on selective glacial erosion above

- For the “knock and lochan” style landscape, and the persistence of the macro-scale landforms on glaciated shields, there are newer studies than Embleton and King 1975, especially for Scotland and the northern Fennoscandian shield, see above.

We have now included additional references with regards to this topic (see Section 6).

- “: : :.and the fact that they average the same elevation over large distances is highly indicative of shore platforms” – and of many other, much larger erosion surfaces.

*We do not dispute this, however, we have edited the discussion in accordance with point 3 to give greater consideration to the role of a variety of processes as possible mechanisms for the formation of the surfaces (**Section 6**).*

- Here you discuss the necessary gradual rise in sea level, and the possible tilt of the block, as parts of landscape development. These are crucial points that should get a lot more attention.

*We no longer argue for a likely mechanism of formation for the erosion surfaces and therefore the discussion on landscape development in minimised. Instead, we suggest the variety of possible mechanisms for the formation of the surfaces (**Section 6**).*

Chapters 6.2 and 6.3, and chapter 7

- See comments above about alternative formation processes

See responses above, particularly point 3.

- An important mention here that “it is unfeasible that all of this erosion occurred via marine processes” but is part of a complex landscape evolution history. Fluvial erosion yes, what about deep weathering?

*We agree that deep weathering during the long-term evolution of the surface could occur (**Section 6**).*

Figures:

The figures are generally good.

Figure 2: Please indicate in the figure or in the figure text what the dashed lines in B-B’ mean – it took me a while to find and understand that information in the text.

*We have now amended the figure caption (see **Figure 3**) so that the meaning of line B-B’ is more evident to the reader.*

Figure 3: A very nice figure, showing the topography, the results of the isostatic rebound calculation, ice flow velocity in one figure to clarify connections or differences. However, please provide a larger version of this figures, the text in the panel legends is barely readable.

*We have now amended the figure so that the panel legends are now more clearly readable (see **Figure 4**).*

Figure 5: Please clarify the use of different ways to calculate the roughness index and what exactly the values in the legends mean. Explain in chapter 4.1.4 how the index was calculated and what the values mean.

We have not detailed the method for the reasons outlined above, but further information on roughness has been added to Section 3.3.

Please add a figure showing the geology of the area.

*We have now added **new Figure 2** which shows the regional geological and tectonic setting.*