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Comment

Interactive comment on “Ancient pre-glacial erosion surfaces preserved beneath the West Antarctic Ice Sheet” by K. C. Rose et al.

Anonymous Referee #1

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Review: Ancient pre-glacial erosion surfaces preserved beneath the West Antarctic Ice Sheet Rose et al.

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

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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.

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.

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 in-

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cludes 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.

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.

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

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wide at most. Examples for extremely wide shore platforms on other locations on Earth would support the authors' argument for a marine origin. 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 be accounted for by tilting, faulting or warping after formation? 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. 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? 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.

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).

Detailed comments about the text:

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.
- The genetic term wave-cut platform is not in use any more, use shore platform instead.
- Sharpen the arguments or reconsider the formation process and timing of the erosion Surface

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

Chapter 3.2

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

Chapter 3.3

- There are many relevant papers to refer to when it comes to DEM-analyses of erosion surfaces, see above.
- The roughness characteristics are interesting but it is not entirely clear what you use it for. Please clarify.

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 deep-

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ened landscape elements – does this block have the characteristics of a typical shore platform?

- 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?

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? 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.

Chapter 5

- This chapter should be part of the discussion.

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.

- 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.

- “. . .and the fact that they average the same elevation over large distances is highly

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indicative of shore platforms” – and of many other, much larger erosion surfaces.

- 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.

Chapters 6.2 and 6.3, and chapter 7

- See comments above about alternative formation processes

- 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?

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.

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.

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.

Please add a figure showing the geology of the area.

Interactive comment on Earth Surf. Dynam. Discuss., 2, 681, 2014.

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