tektonika

Review Report

Bonow et al. The High Plains of Southern Norway: Result of Late Mesozoic – Cenozoic Episodic Tectonics, **TEKTONIKA, 2026.**

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1st Round of Revisions

Decision Letter

Thankyou for your patience whilst we worked to find reviewers for your submission to τεκτονικα, "The Elevated Passive Continental Margin of Southern Norway - Result of Late Mesozoic – Cenozoic Episodic Development ". We have now received two reviews, which are included below.

You will see that both reviewers have similar admiration for your submission, but also some mutually complementary suggestions for what it might still need. In particular, we note that there is agreement on the need for revisions to

(i) structure the submission more appropriately for the goals of a journal covering tectonics and structural geology, and

(ii) reduce the volume of the manuscript so that its central messages are delivered more concisely.

Reviewer B has a number of more targeted suggestions for revisions. Reviewer F has returned an annotated version of your manuscript, which is attached.

The associate editor and I concur that the comments and suggestions, taken as a whole, should prompt a set of revisions that are large enough for a second round of review to be necessary. We look forward to receiving your revised manuscript in due course.

Kind regards,

Graeme Eagles

Comments by Reviewer 1

Reviewer B.

I appreciated the manuscript entitled "The Elevated Passive Continental Margin of Southern Norway - Result of Late Mesozoic–Cenozoic Episodic Development", Ref. Review TEKTONIKA-2025. The topic is of great interest to the geomorphological community, especially the topographic dynamics of elevated post-rift landscapes. The paper presents important observations derived from topographic metrics, fieldwork, and occurrences of low-relief surfaces, with a robust review that documents the formation of paleosurfaces with distinct formative chronologies, supported by detailed mapping and integration of AFT data. However, I recommend incorporating quantitative topographic data (e.g., ksn, local relief, χ–elevation) combined with lithology and structural domains, as well as channel profiles with knickpoints and lithologic crossings to support the morphostratigraphic inferences. Tectonic movements should be more explicitly described (kinematics, reactivation phases, and regional implications), and AFTA data should be presented explicitly (maps/tables with locations) to allow a clear assessment of spatial distribution and correlation with the LPS/MPS/UPS. I also note the excessive length of the manuscript (main text + numerous figures/supplements), which hampers a thorough review; I suggest reduction and synthesis (merging figures and including an event chart integrating tectonics–burial/exhumation–formation/uplift of the surfaces). I hope these suggestions help to improve the manuscript.

Specific comments

1. Introduction

The Introduction is well written and situates EPCMs effectively, but the objective becomes somewhat diffuse at the end. I suggest closing the section with a clear, testable objective.

2. Study Area (includes 2.1–2.5)

The geological/geomorphological description is comprehensive but lengthy. Please condense and move details to the Supplement.

Figure 3: The figure should show structural data: traces and kinematics of major faults (incl. the Møre–Trøndelag Fault Complex, MTFC), regional foliations, and, if available, evidence of recent faults. Include slip-sense arrows.

Add a map showing the distribution of AFTA data to convey spatial coverage and uncertainties.

2.3/2.4 Jurassic surface and regional development. Excellent discussion, but reinforce it with swath profiles showing mean slope/dip of hillslopes and the onshore–offshore continuity of the Jurassic surface.

2.5 MTFC: Make the kinematic history explicit and discuss expected impacts of footwall uplift vs. elastic flexure on the dome width.

3. Principles of Stratigraphic Landscape Analysis (SLA)

SLA applications often rely too heavily on visual interpretation without sufficient quantification of rock strength and basin dynamics. In some cases, SLA may underestimate erosion rates, conflate lithologic controls with tectonic signals, and overlook basin dynamics over time. Please highlight how these limitations were addressed by the authors. Also, move this subsection to Methods; it is methodological and should be transferred to Section 5.

4. Methods

State explicitly that "the topography within 5 km of line" in Fig. 4 corresponds to a swath profile; show the swath window on the map figure and adopt consistent symbols/colors.

Discuss the sensitivity of interpretations to the slope threshold choice (6.5° vs. 12°) and justify it with a robustness test.

I strongly suggest integrating χ–elevation, ksn, local relief, and knickpoints to corroborate the elevation levels of the surfaces and their transitions (basin-by-basin profiles, altitude histograms by lithology/structural unit).

5. Relief (Results)

The definition of paleosurfaces appears in Materials and Methods; it should be presented exclusively here (Results), leaving in Methods only the criteria.

The qualitative comparison between surface locations and lithologies is a very good approach, but it needs quantitative data: for example, mean/median elevations of the LPS/MPS/UPS by geologic unit to demonstrate whether there are (or are not) systematic elevation differences among lithologies. If this is not feasible, please show more profiles intersecting lithologic boundaries.

6. Evidence for when/how the surfaces formed

What field evidence supports the pre-glacial stages? Uplifted strath terraces, incised paleochannels? If the inference is based solely on slope changes, please make this clear (Fig. 16).

7. Discussion

I suggest an event chart integrating tectonic episodes, burial/exhumation phases (AFTA), sedimentary evidence, and the formation/uplift of the surfaces.

Include a balanced discussion with counterpoints for mechanisms involving isostasy + erosion, dynamic topography linked to the Iceland Plume, and tectonic reactivation.

Figure 17. Very useful, but the relationship with the geological substrate (dips/attitudes) is not clear. I suggest adding dip data and complementary onshore panels showing major structures in the conceptual profiles.

Figures (additional notes)

Fig. 3 should include faults, foliations, and kinematics (with standard symbols).

Fig. 4. Indicate on the map the swath width (±5 km) and add dip measurements on the onshore segments of the profiles.

Fig. 6. Spell out all abbreviations in the figure caption (even if explained elsewhere).

Figs. 9 and 11. Consider merging or rationalizing—there are many figures; combining 9 with 11 may help.

Supplementary materials

S1 (.ai): convert to layered vector PDF.

S2–S3: include a complete legend (colors/lines) and metadata (projection, vertical exaggeration, DEM resolution).

S5: consider moving photos/3D views to the Supplement and reducing in the main text.

Recommendation: Resubmit for Review

Comments by Reviewer 2

Reviewer F (Tony Doré):

This paper is an excellent piece of work - partly review, partly new with special attention to the landforms in southern Norway. It is probably the best explanation of the southern Norwegian mountains - what form they take and why they might have evolved - that I have seen to date. It also contains what I think are the most comprehensive descriptions yet presented of the planation surfaces of the area, complete with detailed maps. The relationship between the various "palaeic" surfaces and the offshore stratigraphic succession is covered clearly and is credible. The paper features beautiful illustrations and is very well-written. I have made some minor English corrections in the attached annotated copy, and have also added some comments for your consideration.

The issue here is that the paper is largely geomorphological. To fit in a structurally-orientated journal like Tektonika, the tectonic side of things needs to be given more substance. This can be done, with a little bit of effort. As it stands, the section on mechanisms is a bit simplistic and under-referenced. But at least one of the co-authors (Japsen) has gone into more detail on potential mechanisms. This reasoning would stand repetition here, particularly given the need to beef up the structural side of things.

Some examples of where better reasoning could be applied:

Lines 808-810: You say the Jurassic peneplain is due to uplift of the "North Sea Dome" - so what is the relationship between the two features? Are they the same dome, joined up, or separate? Is the magnitude of uplift comparable? What is the relationship between the Jurassic peneplain and the offshore "Base Cretaceous Unconformity", itself a composite feature? Finally, are there potential alternative explanations for the uplift? Add references.

Lines 811-816: I can see that explanations for the Mid-Cretaceous UPS are giving you a bit of a problem. The footwall uplift idea, which you had earlier cast doubt upon, doesn't really seem to work. I think you could make more of the relationship to inversion, which must in turn have been related to continental scale kinematics. The inversion along the Tornquist Line was probably related to Tethyan closure tectonics. The Austrian phase of the Alpine orogeny was mid-Cretaceous in age - etc. Add references.

Lines 817-818: A one-liner, apparently attributing the MPS to the "emplacement" of the Iceland Plume, is not enough. Some advocates of the Iceland Plume think it was around long before this time. Check Torsvik’s papers - he, at least, starts the plume at about this time - and quote them. Furthermore, if you like the plume idea, then it is still there today. So you are really talking about what some workers think was the initial burst of activity on the plume. Personally, I don't like the idea of the southern Norwegian uplift being somehow linked to a plume that was supposedly centred 1000 km away to the east. But I don't have to like it, so long as you make cogent arguments. What is undoubtedly true is that your MPS is roughly coincident with the start of the North Atlantic Igneous Province of Scotland, the Faroes, the Greenland margin and the Mid-Norwegian margin, i.e. with a period of potentially intense thermal and (possibly) dynamic uplift. Again, quote references. Also, can you rule out Alpine-Pyrenean tectonics, i.e. intraplate stress, as invoked for the 23 Ma activity, for the 60 Ma event?

Lines 824-828: Finally, outward-flowing asthenosphere from the Iceland plume may or may not have caused uplift of the continental margins, as Japsen suggested earlier. But southern Norway is hardly the continental margin, is it? Can we see a better discussion of the causes of the late event please, even if you have to repeat some of the arguments from earlier papers?

To make it clear, I'm recommending minor revision for this paper, which as a stand-alone is a great piece of work. That minor revision, in my opinion, should consist of better arguments on tectonic mechanisms, with more reference to previous work. Given the outstanding level of scholarship you have shown in discussing previous work on landform evolution, I think this should not be too difficult. The abstract should also make reference to potential tectonic mechanisms.

Recommendation: Revisions Required

Authors' Reply to Reviewer 1

I recommend incorporating quantitative topographic data (e.g., ksn, local relief, χ–elevation) combined with lithology and structural domains, as well as channel profiles with knickpoints and lithologic crossings to support the morphostratigraphic inferences.

As we have written, the interpretation of the surfaces is based on "morphostratigraphic data"; abrupt changes in gradient. We have examined and shown an example that the surfaces are not dependent on geology. Channel profiles with knickpoints are not really relevant to our interpretation.
We used "local relief" in our interpretation but do not think that ksn and χ–elevation would add anything to our interpretation. We have compared our interpretation with a quantitative interpretation (Etzelmüller et al., 2007) and shown that it misses a lot of detail that we have found.

Tectonic movements should be more explicitly described (kinematics, reactivation phases, and regional implications).

We have written a more comprehensive discussion of the possible causes of the various uplift events (Section 9)

AFTA data should be presented explicitly (maps/tables with locations) to allow a clear assessment of spatial distribution and correlation with the LPS/MPS/UPS.

The AFTA data is described comprehensively in Green et al. (2022). To repeat it here would add enormously to the size of the paper.

I also note the excessive length of the manuscript (main text + numerous figures/supplements), which hampers a thorough review; I suggest reduction and synthesis.

- *3 figures removed from the main text (old figs 5, 12 and 15 placed in the Supplement).*
- *Table 2 added in response to the request for a chart with an overview of the tectonic history (comment G5 of Reviewer B).*
- *Expanded discussion of mechanisms in Section 9 (requested by Tony Doré). Expanded from 750 to 1500 words. We note that Reviewer B suggested several expansions of the manuscript, that we have declined.*
- *Discussion of alternative ways of mapping moved to Supplement S7.*
- *Length of main text (incl. Table 1): 9600 words, was 8800. New Table 2: 500 words*
- *We find that the current version of the paper is concise, given the complexity of the subject.*

This reviewer also wishes us to add large amounts of extra data and figures.

I suggest including an event chart integrating tectonics–burial/exhumation–formation/uplift of the surfaces.

We agree with the reviewer that such a summary would improve the readability of the paper and have thus added a new Table 2

The Introduction is well written and situates EPCMs effectively, but the objective becomes somewhat diffuse at the end. I suggest closing the section with a clear, testable objective.

OK – we have added:

'The Norwegian margin is thus in many aspects a typical EPCM, and the debate about the origin of the Norwegian mountains is also typical of the world-wide discussion about the origin of EPCMs.. . . . . The Norwegian mountains is an obvious test site for this debate.'

The geological/geomorphological description is comprehensive but lengthy. Please condense and move details to the Supplement.

We find that all aspects covered in Section 2 (Study area) are fundamental for the arguments presented in this paper.

Add a map showing the distribution of AFTA data to convey spatial coverage and uncertainties.

This is available in Green et al (2022).

Jurassic surface and regional development. Excellent discussion but reinforce it with swath profiles showing mean slope/dip of hillslopes and the onshore–offshore continuity of the Jurassic surface.

Thank you. The suggested relations are illustrated in Figs 4 and 5.

Make the kinematic history explicit and discuss expected impacts of footwall uplift vs. elastic flexure on the dome width.

We do this in our revised discussion on causes of uplift (Section 9), however, we also aim at keeping the length of the paper at a minimum.

SLA applications often rely too heavily on visual interpretation without sufficient quantification of rock strength and basin dynamics. In some cases, SLA may underestimate erosion rates, conflate lithologic controls with tectonic signals, and overlook basin dynamics over time.

If we need to discuss all this, it would be the subject of a later paper. We discuss the relationship between the surfaces and lithology in Section 5.5.

Please highlight how these limitations were addressed by the authors. Also, move this subsection to Methods; it is methodological and should be transferred to Section 5.

OK. The discussion of SLA is now part of the Methods section.

Discuss the sensitivity of interpretations to the slope threshold choice (6.5° vs. 12°) and justify it with a robustness test.

These criteria were originally developed for Bonow et al. (2003). As we write, they were reviewed for this paper and we see no reason to alter them and they were used as the criteria for differentiating between surfaces, slopes and escarpments.

The definition of paleosurfaces appears in Materials and Methods; it should be presented exclusively here (Results), leaving in Methods only the criteria.

We do not understand this comment. The surfaces are exclusively discussed in Section 5 (Relief).

The qualitative comparison between surface locations and lithologies is a very good approach, but it needs quantitative data: for example, mean/median elevations of the LPS/MPS/UPS by geologic unit to demonstrate whether there are (or are not) systematic elevation differences among lithologies. If this is not feasible, please show more profiles intersecting lithologic boundaries.

This addition would make an already long paper very long. We compare in the text what the reviewer calls our "qualitative" method with the "quantitative" method used by Etzelmüller et al. (2007) and show that this approach missed a lot of the details that our method discovered.

What field evidence supports the pre-glacial stages? Uplifted strath terraces, incised paleochannels? If the inference is based solely on slope changes, please make this clear (Fig. 16).

In this paper we show one example of fluvial incision of the LPS (Fig. 14). Lidmar-Bergström et al. (2000) and Bonow et al (2003) showed several more and also referred to papers dating back to 1919 who argued for the presence of pre-glacial, fluvial valleys in the Norwegian landscape.

I suggest an event chart integrating tectonic episodes, burial/exhumation phases (AFTA), sedimentary evidence, and the formation/uplift of the surfaces.

We have added a new Table 2.

Include a balanced discussion with counterpoints for mechanisms involving isostasy + erosion, dynamic topography linked to the Iceland Plume, and tectonic reactivation.

This would be a large topic and would be suitable for a new paper. To do so, we need to add an interpretation of the Jurassic surface and will do that in a later paper and then integrate all the surfaces with AFTA in a third paper. However, the discussion of Mechanisms is expanded

The figure should show structural data: traces and kinematics of major faults (incl. the Møre–Trøndelag Fault Complex, MTFC), regional foliations, and, if available, evidence of recent faults. Include slip-sense arrows.

We have no reason to think that this would add anything to our analysis.

The figure should include faults, foliations, and kinematics (with standard symbols).

We have added the Lærdal-Gjende Fault System to Fig 3 as it is relevant to a discussion of Cretaceous footwall uplift and the formation of the UPS.

State explicitly that “the topography within 5 km of line” in Fig. 4 corresponds to a swath profile; show the swath window on the map figure and adopt consistent symbols/colors.

Indicate on the map the swath width (±5 km) and add dip measurements on the onshore segments of the profiles.

OK - Figure and caption revised accordingly

Spell out all abbreviations in the figure caption (even if explained elsewhere).

OK – we have done so in all captions

Consider merging or rationalizing—there are many figures; combining 9 with 11 may help.

OK - We have removed three figures from the main text (old Figs 5, 12 and 15). Combining Figs 9 and 11 will result in poor graphics.

Very useful, but the relationship with the geological substrate (dips/attitudes) is not clear.

Thank you. We have moved old Fig. 17 to Section 2 to become new Fig. 4.

I suggest adding dip data and complementary onshore panels showing major structures in the conceptual profiles.

We want to keep the drawing simple to maintain it as a useful illustration. No changes.

Convert to layered vector PDF.

We now include a vector pdf as Supplement 1 and a kmz file as supplement 4

Include a complete legend (colors/lines) and metadata (projection, vertical exaggeration, DEM resolution).

OK – We have done so. A mistake!

Consider moving photos/3D views to the Supplement and reducing in the main text.

OK - Old Fig. 5 moved to Supplement 5. Old Fig 12(a) moved to Fig 14. Old File 12(b) moved to File 14. Old file 15 moved to Supplement 7.

Authors' Reply to Reviewer 2

To fit in a structurally-orientated journal like Tektonika, the tectonic side of things needs to be given more substance. This can be done, with a little bit of effort. As it stands, the section on mechanisms is a bit simplistic and under-referenced. This reasoning would stand repetition here, particularly given the need to beef up the structural side of things.

We have thoroughly revised and extended our discussion of the causes of uplift that led to the formation of the surfaces (Section 9).

You say the Jurassic peneplain is due to uplift of the "North Sea Dome" - so what is the relationship between the two features? Are they the same dome, joined up, or separate? Is the magnitude of uplift comparable? What is the relationship between the Jurassic peneplain and the offshore "Base Cretaceous Unconformity", itself a composite feature? Finally, are there potential alternative explanations for the uplift? Add references.

Sentence deleted and substituted with new text (Section 9.1):

"We suggest that the uplift and erosion that caused formation of the Jurassic surface on the slopes of the Southern Scandes were related to the same mechanism that gave rise to the mid-Jurassic "mid-Cimmerian Unconformity (MCU) in the North Sea. "

. . . .   "However, the doming in the North Sea was part of a much larger area affected by mid-Jurassic exhumation, including Scandinavia and East Greenland (see figure 4E of Japsen et al., 2024), representing the final phase of the break-up of Pangaea (Veevers, 2013; Japsen et al., 2016)."

I can see that explanations for the Mid-Cretaceous UPS are giving you a bit of a problem. The footwall uplift idea, which you had earlier cast doubt upon, doesn't really seem to work. I think you could make more of the relationship to inversion, which must in turn have been related to continental scale kinematics. The inversion along the Tornquist Line was probably related to Tethyan closure tectonics. The Austrian phase of the Alpine orogeny was mid-Cretaceous in age - etc. Add references.

We have rewritten this comprehensively ending with this summary (Section 9.2):

"We therefore suggest that the UPS was formed after crustal-scale folding, that involved bending of the mid-Jurassic peneplain in Norway (Japsen et al., 2024). The driving force may be the same as that which caused movement of the Adria plate northwards relative to the European plate, initiating subduction of the intervening oceanic crust, and causing basin inversion and thrusting in central Europe (Kley and Voigt, 2008; Pfiffner, 2014)."

A one-liner, apparently attributing the MPS to the "emplacement" of the Iceland Plume, is not enough. Some advocates of the Iceland Plume think it was around long before this time. Check Torsvik's papers - he, at least, starts the plume at about this time - and quote them.

Furthermore, if you like the plume idea, then it is still there today. So you are really talking about what some workers think was the initial burst of activity on the plume. Personally, I don't like the idea of the southern Norwegian uplift being somehow linked to a plume that was supposedly centred 1000 km away to the east. But I don't have to like it, so long as you make cogent arguments. What is undoubtedly true is that your MPS is roughly coincident with the start of the North Atlantic Igneous Province of Scotland, the Faroes, the Greenland margin and the Mid-Norwegian margin, i.e. with a period of potentially intense thermal and (possibly) dynamic uplift. Again, quote references. Also, can you rule out Alpine-Pyrenean tectonics, i.e. intraplate stress, as invoked for the 23 Ma activity, for the 60 Ma event?

We have rewritten this comprehensively (Section 9.3).

"The primary, direct evidence for Paleocene uplift is the fan sedimentation west of Norway, which was derived from the Norwegian hinterland, reaching the fans in rivers flowing west (Sømme et al, 2019). Sømme et al. (2019) showed that fan sedimentation started at 68 Ma, coincident with the onset of doming above the Iceland plume rising below central Greenland (Japsen et al., 2023). The dome was ca. 2000 km in diameter; a size consistent with the calculations of Campbell (2007) and with the area affected by the Maastrichtian rise of the Iceland plume (Japsen et al., 2023). Southern Norway was thus within the eastern side of this dome."

Finally, outward-flowing asthenosphere from the Iceland plume may or may not have caused uplift of the continental margins, as Japsen suggested earlier. But southern Norway is hardly the continental margin, is it? Can we see a better discussion of the causes of the late event please, even if you have to repeat some of the arguments from earlier papers?

We have rewritten this comprehensively (Section 9.5).

"Early Pliocene uplift raised the conjugate margins of the NE Atlantic with maximum contribution where Greenland is closest to Iceland (black numbers in figure 4F of Japsen et al., 2024). This indicates that dynamic support from the Iceland plume contributed to the early Pliocene uplift due to outward-flowing asthenosphere extending beneath the margins, as indicating by tomographic studies (Rickers et al., 2013; Schooneman et al., 2017). The onset of this episode happened at a time of peak activity of the Iceland plume coincident with a reduction in the spreading rate in the NE Atlantic, linked to the dynamics of the Pacific plate (Poore et al., 2009; Iaffaldano and DeMets, 2016). We note, however, that there was also renewed compressive stress from the Alps at this time as the continental crust of the Mont Blanc massif was emplaced (Egli & Mancktelow, 2013). "

The abstract should also make reference to potential tectonic mechanisms.

Mention of a discussion of mechanisms added.

Text: "Why are there mountains in Norway? This provocative and seemingly naïve question remains unanswered by this study". Comment: You have done yourself a disservice here. I think you have gone a long way towards providing an explanation, or a series of them. I suggest "This provocative and seemingly naïve question is still not fully answered by this study, However, as a first step…"

Text changed accordingly

Text: "Whereas the ultimate causes driving these vertical movements remain elusive (and their definition outside the scope of this paper), our study demonstrates . . ." Comment: Cut this, and beef up the mechanisms section somewhat. You don't have to do new work, just explain the mechanisms better (I have made some suggestions) and add more references. It's a structural journal...

We have done so.

2nd Round of Revisions

Decision Letter

We have reached a decision regarding your submission to τεκτονικα, "The Elevated Passive Continental Margin of Southern Norway - Result of Late Mesozoic – Cenozoic Episodic Development ".

Our decision is: Minor Revisions Required

Comments by Reviewer 1

As per my emails to the editor, I can confirm my initial impression - i.e. that the authors have done a thoroughly professional job in responding to the referees, and in documenting their response. Their tabulated replies are probably the clearest reviewer response I have seen to date. Very impressive! Furthermore, the authors have taken great pains to accommodate the reviewers' points and to entertain different opinions and alternative hypotheses. Their original text has been modified considerably. The new text reads well, and is also less tentative, more confident. The English and the quaity of the illustrations remains good.

I still don't agree with the authors on everything (the idea of a wisp of the Iceland Plume making its way from the centre of the Atlantic, across the Faroe-Shetland Basin and northern North Sea, to cause uplift in southern Norway sounds far-fetched to me) but as I said in the last review, I don't have to agree. The main point is that the arguments are better itemized now, and where alternatives exist, they are mentioned.

The authors have also responded reasonably well to the request from both reviewers to include more discussion of tectonics, given the nature of this journal.

So in summary, I believe Tektonika can now accept this very good manuscript with confidence.

Tony Doré, London, February 2026.

Comments by Reviewer 2

Thank you for the opportunity to re-evaluate the revised manuscript entitled “The High Plains of Southern Norway: Result of Late Mesozoic–Cenozoic Episodic Tectonics.”

The revised manuscript shows substantial improvements. The most notable are:
(i) the rewriting and expansion of the uplift mechanisms section, now structured into five well-referenced subsections linking each peneplain to specific tectonic mechanisms;
(ii) the inclusion of the new Table 2, which integrates tectonic episodes, sedimentary evidence, AFTA constraints, and the formation/uplift of the surfaces, significantly improving synthesis;
(iii) structural reorganization, with the principles of SLA moved to the Methods section and a strengthened Discussion;
(iv) rationalization of the figures, with three moved to the Supplementary Material and partial compensation through a new 3D panel in Fig. 14 and the addition of Table 2;
(v) incorporation of the Lærdal-Gjende Fault System (LGFS) in Fig. 3 and in the discussion of Cretaceous extension; and
(vi) a revised abstract and conclusions with explicit reference to tectonic mechanisms and a clearer position regarding the footwall uplift and Caledonian remnant hypotheses.

These changes address important concerns raised during the first review stage, and the manuscript is considerably stronger now. However, **several** suggestions from my initial review were not incorporated, in several cases with the justification that the manuscript is already long. Below, I assess those that I believe still warrant consideration.

(1) Justification for the slope thresholds.
This issue remains a concern. The entire surface mapping depends on these thresholds, and the authors’ response was: “These criteria were originally developed for Bonow et al. (2003). As we write, they were reviewed for this paper and we see no reason to alter them and they were used as the criteria for differentiating between surfaces, slopes and escarpments.” Bonow et al. (2003) attribute these values to a “best-fit model developed by Sulebak (Lidmar-Bergström et al., 2000),” but I could not identify the calibration procedure for these thresholds in the cited reference. I do not question that the thresholds produce geomorphologically meaningful results. Rather, I note that the values may be related to practical constraints associated with contour spacing at the 1:500,000 scale, which would be a perfectly valid justification. However, this rationale is not explicitly stated in the manuscript. I request that the authors add a brief sentence (in Section 3.4) explaining how the 6.5°, 12°, and >12° thresholds were determined and whether the mapped surfaces are sensitive to small variations in these values. Even a qualitative discussion would substantially improve methodological transparency.

(2) Quantitative support for the lithological independence of the surfaces.

The claim that the surfaces are independent of geology is central to their interpretation as peneplains. In Section 4.5, the authors present a consistent geometric argument, based on cross-sections (Fig. 13c, d), demonstrating that the LPS cannot systematically correspond to the Base Cambrian Unconformity (BCU). However, the synthesis of this reasoning (Fig. 13e) is presented as a conceptual sketch without precise geographic location, measured contacts, or vertical scale, which limits its demonstrative strength. Moreover, the authors state that the coincidence between the LPS and the BCU in parts of Hardangervidda is "only a coincidence." This interpretation is relevant, but it remains essentially qualitative and, in some passages, does not explicitly rule out the possibility of local lithological control. Given that lithological independence is a central component of the thesis, this point could be strengthened with minimal quantitative support.
I understand the concern regarding manuscript length as well as the comparison made with the statistical method of Etzelmüller et al. (2007), whose approach allegedly "missed many details." However, simply stating that the alternative approach did not capture sufficient detail does not replace a positive demonstration of the robustness of the present interpretation. I therefore suggest including a brief comparison reporting the mean elevation of the LPS and the mean elevation of the lithological units in the same areas. These values can be directly extracted from the DEM already used and from the mapped surfaces. Such an addition would require only a few lines but would convert a currently qualitative argument into a quantitatively testable one, more convincingly reinforcing the conclusion that the altimetric distribution of the surfaces is not controlled by the underlying lithology.

Recommendation
The revised manuscript has improved substantially, particularly in its treatment of tectonic mechanisms, which was the primary concern. The mapping and interpretation of the peneplains represent an important contribution to the understanding of the Scandinavian EPCM. The two remaining issues listed above are specific and limited in scope, and I believe they can be resolved without major restructuring. Therefore, I recommend minor revision, conditional upon the authors addressing the concerns raised above.

Sincerely,

References cited in this review

Bonow, J.M., Lidmar-Bergström, K. and Näslund, J.O. (2003). Palaeo-surfaces and major valleys in the area of the Kjølen Mountains, southern Norway – consequences of uplift and climatic change. Norwegian Journal of Geography, 57, 83–101,

Etzelmüller, B., Romstad, B. and Fjellanger, J. (2007). Automatic regional classification of topography in Norway. Norwegian Journal of Geology, 87, 167–180.

Lidmar-Bergström, K., Ollier, C.D. and Sulebak, J.R. (2000). Landforms and uplift history of southern Norway. Global and Planetary Change, 24, 211–231.

Authors' Reply to Reviewer 1

- Accept recommendation: no reply required -

Authors' Reply to Reviewer 2

I request that the authors add a brief sentence (in Section 3.4) explaining how the 6.5°, 12°, and >12° thresholds were determined and whether the mapped surfaces are sensitive to small variations in these values. Even a qualitative discussion would substantially improve methodological transparency.

We have followed the reviewer's suggestion and expanded the explanation for the thresholds for slopes. We substituted this text

'We identified a low-relief surface where 100-m contours are spaced more than 1 km apart, corresponding to slopes less than 6.5° (Bonow et al., 2003). We identified the edge of a low-relief surface as locations where widely-spaced contours become more closely spaced, corresponding to slopes of more than 6.5° (Fig. 6a). Changes of this type happen at the edge of escarpments (where 100-m contours are less than 500 m apart, corresponding to slopes greater than 12° and less commonly where the transition between two surfaces is a slope between 6.5° and 12° (Fig. 6b, blue arrow). Changes in slope angle between 6.5° and 12° also occur on the sides of valleys incised into the surfaces.'

with these words

'We identified a low-relief surface where 100-m contours are spaced more than 1 km apart, corresponding to slopes less than 6.5°, the same dip that Bonow et al.(2003) used in their analysis of the Kjølen area. Their analysis used an objective slope map (0° to 90°) that showed large areas with low inclination (<6.5°, interpreted as surfaces) and steeper slopes bounding these areas. The slopes showed a distinct stepped pattern; the highest surface was bounded by a distinct slope, followed by another surface at lower elevation, followed by yet another distinct slope. The delineation of the surfaces in the Kjølen area in this study is highly similar to that published by Bonow et al. (2003) despite differences in the map scale and we adopted the same contour interval (100 m) and dip angles to define surfaces, slopes and escarpments.

We identified the edge of a low-relief surface as locations where widely-spaced contours become more closely spaced, corresponding to slopes of more than 6.5° (Fig. 6a). Changes of this type happen at the edge of escarpments (where 100-m contours are less than 500 m apart, corresponding to slopes greater than 12° and less commonly where the transition between two surfaces is a slope between 6.5° and 12° (Fig. 6b, blue arrow). Changes in slope angle between 6.5° and 12° also occur on the sides of valleys incised into the surfaces.'

The synthesis of this reasoning (Fig. 13e) is presented as a conceptual sketch without precise geographic location, measured contacts, or vertical scale, which limits its demonstrative strength.

We thought that such a conceptual sketch would be useful to explain the principles that the Base Cambrian Unconformity is below Caledonian rocks in some areas, at the present Precambrian basement surface in others and eroded away in the remainder. However, it has obviously caused confusion, so we have removed it from Fig 13.

Moreover, the authors state that the coincidence between the LPS and the BCU in parts of Hardangervidda is "only a coincidence." This interpretation is relevant, but it remains essentially qualitative and, in some passages, does not explicitly rule out the possibility of local lithological control. Given that lithological independence is a central component of the thesis, this point could be strengthened with minimal quantitative support.

We have redrawn Fig. 13:

- *added a new profile 13c, that illustrates a region where the three palaeic surfaces have formed on the same lithology, Precambrian basement,*
- *dropped the sketch in (old) Fig. 13e,*
- *dropped the Mandal-Ustaoset Fault which we did not discuss in the previous version*

We hope that these revisions now make the observations clearer that the BCU is below Caledonian rocks in some areas, at the present Precambrian basement surface in others and eroded away in the remainder. There must, therefore be at least one place where the BCU coincides with at least one of the palaeic surfaces; Hardangervidda (Fig 13d). The evidence that the BCU is missing to the south of Hardangervidda is the presence of Precambrian sediments near the summit of Gausta mountain, more than 600 m above the Lower Palaeic Surface on Hardangervidda, a short distance to the north of Gausta (Fig. 13e).

I understand the concern regarding manuscript length as well as the comparison made with the statistical method of Etzelmüller et al. (2007), whose approach allegedly "missed many details." However, simply stating that the alternative approach did not capture sufficient detail does not replace a positive demonstration of the robustness of the present interpretation.

We have nowhere stated that Etzelmüller's approach "missed many details" nor that it 'did not capture sufficient detail'. To avoid any future misunderstandings regarding our conclusion, we have reformulated the last part of that paragraph:

'A comparison of our interpretation with theirs shows a number of major differences. Our approach has distinguished many planation surface remnants, especially of the MPS and UPS that are smaller than the 7-km window used by Etzelmüller et al. (2007). The 7-km window has clumped our MPS, UPS surfaces together with the higher alpine topography into their 'Higher mountain plateaux (Table lands, "vidde")' (see Supplement S7 for details).'

I therefore suggest including a brief comparison reporting the mean elevation of the LPS and the mean elevation of the lithological units in the same areas. These values can be directly extracted from the DEM already used and from the mapped surfaces. Such an addition would require only a few lines but would convert a currently qualitative argument into a quantitatively testable one, more convincingly reinforcing the conclusion that the altimetric distribution of the surfaces is not controlled by the underlying lithology.

We have considered the reviewer's suggestion that we should provide quantitative support for our conclusion that the surfaces are not controlled by geology.

We have no idea how this could be done. The surfaces we have identified are present in only restricted altitudes above sea level, whereas geological units can be present over large altitude ranges; for example, Precambrian basement is present from below sea level to over 2000 m a.s.l.

We think that our technique of identifying surfaces where dips are less than 6.5^{o} separated by slopes and escarpments at higher angles must be a quantitative approach that is sufficiently reproducible by other researchers.

However, we have attempted to show the relationship between the palaeic surfaces and geology by modifying Fig 13. Fig 13c now shows that that all three palaeic surfaces are present on the same geological unit (Precambrian basement) in the same area. Fig. 13d now shows that one of our surfaces is continuous from a geological unit that is not easily eroded (Precambrian basement) to one that is much more easily eroded (phyllites). Fig 13e shows the presence of the MPS at a higher level on relatively easily eroded geological unit (phyllites) whereas the nearby LPS is present at a lower level on a much less easily eroded rock (Precambrian basement). Fig 13e and associated text also shows that the LPS on the Precambrian basement at Hardangervidda coincides with the Base Cambrian Unconformity in some parts of Hardangervidda but cannot be the BCU everywhere (see discussion in 2b above).

The section in Escarpments (unchanged) is now Section 5.5. This is because we demonstrated in that section that the phyllites are more easily erodable that other geological units in the southern Scandes. This observation is important in the following section 5.6 about the relation between the palaeic surfaces and geology. The latter section has been rewritten and Fig 13 redrawn to clarify our interpretation.

Acceptance Letter

Peter Japsen, Johan M. Bonow, James A. Chalmers:

We have reached a decision regarding your submission to τεκτonικa, "The Elevated Passive Continental Margin of Southern Norway - Result of Late Mesozoic – Cenozoic Episodic Development ".

Our decision is to: Accept Submission

Graeme Eagles – Journal Editor

Claudio Salazar-Mora – Associate Editor

03/27/2026