Answers to the reviewers

Reviewer 1 (Anne Duperret)

The aim of this paper is to explore if slow uplifts in stable continental regions, evidenced by long-term erosion, is driven by simple isostatic adjustments. Surprisingly, slow coastal uplifts have been attributed to global mantle dynamics, plate tectonics, regional lower crustal flow triggered by glaciations cycles, local fault reactivation or local volcanism, but never to simple isostatic adjustments. The question is thus original and of first interest.

The study is then dedicated to the Britain part of the Armorican Massif, using three main models. Maximum uplift rates due to denudation appears to be localised in central Brittany lowlands where denudation rates are the lowest. This is a surprising result by comparison with the other denudation rates calculations and regional modelling. In the discussion, the authors try to resolve this conundrum by comparisons with some others results of the literature, but never discuss the initial assumption. This is the main weakness of the paper.

Nevertheless, denudation rates models established in the Britain part of the Armorican Massif is an original and good piece of work, useful for the community. It needs thus to be published. Models of vertical deformation due to denudation show globally equivalent rates. The variations observed between highlands and lowlands could thus be explained by additional local processes, that needs to be discussed with various processes (local tectonics? climatic variations? crustal and lithospheric variations? isostatic model adaptation?)

We agree that the model and the physical formalism that we choose (thin elastic plate) can only bring first-order long-wavelength information, and that our study should not be used directly for small-scale interpretations or predictions, as for instance related to fault activity. This point is discussed in Section 6 in relation to the QNEF fault and its proposed activity in previous studies. We rephrased this section to better show this limit.

Uplift rates, regional sea-level and geodynamics: This paragraph is based on comparisons with uplift rates deduced from work on marine terraces located in Cotentin peninsula and extended all over the western Europe coastline (Pedoja et al, 2018). An attempt of Pedoja's data re-interpretation is realised using various potential altitude of past-sea-levels, but the exercise evidence a lot of uncertainties. This is amplify by the fact that north Brittany armorican marine terraces are located in the central lowland Brittany coast and they are not precisely dated. (see specific comments). Marine terraces of north Brittany needs first to be studied in details to suggest some new uplift rates based on the suggested methodology.

We agree that terrace data are associated with significant uncertainties, especially on their ages, and should therefore be studied in more details. However, such a detailed study is well beyond the scope of our paper. Here we only wish to point out that, within these uncertainties, the elevated terraces around the Armorican Peninsula could simply be explained by uplift in response to local erosion (without calling for mantle dynamics or large-scale tectonics as proposed by previous studies). This variability in the terrace data is better discussed in the relevant section of the discussion and shown in the new Figure 9.

Comparisons with Pleistocene uplifts deduced from the elevation of marine deposits in central lowland Brittany are in agreements (16+/-2 m Ma^{-1} versus 12-15 m Ma^{-1}). This comparison suggests that erosion-driven uplift is enough to explain the elevated Quaternary marine markers in central lowland Brittany. Unfortunately, calculated uplift rates due to denudation in the western highs suggest a quasi-stationary surface diminishes by 4-10 m Ma^{-1}, whereas erosion rates are the highest (15-25 m Ma^{-1}). If erosion-driven is sufficient to explain uplifts in central lowland of Brittany, it seems that the model used in this study do not fit data in western highs of Brittany, where another additional origin may be suggested?

The comparison of marine sediment elevations with the model outputs is only reasonable in the central lowland because of the presence of Cenozoic marine deposits in the centre of the region. Such robust control points are not available in the western highlands. Therefore, in the west, we can only compare to the near-coast markers (marine *terraces (with the strong variability discussed above). It is therefore difficult to conclude about the western region and we limit our discussion to the central one.*

Another point of view is to consider erosion rates in western highs of Brittany as mainly driven by isostasy (taking into account higher incisions rates on higher elevation of the western Armorican Massif) and to consider another additional source for the lowlands of Brittany, such as recent tectonic control along the QNEF zone to explain differences between the higher denudation rates in western highlands than in eastern lowlands. This needs to be evoked in the Discussion. Comparisons with previous studies (Bonnet et al, 2000 ; Lague et al, 2000), based on quaternary fluvial incisions in Brittany, evidences unconcordant results related to differential uplifts from each part of the Quessoy-Nort-sur-Erdre Fault (QNEF) and uplifts rates ratio. Bonnet et al (2000) have studied the river incision and basin drainage, directly associated with quaternary uplifts. Oust and Vilaine rivers drainage basins are located on each part the QNEF trace. One on highland (Oust) and the other on lowland (Vilaine). Bonnet et al (2000) conclude that « the Oust drainage basin records 30 m of additional base level fall compared with the Vilaine basin, which represents an estimation of the differential uplift along the QFZ since the onset of relief formation ». Lague et al (2000) evidenced differences in uplit ratio on each part of the QNEF and CSA, with higher uplifts in western Brittany. Denudation rates calculated in this study are also higher in western highlands than in eastern lowlands. There is thus some changes of denudation and uplift rates between western highlands and eastern lowlands, with high denudation and uplifts to the west and lower denudation and uplifts to the east. Such discrepancy has been previously attributed to a localised tectonic control (QNEF). Authors consider that additional processes could induce additional uplifts signals not recorded on the elevated marine terraces along the northern Armorica Peninsula coastline. Unfortunately, there is no data and work presented on marine terraces (see specific comments).

Actually, this I not totally correct:

** Our study and those of Bonnet al. (1998) and Lague et al. (2000) (as well as Delmas et al., 2012) all agree that the western highlands show faster erosion rates and incision rates.*

** We show that uplift rates driven by the isostatic flexural response to erosion rates are faster in the central lowlands than in the western highlands (due to cumulative 3D effect, as explained in Section 5.3).*

** Bonnet al. (1998) and Lague et al. (2000) observe faster incision rates in the west and interpret them as indicative of faster uplift rates, which they associated with normal motion of the QNEF system.*

** Thus, the interpretation of Bonnet al. (1998) and Lague et al. (2000) is opposite to the uplift pattern solely due to erosion.*

** A possible solution is that the observed differential incisions rates are indeed due to differential uplift rates (on top of the erosion-driven uplift), such as normal motion of 10-60 m/Ma on the QNEF system. But that local motion is not recorded in the local marine terraces, albeit with the large variability discussed above (they should show differences of altitudes of 1-8 m between the western and eastern side of the QNEF).*

** A second possible solution is that differential incision rates identified by Bonnet al. (1998) and Lague et al. (2000) is not caused by differential uplift but by a difference in the erosion state of western highlands and central lowlands (different response time to base level variations during to different lithologies and morphologies).*

In this second interpretation, there is no conflict between our study and that of Bonnet al. (1998) and Lague et al. (2000), only a difference of interpretation regarding the cause of differential incision rates and the potential QNEF motion. We have reworked Section 6.3 of the Discussion to better explain this point.

General comments:

lines 280-284: There is a scarcity of sedimentation rates data and a lack of datations in this context.

Agreed. We are limited to available data.

Line 281: even if peaks of quaternary sediments are localized in the paleo-Fleuve Manche river, with rates up to 10-20 m.Ma-1, sediments do not provide from the little drainage basins of the northern armorican coast. Sediments

mainly originated from the Seine, the Somme, the Thames and the Solent rivers drainage basins, located in the central and eastern English Channel….

Agreed. However, we are only Interested in the mass accumulation / removal for the modelling step and the origin of the sediment deposits play no role in the lithosphere response to local sedimentation and erosion. As shown in our 2D tests, sedimentation / erosion processes located 100s of km away from the study area have no effect on local vertical motions due to the relatively low flexural rigidity of the lithosphere.

Moreover, the Quaternary marine sediments are highly mobile on the continental shelf, under the influence of waves, tides and currents. Older marine sediments may be remobilized on the continental shelf.

Agreed, that is why we do not consider sedimentation processes and rates over short-term periods but only as an average rate over the Quaternary period (plus the available data do not have the resolution to discriminate between short-term sediment remobilizations).

During highstand sea-levels, north armorican basin drainages may be responsible of only 1-5m sediment thickness on coastal areas. It is not clear if sediment peaks up to 10-20 m.Ma-1 have been integrated in the model.

Yes. As indicated, thick near-shore deposits of few meters during high-stands correspond to the 10-20 m/Ma average rates.

Line 283: subsidence rates?

Figure 5 shows the vertical velocity along a 2D-transect induced by different sedimentation rate models. We added an explanatory paragraph relative to this question.

Line 290: It is not clear how is calculated onshore uplifts versus offshore sedimentation rates using a simple 2D model. Could you explain the law input in this 2D model? It is specified that marine sedimentation is very low compare to denudation rates (ten times smaller) and that sedimentation rates have not been included in the model. I am agree, but is this paragraph necessary?

A short paragraph was added to explain more clearly this 2D-model and the sedimentation impact.

Line 386-387: Middle and Upper Pleistocene apparent uplift rates of Pedoja et al (2018) are estimated for the Cotentin Peninsula. The analogy with armorican peninsula and western european coastlines is only suggested by the authors.

We disagree. From Pedoja et al. (2018), discussing the full western Europe database (p. 351): "upper Pleistocene (MIS 5e) apparent uplift rates range from -0.016 ± 0.008 mm/yr to 0.16 ± 0.01 mm/yr with a mean of 0.05 ± 0.01 mm/yr (Fig. 10A). Apparent Middle Pleistocene uplift rates range from 0.02 ± 0.01 to 0.08 ± 0.02 mm/yr (mean 0.05 ± 0.01 mm/yr) (Fig. 10B)." Which we summarized as: "Based on present-day elevations of MIS 5e and MIS 11 marine terraces (Pedoja et al., 2018) estimate mean Middle Pleistocene and Upper Pleistocene apparent uplift rates of 50 ± 10 m.Ma-1 for most of Western Europe coastline, including the Armorican Peninsula".

In order to avoid the impression that the Armorican Peninsula is explicitly mentioned in Pedoja, we rephrased to "for most of Western Europe coastline, from southern England to Portugal".

Line 388-389: the original sentence in Pedoja et al (2018) is: « The onset of such Western European sequences occurred during the Miocene (e.g. Spain) or Pliocene (e.g. Portugal). We interpret this Neogene-Quaternary coastal uplift as a symptom of the increasing lithospheric compression that accompanies Cenozoic orogenies ». The mean uplift rate of 10 m. Ma-1 of some coastal NW european area with rasas is thus evaluated since Mio-Pliocene (about 5-20 Ma), not only during Pleistocene.

Agreed. We rephrased our sentence to: "they argue that these data cannot be explain solely by eustatic sea-level variations and require a mean uplift rate of ca. 10 m.Ma-1, due to an "increasing lithospheric compression that accompanies Cenozoic orogenies."" We use quotation marks to specify that the "increasing compression" associated with Cenozoic orogenies is a direct idea of this publication.

Line 390-391: Data from Pedoja et al (2018)?

Yes. We added the referece.

Line 395-396: Late Pleistocene uplift rates of the armorican peninsula coastal area estimated using marine terraces altitudes (MIS 5e) and potential regional eustatic sea-level (from 2 to 9m) give large uncertainties, with uplift rates varying from -23, -12, 12 and 31 m.Ma-1. With a Late Pleistocene regional eustatic sea level of 2 and 4.5m, the uplift rate appears to be negative. Is it subsidence ? Is this realistic ?

Yes, a low eustatic sea level of 2-4.5 m (proposed in the literature) implies a Late Pleistocene regional subsidence rate of -23 to -12 m/Ma to explain the elevated terraces. The convention "positive up", with negative velocities indicating subsidence rates, is used throughout the paper and explained at the beginning, so we do not feel it necessary to explain it again here.

A long-term Mio-Pliocene uplift rate of the whole Armorican Massif is evidenced by the elevated Faluns Sea deposits, but we are not aware of any direct evidence of Pleistocene vertical motion (other than the terraces). So a Late Pleistocene subsidence rate would be surprising, but cannot be ruled out. However, we do hint at a preferred interpretation in the following paragraph by indicating that a eustatic sea level of 5 m is most appropriate to explain the average terrace heights if the uplift rate is due to regional erosion.

Line 396: Add « respectively » after 31 m Ma^{-1}

Changed accordingly.

Line 408: The Pleistocene uplifts deduced by Bessin et al (2017) in central lowland regions of the Armorican Massif are between 15.5 in eastern Brittany lows and 28.8 m.Ma-1 in Carentan flats (Cotentin Peninsula). Comparison with uplift rates calculated under the influence of denudation rates in eastern Brittany lows (12-15 m.Ma-1, this study, line 336 and up to 22 m.Ma-1, if considering random model that must be considered as an upper bound, line 358). Considering only the nine basins eroding the eastern lowlands, the average rate of denudation is 9 +/-6 m.Ma-1 (this study, line 368). i.e. 3-15 m.Ma-1 In eastern Brittany lowlands, such direct comparison suggests few lower pure denudation rates (this study) than Pleistocene uplifts rates (Bessin et al, 2017), not really in opposition with Bessin et al (2017) conclusions (Pleistocene uplifts are related to either the intensification of the Africa-Apulia convergence or a climate-induced erosional enhancement of this long-term uplift)

This is not quite correct: Yes, our results show low denudation rates in the centra lowland (9 m/Ma), but the uplift rate due to the response to erosion is relatively fast (12-15 m/Ma) because it is driven by the isostatic flexural response to erosion rates from all around (central lowland, western highland, Cotentin, ...), as explained in section 5.3.

So, the erosion-driven uplift rate in the central lowland is equivalent to the uplift rate derived by Bessin et al., and there is no need to appeal to additional geodynamic or climatic processes.

Line 422 : « as suggested by the agreement with marine data » ??? What type of marine data ? Marine terraces of the armorican peninsula with very large uncertainties ? or also uncertain marine quaternary sedimentation rates around the Armorican Peninsula ?? If not, change marine data by : Cenozoic marine deposits.

Changed accordingly to "as suggested by the agreement with the Cenozoic marine deposits".

Line 425 : « These would produce strong uplift signals that are not recorded in the elevated marine terraces along the northern Armorican Peninsula coastline ». The problem is that no clear data on marine terraces are presented in this paper. If authors consider supplementary data published by Pedoja et al (2017), marine terraces from northern brittany are reported (with mean altitudes at 17m), but could not be interpreted directly without supplementary work, especially due to a lack of datations.

Agreed. We have rewritten this whole paragraph: "However, this would result in differential altitudes of ca. 1.5–8 m of the MIS 5e marine terraces on both sides of the QNEF system along the northern Armorican Peninsula coast. The variability in the local terrace data preclude a robust conclusion, but they do not show a clear east-west difference in altitudes (Fig. 9). ... Overall, the lack of marine deposits in the western highland and the variability in the marine terrace data preclude distinguishing between the two propositions and the potential activity, or not, of the QNEF system."

We also added the individual terrace data on the new Figure 9 to indicate that they don't resolve the issue.

Line 436: You need also to cite Quessoy-Nort-sur-Erdre Fault, as developed in the discussion.

Changed accordingly.

Line 440-442: I am not agree with this sentence for reasons detailed above, even if there is surely uncertainties in eustatic sea-level corrections.

The sentence is: "Considering the uncertainties in eustatic sea-level corrections, these erosion-driven uplift rates can explain the uplifted Late Pleistocene marine terraces along the Armorican Peninsula coastline (Pedoja et al., 2018), without necessitating additional geodynamic processes such as regional compression or local active faults. "

We keep this sentence because, iIn our opinion, it is a clear and careful statement of our results and conclusion. We acknowledge the uncertainties associated with the marine terrace data and eustatic correction, and we state that, given these uncertainties, the data "can" (not "must") be explained by erosion-driven uplift without a need for additional processes. We do not pretend that erosion-driven uplift is the sole mechanism, nor that other mechanisms must be rejected. But we do show that it must be taken into account, at least as much as other propositions in other studies that were not based on any modelling or quantification attempts.

<u>Technical corrections</u>

Line 60: the four main domains of the Armorican Massif are separated only by major crustal-scale shear zones, i.e. NASZ and SASZ. This includes the Leon Domain, the North Armorican Domain, the Central Armorican Domain and the South Armorican Domain. The Quessoy-Nort-sur-Erdre Fault (QNEF) is not considered as a fault bounding one of the domains of the Armorican Massif. Please correct the sentence.

Noted. We changed to "It comprises four main tectonic domains separated by major crustal-scale faults and shear zones (North-Armorican and South-Armorican Shear Zone systems), with other major fault systems also noticeable (e.g., Quessnoy-Nort-sur-Erdre Fault, QNEF, Fig. 3)."

Figure 1: stripes are green and not blue. Please correct. Please add the trace of the actual coastline and indicate the complete reference of BRGM used to produce this geological map. The map legend is not geology, but lithology.

Changed accordingly for the stripes colour. The Geological map and associated informations is moved to Fig. 3.

Reviewer 2 (Anonymous):

The approach is well-explained, and the interpretation aligns with the presented results. I have some major concerns regarding the geomorphology of the study area, along with suggestions for the figures and the method-results sections. I am convinced that the paper can be significantly improved by adding additional information and details. I believe that the suggested changes require moderate-to-major revisions. Overall, most components of the manuscript are in pretty good shape, and the authors should be able to address my comments fairly easily, as my recommendations do not involve substantial additional analyses or changing interpretations.

We acknowledge your concerns and remarks. We clarified the text and we have addressed your questions and remarks.

Major comments and recommendations:

I really appreciate the study and interpretation, but there is absence of a description of the geology and relief within the sampled basins, constituting the most significant weakness of the current manuscript. When dealing with cosmogenic nuclides to quantify denudation, it's crucial to discuss and present some details, such as figures and field pictures of the study areas. A more comprehensive understanding, particularly of quartz-bearing lithologies, is necessary when interpreting 10Be-denudation rates. This could be addressed by adding a figure (details in the comment below) and a few lines in the main text while describing the sampled basins. Thus, I highly suggest adding a figure that includes hillshade and topography of the study area, along with field pictures. For example, include Figures A and B, each with a zoomed view – one focusing on the western region and the other on the central region. Present topography, elevation scales, sampling points, and watershed boundaries in these figures.

As requested, we added a new figure showing picture examples of typical sampling sites (Fig. 2) as well as figures of the regional topography, slope, relief and lithology (Fig. 3).

A more detailed methodological analysis for each drainage basin and sampling site could be conducted, but our objective in this study is to provide a general estimation of regional denudation rates (and the associated geodynamics), not a specific study of individual sample and site. More specific analyses can be conducted in the future using our data, refined morphological and geological studies.

The structure is a bit confusing; authors are blending methods and results in the same sections (perhaps this was intentional).

We have kept the overall structure but added a "Method" section at the beginning of section 5 to better separate from the results.

A very good work has been done in the chapter 3.1 and 3.2 where the nicely discuss the methods and results in two distinct sections. I would suggest maintaining this format for the rest of the manuscript, specifically in sections 4 and 5, b by describing the methods first and then presenting the results. The information is already in the chapter, so the authors can unpack and restructure sections 4 and 5 accordingly.

Agreed, for Section 5 (new section 5.1. Methods). For Section 4, we prefer keeping the present structure because this section is essentially a description of the model construction, without specific "method" to individualize.

The authors did a good job in discussing previous data from marine terraces and sedimentation rates carefully, comparing them with 10Be-denudation rates. I really appreciated that. However, please consider adding all this information in the figures or creating a new, more exhaustive figure that incorporates all the data.

The individual terrace data (converted to uplift rates) are now presented in the new Figure 9. The actual values are given (with detailed information in Pedoja et al., 2018 supplementary material).

The authors employed elevation and basin-average slope as controlling factors for basin-wide denudation rates. However, I suggested using ksn (channel steepness) and local relief (the difference between minimum and maximum elevations). By applying the stream power law, they can also estimate erodibility parameters to predict

denudation values. I acknowledge that this is a new analysis to undertake, but it would offer an additional constraint for comparison with the nice dataset compiled by the authors.

We agree that other morphometric parameters can be tested for better predictive values. However, as discussed in the text, this kind of analysis was attempted in several studies with little success, other than pointing out that the slope is usually the "less bad" predictor. Since we base our erosion-rate model on data from the entire Western Europe, redoing such an analysis would not be a small task and would likely call for a new study in itself (e.g., estimation of morphometric parameters for the whole database). This particularly true of a channel steepness analysis that would require recalculating ksn for all sampled basins in western Europe. This should certainly be done in a new study.

Given the uncertainties and variabilities in the data and methods, and as we do take a conservative approach in order of magnitude interpretations, we consider that adding more complex parameters will not change our main conclusions.

I also suggested comparing their data with data from other slow tectonic settings. This is firstly interesting for discussing the study area within a global context and providing insights into the mechanisms that drive such erosion values.

We wholeheartedly agree that such a comparison would be interesting, but this would also be a major endeavour. A global compilation of denudation rates and geomorphic indicators in slow tectonic regions would be a good basis for studying the processes that drive erosion. We'll keep this in mind for a future dedicated study,

Secondly, it would be valuable to include a brief discussion on the climatic impact on denudation rates by comparing averaged precipitation or climatic data with denudation rates in other settings. This can be done by just adding a small chapter or few lines in the section 6.1or 6.3.

As discussed in the text, our TCN data represent erosion rates over the last 20 to 200 kyr (on average 45 kyr). During this period, the climate and the associated parameters (e.g., vegetation cover) have seen major changes from glacial to interglacial. It would be very difficult to define climatic variables (precipitation, temperature, evapotranspiration) for our study area, other than using generic values without specific local estimations. A detailed comparison could be done with present-day climatic variables, but it would not be very relevant considering the time range sampled by the TCN data.

Line 10: "and larger scale processes (e.g., crustal and deep-seated dynamics)"

Changed accordingly.

Lines 24-25: Please be more specific, it is too general. For instance, South Africa is highly debated if stable (e.g., Erlanger et al., 2012) of supported by mantle dynamics during the late Cenozoic (e.g., Roberts and White, 2010 and many others). This example needed to be discussed and compared in the manunscript

Erlanger, E. D., Granger, D. E., & Gibbon, R. J. (2012). Rock uplift rates in South Africa from isochron burial dating of fluvial and marine terraces. Geology, 40(11), 1019-1022.

Roberts, G. G., & White, N. (2010). Estimating uplift rate histories from river profiles using African examples. Journal of Geophysical Research: Solid Earth, 115(B2).

We modified to: "i.e., in areas unaffected by direct plate-boundary processes (e.g., Australia, South Africa, northwestern Europe)".

The case of South Africa and the fect of dynamic topography are very interesting but also strongly debated. Considering that we do not address dynamic topography and limit our study to the Armorican Massif, we prefer not to good into this level of details here. As for the comment about "slow tectonic region", we agree that this is a kind of analysis and study that should be done carefully.

Lines 26-27: Not only, even river terraces.. there are also other geomorphic markers that can be used. Please rephrase

Agreed. We changed "coastal or alluvial landforms, such as marine terraces, planation surfaces (rasas) or river terraces, and from sedimentary record, such as raised beaches or endo-karstic infilling"

Line 30: OK, but please also cite recent works for each mechanism. & **Lines 30 – 31: This should be the challenge/gap. However, the studies cited by the author are outdated. There are more recent works, (Erlanger et al. 2012 and others) that discuss that hypothesis. I suggest a better rephrasing to explain the gap in knowledge that the author wants to fill**

We are not aware of much more recent studies that discuss the role of erosion or other processes in driving coastal uplift. Plus, references from the early 2000s may be 20 years old, but it doesn't mean that they are outdated (at least to the 2nd author). The study of Erlanger et al. (2012) does show the steady-state nature of incision rate / coastal uplift rate in South Africa during the Cenozoic but it does not address the origin of this slow uplift rate.

We added the reference to Erlanger et al. (2012) as an example of a study that addresses the nature and timing of coastal uplift in a stable continental region. But we do not see how to better phrase the issue that what is currently written: "Surprisingly, none of these studies consider the role of long-term erosion as a potential driver of coastal uplift through simple isostatic adjustment."

Line 33: they are not period

Agreed, "period" modified to "epochs"

Line 34: please be more precise. rejuvenation or uplift?

"upheaval" modified as "uplift"

Line 34 – 35: unclear, please rephrase. spatial variation of uplift?

"including indications of spatial variations that may reflect local fault reactivation" modified as "including indications of spatial uplift variations that may reflect local fault reactivation"

Line 36: "also" suppressed

Modified accordingly

Lines 36 – 37: this is unclear. I understand the general meaning but author can better rewrite. Surrounding = regional. sedimentation system is vague. "specific advantages for testing the role of erosion in local and regional uplift" this concept should be better explain below the opening in order to clarify the challenging

Modified to: "Due to its peculiar geography, it is only affected by local erosion. Mass redistribution from regional inland sources is not possible due to the watershed geometry, and offshore processes (e.g., talus deposits) are negligible due to their low long-term magnitudes (cf. Section 4)."

Line 38: "overall" suppressed

Modified accordingly

Line 41 (Figure 1): The figure would increase readability by adding shoreline and hillshade topography in the background with a transparent geological map

Figure 1 was modified to include the coastline. Slope and lithology were added in the new Figure 3.

Lines 45 – 47: unhandy sentences. I suggest to remove this summary from the Introduction. Please use this space with general information about method ("Action") and conclude the introduction by describing major findings.

We apologize but we don't understand the comment. This section is in fact a short description of the method (what will be done) and the main results. Since the other two reviewers did not comment about this section, we keep it as is.

Line 49: "(Section 4)" suppressed

Modified accordingly

Line 50: "(Section 4)" suppressed

Modified accordingly

Line 51: “Amplitude” highlighted. Usually amplitude is referred to topographic wavelength. Here, I would use "rates of" denudation rates

Replaced by “magnitude”

Line 52: “geological” highlighted

Replaced by “absolute”

Lines 53 – 54: “the flexural isostatic response to local denudation rates, without need for additional processes such as lithosphere bulging or local fault activity.” highlighted. Here, the author may state the wavelength of such flexural response proposed.

Agreed. The associated wavelength is now mentioned.

Line 64: The Armorican Massif is composed of

Modified accordingly

Lines 65 – 66: planation surface and stratigraphy offshore? I am not familiar with that work but they seems to use stratigraphic contraints.

Agreed. Mention to stratigraphic constraints added.

Line 66: ??

The hyphen is missing: “mid-Mesozoic”

Line 66: Usually, when you talk about time, early, middle and late should be without capitols

Changed accordingly

Lines 74 – 75: They can provide a good marker for estimates of long-term regional uplift

Agreed, replaced by “While the Faluns deposits are good markers for estimation of long-term uplift rates by being characterized by shallow-depth open marine fauna, the Red Sands presents lower robustness for such an estimation due to their complex nature of continental sheetflood, fluvial, and estuarine deposits (Néraudeau et al., 2003; Brault et al., 2004).”

Lines 76: "architecture"

We prefer to keep “characteristics” because it includes not only the architecture but other parameters as sediment types.

Line 84: “tend to be spatially associated” is highlighted and: Please provide more details. "rivers drain E-W following main tectonic structures" for instance

We added “(e.g. following the major NW-SE exhumed shear-zones in southern Britany)” as an example.

Lines 87 – 88: suggested editing: "on the landscape evolution" or "topographic development" of different "sectors/domains" of the Armorican..

Modified to “topographic evolution of different sectors of the Armorican Massif.”

Line 92: 60 m respect to what? 60 m is the difference between the lowstand and highstand?

The 60 m is relative to the current sea-level and is the assumed secondary flood limit.

Line 92: Agree, nbut pleae explain how the author estimate this precise values.

Estimation was made using the assumed eustatic highstand and ESR dating. We added “and ESR (Electron Spin Resonance) dating”. See above for the finite uplift estimation.

Lines 100 – 101: “for western Brittany (31 terraces), 5 m for western Cotentin (7 terraces), and 8 m for the Channel Islands (4 terraces).” id highlighted, and: nice summary, however readers can not figure out where are these locations. Is it possible to add in fig. 1 the locations?

Locations are added in the new Figure 9.

Lines 105 – 107: That depends on the locations, right? If yes, I suggest to better organize

The proper value depends indeed of the location but the obtention of all local values are not possible. We present here the main, regional trend. We added “regional trend” to support this.

Lines 110 – 111: “We come back to these estimations in Section 6.” is strikethroughed.

Changed accordingly

Line 114: Please add these two major rivers in the figures

Theses two rivers are added in the figure 3a

Lines 116 – 117: I kindly ask to rephrase this sentence since unclear.

Modified to “Assuming that this morphological difference is inherited from a differential uplift since the Pleistocene, these studies indicate a relative west / center uplift rate of ca. 10 – 15 m ·Ma^{-1}.”

Line 127: suggested revision: To estimate, without in order.

Modified accordingly

Lines 130 – 131: Assuming that the quartz-bearing rock is evenly distributed within the sampled catchments.

Agree with that at first order, and assuming that the erosion rate is indeed homogeneous headward… The precise relationship between quartz percentage, mass participation into the fluvial system, production rates, etc. is unknown. However, as we are mostly interested in the order of magnitude of the erosion, and given the ubiquitous presence of quartz in the area, we believe that out approach is sound. We added “as long as the main hypothesis of this method are respected (e.g. nearly homogeneous quartz content in the watershed).”

Line 138 (Figure 2 caption): what does it mean? <u>And:</u> Please adding a color bar in the legend for elevation.

The colour-bar is added in the figure. “Octopus drainage basin denudation” means that the other watershed-scale denudation rates we use are directly taken from the Octopus database, the reference is present in the same caption.

Lines 160 – 164: “Drainage basin average denudation rates were derived from 10Be concentrations using the online CRONUSEarth system (Balco et al., 2008) (https://hess.ess.washington.edu/). This commonly-used calculator comprises some simplifications and assumptions that we consider reasonable given our study context: e.g., denudation rates are at steady-state; river sediments do not have a complex history following first exposure (no significant burial in river terraces); mean watershed slope and latitude are used for computation of the production rate scaling factor. Given” is highlighted And: Here, I understand that we are in the result session of 10Be-derived denudation rates. So, details about final conversion of in situ-produced 10Be to basin-wide denudation rates are better placed in the above method section.

Agreed. The whole paragraph (from 160 to line 167 is moved at the end of the previous section.

Line 165: They are not very small..

It depend on the definition of small. If compared with study as Stutenbecker et al. (2017), our watersheds are indeed rather big. However, if compared with other studies as for instance Molliex et al. (2016) our watersheds are small. Altogether we defend that we sampled small watersheds for regional studies in stable continental area (see figure 2)

Line 167: author can consider to refer to Table 1 in the previous sentence, instead to write a sentence within the text.

This line is removed from the text and a reference to the Table 1 added in the first sentence of the section 3.2.

Lines 177 – 179: I suggest to break and not mix the description of Llyon and Leff with Argenton

"Differences for the Layon and the Leff rivers are within the uncertainties, while the Argenton site shows a 4.5 m.Ma^{-1} discrepancy, outside of the measurement uncertainties even at the 99% confidence level." modified as "Differences for the Layon and the Leff rivers are within their respective uncertainties. The two measurements of the Argenton site shows a 4.5 m.Ma^{-1} discrepancy. This value is outside of the individual measurement uncertainties even at the 99% confidence level."

Lines 179 – 180: general sentence suitable for inclusion in the method section

We chose to let this explanation here because we do not discuss substantially the TCN results latter and we think that we must point out some warning about our results for morphological inferences or later use of our results. We do not go in detail into that kind of discussions because our aim was to test the flexural isostatic response to the erosion. Such small scale "problems" or variations are first, smoothed by the lithospheric rigidity that act as a low-pass filter and, second this lithospheric response itself is associated with large uncertainties and assumptions (e.g. the "proper" value of the flexural rigidity).

Lines 181 – 182: There are some papers that discuss this issue, like Von Dongen et al., 2019; Carretier et al., 2009. Why do not compare your results and better discuss that in the Discussion chapter?

van Dongen, R., Scherler, D., Wittmann, H., & von Blanckenburg, F. (2019). Cosmogenic 10 Be in river sediment: where grain size matters and why. Earth Surface Dynamics, 7(2), 393-410.

Carretier, S., Regard, V., & Soual, C. (2009). Theoretical cosmogenic nuclide concentration in river bed load clasts: Does it depend on clast size?. Quaternary Geochronology, 4(2), 108-123.

See above. Given the few data we have, any attempt for a proper morphological discussion would be dubious. We did check, albeit not thoroughly for time/cost reasons, the grain size parameter in case of major, generalized granulometric control. As we found the same order of magnitude in the denudation rate, we are confident in the afterward constructed erosion-rate maps.

Lines 183 – 184: Interpretation, this should be placed in the discussion.

We disagree. This is rather associated with the direct results (estimation of denudation rates), and we prefer to keep the discussion to the more speculative parts on regional processes and geodynamics.

Line 187: suggested revision: change with bedrock

We change with """ as the outcrop term gives informations about the morphological context. We did not, for instance sampled cliffs.

Table 1: I would suggest to add the columns of lat and long f the sampling locations. Usually, even the amount of 9Be carrier added is reported. <u>And</u>: Bedrock samples? not clear, please change with bedrock even in the text

"Outcrops" modified to "*outcropping bedrock*". *We add a supplementary table with the input data for CRONUS online calculator.*

Line 203: This first part of the result section 4.1. sounds as a discussion section. Authors may consider to place these in the discussion.

Cf. previous answer on "interpretation".

Line 204: D

Modified accordingly

Lines 204 – 206: That is true. I felt frustrating to not see any pictures or basin analysis so far. what seems missing is pictures of bedrocks/field and river profiles and/or some detailed analysis of some basins showing the topography wihtin and lithology. I felt that geological observation and basin-scale analysis is kind of missing, I believe the paper would benefit and improve readability with these adjustments

We added a new Figure 2 with sampling site pictures and new Figure 3 with morphometric and lithology information.

Line 208: suggested revision: controlling factors

We preferred the term explanatory as being part of a predictive modelling approach and that we do not try to constraint au causal link between a given parameter and the denudation rate. Changed as "statistically predictive".

Lines 211 – 212: I know that is a general statment. But since author are working in a slow tectonic settings i would suggest some comparison with similar areas, see compilation in Clementucci et al., 2022; Piefer et al., 2021. Very often when uplift is evenly distributed or very low, rock-types and clima play a major role more than slope and steepness in controlling 10Be denudation rates and topographic reliefs. I really think that would be very interesting to discuss slow tecotnic areas in the discussion section.

Clementucci, R., Ballato, P., Siame, L. L., Faccenna, C., Yaaqoub, A., Essaifi, A., ... & Guillou, V. (2022). Lithological control on topographic relief evolution in a slow tectonic setting (Anti-Atlas, Morocco). Earth and Planetary Science Letters, 596, 117788.

Peifer, D., Persano, C., Hurst, M. D., Bishop, P., & Fabel, D. (2020). Growing topography due to contrasting rock types in a tectonically dead landscape. Earth Surface Dynamics Discussions, 2020, 1-29.

As mentioned earlier (under general comments), we agree that a comparison with other slow tectonic settings would be interesting. But doing it right would require a specific study. Adding a discussion in this paper would mean either changing its scope (and adding several paragraphs) and doing a superficial comparison.

Line 227: It is ok, but please consider that usually the small scale basins when stochastic processes may play a major role are ca. 10-30 km2.

Noted

Lines 230 – 231: Elevation is not a very important parameters! Usually the controlling factors are local relief, slope or channel steepness of the rivers. Why authors are considering in this analysis elevation?

As stated before, our approach is a statistically predictive one, not a direct causal modelling. Hence we assume that any type of morphological parameters can be tested. Furthermore we point out that the elevation can be viewed as a proxy, albeit probably badly constraint for both climatic variations and relief.

Figure 3: "Please consider the following suggestions:

-add a legend and

-add the outcome of best-fit power law like R2 and/or p-value.

- Authors can even better explain this upper and lower boundaries, are they confidence interval of 95%?

- adding the error bars at least for denudaton rates.

- if you consider elevation and area has controlling factors why don t add the plot of area vs denudation, elevation vs denudation.

We prefer not to add the legend in the figure itself but rather to leave it in the caption. The error and the upper/lower boundaries are more explained in the caption. We do show only the slope vs denudation graphs as it is the best estimator for synthetic denudation rate.

I suggest to estimation ksn and showing the correlation with basin-average channel steepness (ksn) which can provide information about uplift and bedrock erodibility. And so adding ksn as a controlling factor"

The use of ksn might indeed bring further informations relative to the morphology and the local morphological state but we assume that it won't bring useful informations for the long-term erosion rate that we seek for, furthermore, it will add complexity to the same data set (the DEM), aggregating uncertainties for no more than adding a mere descriptor. Furthermore, the proper use of ksn would imply the computation over the whole western Europe and *probably a dedicated methodological study prior to its application into hexagon. Alternatively, it would require to change the hexagon to watersheds. Therefore we keep this suggestion for a possible future development.*

Lines 240 – 247 : Ok, a similar approach has been applied by using the stream power law (Whipple and Tucker, 1999) and converting ksn to erosion values (e.g., Adams et al., 2020, Gallen and Thigpen, 2018; Clementucci et al., 2022 for instance). Basically, E = ksn*K, assimung n=1, I am wondering if the author can consider to compare or at least discussing the difference with other similar approaches.

Gallen, S. F., & Thigpen, J. R. (2018). Lithologic controls on focused erosion and intraplate earthquakes in the eastern Tennessee seismic zone. Geophysical Research Letters, 45(18), 9569-9578.

Clementucci, R., Ballato, P., Siame, L. L., Faccenna, C., Yaaqoub, A., Essaifi, A., ... & Guillou, V. (2022). Lithological control on topographic relief evolution in a slow tectonic setting (Anti-Atlas, Morocco). Earth and Planetary Science Letters, 596, 117788.

Adams, B. A., Whipple, K. X., Forte, A. M., Heimsath, A. M., & Hodges, K. V. (2020). Climate controls on erosion in tectonically active landscapes. Science Advances, 6(42), eaaz3166.

As above, this kind of approach would lead to a more dedicated work. We keep in mind this suggestion for possible future works.

Line 254: “A random model” is highlighted <u>and</u>: It does not seem very promising approach to predict denudation, why author chose to present the random model too?

Indeed. This “random” model was made to test the sensitivity of the lithospheric response to the erosion. We recognize that it does not make any physical sense but it has to be seen, instead as a parametrization test. The veracity of the best solution, or any kind of synthetic erosion rate being dampened by the associated uncertainties our aim was to test if such inaccuracy could be problematic for the uplift/strain estimation.

Lines 260 – 261: I am confused by this description (perhaps it's due to my gap in knowledge). Since the hexagons are 100 km², how is it possible to observe a 10 km small wavelength distribution?

The “10 km” wavelength is the minimal linear step for erosion rate variations as the 100 km² hexagon are associated with a ~ 6 km long nod spacing. We add “the 1D nod spacing” for clarity.

Lines 274 – 275: Actually, the outlet of local river extend to the continental shelf. Please rephrase for clarity

It seems that we have to disagree on the definition of outlet as it stands for the end of a river toward a lake, a pond or the sea. Therefore it can not be located offshore.

Line 276: a, b, c

Changed accordingly

Line 278: “paleo Fleuve Manche river.” is highlighted <u>and:</u> I know where it is, but please add to the figure 2 the name.

We added the information on the figure 4.

Line 281: I thought that that thickness has been deposited only during the last interglacial period. I am not familiar with Aufris et al. papers, they dated core up to 2.5 Ma? Few meters of thickness should be from the last cycle usually. Maybe I am wrong, can authors comment on that?

Augris et al. 2013 are referring to a compilation of available data. Their work provide three independent sedimentation rate types which are associated: fluvial deposits, sand-bar and undifferentiated sediments. The precise age and stratigraphy is not provided and we extract the main elements from this data-set in order to create the three sedimentation inputs that we use.

Lines 282 – 284: I would say are in the same order of magnitude, authors should compare the depocenters which are 10-20 mMa, and so, similar. If the time 2.5 Ma is well contrained by stratigraphic observation, authors can state

that 10bedenudation is likely the same (at least the average over a larger timescale than basin integration time scale itself). That is very interesting!

See above. Furthermore, given the complexity of the available data in term of spatial and temporal sedimentation and given the numerous gap in the sedimentation quantification a "mass-balance" type of study seems out of the current possibility despite its expected important value. For instance, the high sedimentation area (10-20 m/Ma) are very localized zones and can not be properly called depocenters in the same way as we can use for large continental basins.

Lines 290 – 291: Can you please explain the equations and how 2D model has been estimated? As in the general comment, I wouold suggest to make a method section before discussing the results.

We modify the manuscript structure and add more information relative to this point. We hope it helps the reader understanding more clearly our method.

Line 301 (Figure 5 caption): Add tthis information and explain the model in a method section please

We Modified the section with a 5.1 Methods were both the 2D (Fig.5) approach and the regional flexural responses are modelled.

Lines 317 – 320: Please relocate this information to the methods section along with details on the 2D model

As above

Lines 335 – 336: suggested revision: I would remove that general sentence.. when is possible authors may consider to cite the figures at the end of the text.

Changed accordingly. We modified "Figure 6 (A, B, C) shows the uplift rates predicted for the mean denudation rate model associated with the three elastic thickness cases. All three models [...]" as "The three cases of elastic thicknesses modelled uplift-rates, using the mean erosion rate (Fig. 6, A, B, C), [...]".

Lines 339 – 341: not very clear, and this sounds as a discussion lines

"Other potential geographic patterns (e.g., basins eroding the South Armorican Shear Zone) cannot be identified." Modified as "No other geographical pattern, as for instance a north-south differences can be identified"

Lines 348 – 349: I am not sure about this statement, even the mean model consider the nonlinear relationship, it it correct?

The relationship between a given slope and the predicted denudation rate is indeed non-linear for the mean equation (as well as for the two bounding ones). The non-linearity that we express here is related to the unknown equation to be used. In other word, for a given slope the "real" predictive equation is either close to the mean or close to a bounding equations. This uncertainty is non-linear as the difference between the lower-bound and the mean is always smaller than the difference between the mean and the upper bound and that, the higher the slope value, the higher the asymmetry. For example, for a mean slope of 30° (not existent in the studied area), the upper bound predicts denudation rate >> 2000 mm/ka while the mean equation predicts ~ 800 m/Ma and the lower one ~ 200 m/Ma.

Lines 356 – 359: I do not get it sorry, why it should be an upper bound?

Under the used assumptions, it is an upper bound because using a probability of 1/3 for each polygon to be set at the upper equation leads to an artificial over-estimation of the total denudation rate when compared to a more gaussian distribution around the mean. Furthermore, due to the asymmetry of the envelope around the mean, the "deficit" in erosion due to the 1/3 polygons forced at the low-bound values is overcompensated by the 1/3 ones forced at the high-bound values.

Line 362 : Here as in the previous comment, suggest to add a few lines of comparision with rates from different slow-tectonic settings. Where similar or different mechanisms has been discussed to promote such a rates of denudation. In that way, the authors can also discuss the impact of climate as possible controlling factor in the denudation rates (e.g., Erlanger et al., 2012; Piefer et al., 2021; Clementucci et al., 2022).

This discussions relative to controlling factors are indeed interesting on their own and might be useful to go further into the understanding of the regional landscape evolution. Unfortunately we think that this kind of study should be undertaken with a dedicated methodology, bibliographic review, etc. and is therefore out of the scope of this paper. Furthermore, as we are in the long run mostly interested by the uplift rate, the subsequent lithospheric dynamic and associated deformations (e.g. seismic activity), we also point out that many uncertainties arose from this side (e.g. proper definition of the lithospheric rheology) that should also be addressed by dedicated study.

Line 363: They are not very high, I would remove this statement

We point out that they are indeed not high enough to prevent the obtention of a regional estimation.

Line 365 : I am convinced that this description is accurate, but it is challenging to observe the morphological domain without a topographic/relief map. Please consider adding a figure, such as Figure a and b (two zoom), including hillshade and topography, sampling points, and watershed boundaries

We added a new figure with field photography (Fig. 2) and more maps (Fig. 3) in order to provide more understanding of the morphological context.

Lines 371 – 373 : this should be pllaced before the previous sentence which is more general

We disagree as the previous sentences referred to our data while this one is a supported from other studies. However, we modified "Quantitative morphological analyses of the topography indicate higher incision and erosion by a factor of about 1.5–2 in the western highland than in the central lowland (Bonnet et al., 1998; Lague et al., 2000)." as "Bonnet et al. (1998) and Lague et al. (2000) proposed a higher incision and erosion by a factor of about 1.5–2 in the western highland than in the central lowland by the use of quantitative morphological analyses of the topography." for making this point clearer.

Line 373: "The implications in terms of uplift derived in these studies are discussed in Section 6.2." strikethroughed

Changed accordingly

Line 374: spatial variability

Changed accordingly

Line 374: consistent

Changed accordingly

Lines 380 – 382: Agree with the general meaning, but be aware that there are several new contributions discussing the possibility that 10Be-denudation is mostly mechanical erosion and very often underestimate the total denudation (chemical+mechanical), this depends on the main lithotype

Noted.

Line 388: If is only from MIS11 ca. 400 ka, 10 mMa is kind of 20% of apparent uplift. A bit low.. are you sure of these esimations?

Theses are estimations from Pedoja et al. 2018. They do present a rather large uncertainty depending on the sea-level variations to be applied. However, this work point out a significant uplift, albeit slow during, at least the Quaternary. As their determination are sometimes slightly away from our studied area (e.g. the paleo-island of La Hague in the Cotentin), we prefer to consider the order of magnitude, as we do for our own data.

Line 389: Is there a compression?

We here only site the explanation from the literature. This compression however makes sense when considering the tectonic stress (ridge push with an overall NW-SE orientation).

Lines 396 – 397: Could the authors create a figure with all these rates plotted, using arrows to indicate approximate locations? This can be accomplished by adding these data to Figure 5, allowing for a comparison between the model, new data, and previous estimations

We added a dedicated figure summarizing the available data and main results from this study (uplift rates, location of uplifted marine deposits, etc.).

Lines 400 – 405: Not very clear to me, can authors please rephrase and better describe these two scenarios

We sensibly modify the section. We hope it makes thing more clear.

Line 407: Please consider adding arrows and data in figure 5.

We added a summary figure in order to show theses kind of informations clearer.

Line 416: uplift?

No, elevation.

Line 417: Not very clear, please rephrase

"the western highland region elevation diminishes by ca. 10 m.Ma^{-1}" modified as "the absolute elevation of the western highland region diminishes by ca. 10 m.Ma^{-1}".

Line 417: "low rates of uplift".. or "different evolution"

We added "topographic" for the sake of clarity (as it takes into account both uplift and erosion)

Lines 418 – 420: I understand what the authors attempt to say, but is confusing right now. Suggested revision: persistence of the present-day elevation is not clear. maybe "erosional steady state conditions" that promote a steady topography.

"These very low rates of evolution indicate a persistence of the present-day elevation differences throughout the Quaternary, despite the Mid Pleistocene reconfiguration of the drainage network (Guillocheau et al., 2003; Brault et al., 2004)." modified as "These very low rates of topographic evolution indicate a persistence of the present-day East-West elevation differences throughout the Quaternary, despite the Mid Pleistocene reconfiguration of the drainage network (Guillocheau et al., 2003; Brault et al., 2004)."

Line 420: Please use term already used in the text. Here, I can not understand where is this region. Or if is very important report in the figure the term

"Quessoy Fault" modified as "QNEF system"

Lines 424 – 425: It depends on where the faults are located. If they are situated on the coast, the marine terraces may record such an increase in uplift rates. Here we need to know where is this tectonic lineaments (see the above comment too).

"Quessoy fault" modified as "QNEF system", that is present in Fig. 1

Line 426: please use a better reference, not clear where it is

"the differential east-side-down signal" modified as "relative positive west uplift compared with the east region".

Reviewer 3 (Andrew Wickert):

Dr. Malcles and coauthors present an argument that the recent uplift of the Armorican Massif is entirely due to erosional isostasy. I think that their argument seems plausible. However, I do not think that they (yet) demonstrate this.

Agree, we won't say that the erosional-driven uplift is the only one explanation for the Britanny morphogenesis and uplift but we do think that is the major long-term driving process.

I would be glad to see a revised manuscript draft in which they carefully describe their data-analysis and modeling approaches, including more careful (and sometimes more accurate) descriptions of both their cosmogenic 10Be approaches and their flexural isostatic modeling. This review is a bit limited because I am not familiar with the geological setting and therefore cannot evaluate its accuracy. Line-by-line comments, some substantial, follow:

Thank you for the review, we hope our answers and modifications will add more clarity to our point.

36-38. I do not understand how a region could be affected only by its own local erosion unless it is fully bounded by inviscid lithosphere-spanning vertical channels. Meaning: you get yourselves into some trouble further on, which I discuss.

Our point holds for the surface processes, relatively to long-term evolution, the drainage pattern of the Armorican massif being almost radial, there is no mass-transfer to be expected from surrounding region, onshore. This assumption holds mostly for the western and central areas while the proper "zero" uplift effect line induced by contiguous region mass redistributions cannot be precisely estimated (depending on the Te value under the elastic-bending theory). Offshore mass redistribution is more complex to address, especially for short-term discussions that we do not undertake here. Indeed, during high-stands, the sedimentation occurs near the current sea-shore while theses deposits are removed to some extent during the low-stands and redeposited farther toward the continental talus. We tested these two models and they are expected to be a minor impact regionally, if any. The long-term net effect is then assumed to be null, but we cannot address properly the short term impact, as for instance the sole effect of water-rise (or fall) do implies mass redistribution hence flexural response onto the regional dynamic. Indeed, other large-scale processes cannot be utterly ruled out (as mantle dynamic, ridge-push, etc.) but they appears to be of secondary importance, if any, for the studied time-scale.

39. Because of the hierarchical structure of watersheds, I think that all landscapes would have small watersheds. Therefore, I do not know that this is an advantage specific to your field area.

Indeed, almost all region can be subdivided on small-watersheds but if the regional mean absolute watershed size is too large, it can leads to several problems that are not impacting in our case. Keeping in mind that only a limited number of samples are to be measured (therefore the "every-confluence" sampling strategy not to be use), if the studied area is too big, we either have to sample the main watersheds, hence to have a mean denudation rate encompassing different morphology, rock types, production rates, etc. or we can stay with rather homogeneous rock-type, morphology etc. within each basins, that means only to measure the watershed heads. We added: "Their relatively small size allow for near total watershed sampling while keeping a rather homogenous morphology, rock-type, production rates, etc., "

49. Europe --> European

Changed accordingly

52. No spaces needed around this endash (and others like it: X and Y, X date through Y date). Note: Spaces are used when the endash is being used like a comma, to separate a subordinate clause.

Noted, modified accordingly

95. Remove parentheses around Bessin ref

Modified accordingly

98. Minor note: "rasa" is a new word to me. Probably don't mind this; I imagine that it is broadly used in your community.

That is indeed a rather common word in coastal geomorphology. Anyway, we added "(sea-driven planation)" for more clarity for non-geomorphologist readers

117-118. No parentheses around Bonnet ref. Could you provide some quick context around the archaeological findings -- what is it that linked these to differential uplift rates?

We added "(lithic industry from the Cromerian stage, 0.7 – 0.5 Ma found by Monnier et al (1981) in the upper terrace)". This archaeological findings are used as a dating tool for incision rate estimation.

112-124. This paragraph is close to "X1 says Y1 about Z1, X2...", especially when the final sentence seemingly strongly opposes a number of prior sentences. I think it is *okay* as is, but I wonder if there is a way to start with a stronger synthesis or to split the paragraph. Some ideas: "Evidence for recent relative or absolute uplift are below the rate of geodetic data....", And perhaps a separate paragrpah discussing the geomorphic evidence since that likely developed over ca 10^5+ years, and so might be more relevant for the geological record. There is no need to take my suggestions, but at their heart, I see a mix of time scales, methods, and discussion here that could be better clarified.

We slightly modified the section but did not go toward a major modification as it is what ti is: "X1 says Y1 about Z1, …" from the literature.

Figure 2: I trust that you will have a nicer vector-graphics map set for any final published version.

Best possible resolution vector-graphic figure are provided during the answer.

146 "mu" failed to print properly.

Corrected

Section 3.2: Would it be appropriate to cite Granger et al. (1996) here? Their paper has seemed to me to be an important central one in developing the method that you use.

Indeed, among many. We have added the reference

168. Here you have jumped straight to erosion rate. However, I do not yet know some critical information that you must use to calculate this. This information includes:

*** Rock density**

*** Chosen attenuation length**

*** Are you making an assumption of spatially uniform erosion?**

*** Are your quartz sources well distributed across the watershed?**

I further cannot find this information in Table 1. Therefore, could you please add some significant methodological information to make your study more understandable and reproducible before moving towards your desired results (the erosion rates)? I will review the rest of the paper assuming that you did this work reasonably/correctly.

We added in supplementary material the input data file that we use for the erosion rate computation in the CRONUS-Earth system.

181-183. This is a surprisingly short description of the pebble vs. sand denudation rates. If you are going to make this comparison, I think that you will have to do two things:

*** Note which rate was higher (sand vs. gravel) rather than just noting them to be different**

*** Connect your findings to prior work. I've been thinking of the Andes recently, and a couple papers came to mind / I checked into them -- plus, I used to work with Taylor Schildgen (so use these papers with an active warning about possible nepotism of convenience -- I know her work!):**

- Schildgen et al. (2016, see Fig. 7). On the top of p. 407, they note that these pebbles may have lower 10Be concentrations because they could be produced during mass-wasting events. They provide a set of references that could be relevan

- Tofelde et al. (2018) much more deeply analyzed this effect. I think that this reference is very relevant to your findings, and hesitate only because the "Wickert" in the coauthor spot is me... again, cite if you find useful

We acknowledge that this question is important in order to address the fundamental erosional processes and impact into the TCN methodology. However, we don't think that going more into explanatory details or hypothesis than the sole fact of noting a discrepancy is sounded. Indeed, given the small number of data and given the fact that weighting elementary erosional processes that can lead to ^{10}Be concentration differences is out of the scope of this study (and is a proper field of study), we think that trying to propose a causal link is dubious. Instead, we consider this discrepancy as a stochastic process and that the best erosion is probably closer to the mean of the samples than from any of the single ones in virtue of the regression to the mean effect. This "randomly-treated" results, however, highlight a rather large relative uncertainty in the watershed-scale erosion rates and support that only the order of magnitude is thought to be significant.

187-189. You are so efficient in going to your results that I don't know what you did! Could you please note how you used them to obtain erosion rates? Fortunately, ESurf is a long-format journal.

As the method is a common one for denudation rate estimation, and as we use the CRONUS online calculator, we choose not to explain the details (or equations) of the method. The reader is referred to the references, especially the documentation from CRONUS. However, we provide as a supplementary, the input file that we use in CRONUS.

189. I think that Small et al. (1999) might also be worth a look for high-relief rocky surfaces eroding more slowly than regolith-mantled hills; see the end of their article.

We agree that this observation is a classical one and that the ~ 1 order of magnitude of difference is also commonly found.

Table 1, caption. [comparing to main text] "Pebble" and "cobble" are different grain sizes. Please indicate which these are. Numbers are fine too, to avoid the vagaries of the Udden-Wentworth grain scale.

We modified the text and Table 1 caption by classing the samples as sands or gravel-to-cobbles sediments, following the international ISO 14688-1:2017 scale.

233. "complete" is a bit final. "augment"?

We modified "complete regional" by "regionally-filled"

278. "Fleuve Machne river"... River Manche river? Are you just worried that they might not know French :)?

That is indeed probable! The "Fleuve Manche river" was the therm used by Toucanne et al. (2009, 2010). We changed accordingly

282-284. This is a really sudden jump to, presumably, isostatic impacts of sedimentation. You have not introduced this at all.

Indeed as we think it is a consensual process that any kind of large mass transfer (sediments, ice, etc.) imply a solid-earth response (albeit the details can be discussed). This is slightly more detailed in the new section 5 structure.

290-295. Same as above. I would suggest organizing the article to first provide information on sedimentation rates, then introduce the isostatic processes (presumably), and then discuss deposition and (presumably I'm about to arrive at it) erosion.

We have slightly modified the manuscript structure, notably the section 5, with a 5.1 method relative to the flexural isostasy, the 5.2 effect of sedimentation and 5.3. the effect of erosion.

307. Time scale depends on the rheology at the location (e.g., Iceland, S. Patagonia, are responding to load changes over years to decades.

Sorry, it seems that we do not understand your point here (and for the two following remarks). Indeed, we consider that the time scale is whatever we choose it to be, and given this chosen time-scale the rheology might be different and not the opposite way that the time scale depends on the rheology. As we stay inside a reasonable, time-scale of 10^5 *to* 10^7 *yrs, we assume that the elastic plate approximation is reasonable (e.g. Mazzotti et al., 2023) and rather object of a consensus (e.g. Watts, 2001). We agree that it is questionable for detailed questions or complex settings, both problems that we tried not to go into.*

310-312. There are a few problems with this statement. First, the elastic plate is about the wavelength of the deformation and not about the time scale. Second, I think that most humans would consider the mantle to be quite high viscosity.

As said above, we do not sees your point here, especially relative to the "most humans". The mantle and crustal rheology are indeed time-scale dependent as they are "purely" elastic for seismic waves propagations (seconds) and tend to be, at least for the asthenosphere, purely plastic (fluid) for long-time scale (> 10^7 *yrs). This time-scale modulation being temperature or lithological dependent, etc. the precise characterisation is indeed complex. However, comparison between purely elastic formalism and more complex elasto-visco-plastic rheology do support the assumption of an elastic-plate bending as a good proxy for lithospheric response to erosion. See Mazzotti et al., 2023 for more details.*

312-315. How about the elastic component of mantle rheology? Is this something worth considering, and why? (Your time scales compared to something like the Maxwell time of the mantle might give you an answer.)

As the two remarks above, we are sorry that we might not understand where is precisely leading your question. As we only use the equivalent answer of an elastic plate, the proper localisation of elastic core (if multiples) are of secondary importance. The same rational applies for the ductile behaviour of the mantle. As for the GIA, we assume that the viscosity is significant only for the recent erosion (~ 10 kyrs). For longer time scale, the fluid is considered entirely relaxed therefore having a null impact on the uplift. That is to say, for example, for a 10 m/Ma erosion rate over 5 Ma, only the last 10 krs (unlikely the last 100 kyrs) are dampened by the viscosity, which is ~ 0.1 m over 50 m, less than 0.2 % of un-finite uplift.

317. gFlex is a 2D model: it does not resolve variations in the vertical.

Agree for the model parameters, not for the spatial coordinate axis. Indeed, we use the plan solution which is a 2D-type input (x-y) the bending solution being orthogonal to this plan (z-axis) we don't have a pure 2D solution. We modified "3D" as "2.5D" for clarity

318. gFlex cannot provide velocities since it has no viscous-type rheology component or time dependence. Could you please describe what you have done? My first guess is that you provide some time (when?) to remove the load, or perhaps some range of times (provide a constant erosion rate and see what the spatial pattern of isostatic uplift should be). But you shouldn't leave the reader guessing... or worse, the reviewer!

Shortly, we use the elastic response to a sudden surface perturbation, that is integrative over one Ma, as the total bending over this time-span, hence the bending and derived uplift rate for 1 Ma. As we consider only the long-term erosion rate over hundred of thousands or a few million years, the mantle viscous behaviour that control for instance the GIA response is thought to be negligible, totally relaxed. Truly, the recent erosion, of the order of a few millimetres, should indeed be associated with a viscous behaviour but the resultant error in the long-term uplift is almost null. When compared to the Te evaluation, or the uncertainties relative to the steady erosion extrapolation from 10^5 *to* 10^6 *years, the viscous relaxation seems adding too much unnecessary complexity into the model.*

320. Could you discuss these data sources and how they relate to the rather wide range of Te that you tested?

Theses two works are classical Te estimation from gravimetric-topographic coherence/admitance. The one from Kaban et al., 2018 try to correct for sedimentary series effect. Both are providing the same range of T, which is in the classical one for such a context. Because they do present differences we choose to consider the common range from the two and not to select for a precise "best" Te model.

Figure 6 (or text around): Which boundary conditions and solver methods did you use with gFlex?

The details are added in the new 5.1 (Methods) section.

Figure 6 (itself): How did you choose where to mark your "border effects": I imagine that these should extend uniformly from 1 flexural wavelength or so from the edge of your domain. However, they seem to be irregularly drawn. Relatedly, do you think that the decrease in erosion rates to the N, S, and E outside of the grayed-out area, and possibly the maximum magnitude of uplift predicted, could also be produced by such edge effects? Towards this, I wonder if it might be worthwhile to extend your erosion-rate modeling efforts at least one flexural wavelength inland.

The limits are indeed associated with roughly one flexural wavelength at least. We have delimited them manually and they are not to be taken as a sharp boundary for the model output relevance. For instance, the three different used Te would lead to different no-relevance areas that would have complicated the understanding of the figure. Furthermore, as the proper TE value is unknown, and adding that the use of a purely-elastic, homogeneous and isotropic plate leads to unknown unknowns relative to the precise flexural response, hence for the precise delimitation of a "non-consistency" area. Concerning the possible border effect, we think that our modelled uplift (outside the greyed-areas) are reasonably safe of such problems as they are at least one wavelength away from the un-modelled erosional map. Furthermore, the propagation of modelled erosional maps away from the current limits leads to other problems as the lithology tends to become more and more carbonated (hence the method developed here, being based on quartz-bearing lithologies have be properly checked prior any extension toward carbonated areas).

350. I think that the fact that your random model has a similar shape to the fit model is because of two reasons:

1. Your cells are much smaller than a flexural wavelength, so you will see a mean signal when flexurally filtered. Towards this, I would like to see how you came up with your "50 km" number for "minimum sensitivity length", and if you can define this in a more precise/formal way.

2. Your model outputs are affected by the boundaries, both the sea (where you assume that depositional effects are minimal) and the inland regions (where you might be artificially affecting the domain).

We agree with the fact that the shape is almost similar between the random and the "best" model is due to the smoothing effect due to the small size of the polygons. The c.a. 50 km for minimum sensitivity length is roughly the wavelength of bending response due to point force. We remain vague about this value as it depends explicitly on the used Te (and implicitly on the assumed physical simplifications). The offshore effect is indeed a modelling gap, but as we have tested it separately, we are confident that it reflect the real lithospheric response (at least at first order). As expressed before, the border effect from distant onshore areas are probably negligible for the interpreted area where the cenozoic marine deposits are presents (and used for plausibility control).

Section 6 and onwards: Unfortunately, I am not able to evaluate your Discussion and Conclusions. I do not know how you converted the deflections into uplift rates and think that your model boundary conditions may play a role in setting the pattern that you see. This said, I find your idea of all of the recent uplift to be erosional/isostatc to be quite plausible, and I think that this phase of your data analysis + modeling points in that direction.

Thank your for supporting the plausibility of our proposal. We hope that the prior modifications and text expansion help understanding the different steps we did to obtain our final results.