Dear Dr. Scherler (editor) and Dr. Malcles and co-authors;

Thank you for your patience in receiving my re-review following your revisions and response. In truth, this took me some time to constructively navigate the significant pushback from the authors' comments, which while in itself is not a problem, presented challenges in describing and sharing some of the state of the science. My preference as a referee is to assist authors in developing their work, and this is usually quite a congenial and successful strategy. I write this because some of the below may be quite direct, but I still fundamentally feel compelled to help you in this step.

As the authors and editor will see in the review, significant problems remain. Many of these are the same or similar to those that I noted during the prior round of review. Primary among these is that boundary effects impact the authors' model results: the authors have now stated a flexural wavelength to justify their boundaries, but which is a factor of 5–6 times smaller than the flexural wavelength that I compute for their given elastic thickness, using the tools within gFlex and double-checking the equations against literature. This alone unfortunately makes the work unpublishable: the conclusions are based on model results that are contaminated by the lack of erosion outside of the model domain. In addition, I have persistent but more manageable questions about 10Be sample collection and analysis. Furthermore, the authors still do not describe how they obtain an uplift rate from gFlex (itself a model to provide static solutions), though this time I have chosen to guess that they use it to spatially filter (smooth) the erosion rate into an uplift rate. Though not stated below, information on the assumed densities of material removed may need to be combined with the erosion rates to properly perform this calculation, and it should be described in the article. Finally, the description of the isostatic adjustment model and process is largely incorrect and confuses terms that relate to spatial vs. temporal response. I realize that all of this is quite disappointing to hear after a round of revisions.

I will note comments, responses, and/or line numbers, as appropriate. This is also unusual: typically I scan the authors' responses and primarily look through the revised manuscript. However, here, many of the authors' responses require their own discussion.

Line numbers will be given from the tracked-changes version of the revised manuscript.

Where no comments are given, you may freely assume that I am satisfied with your reasoning and/or revisions.

42. "Due to its peculiar geography, it is only affected by local erosion."
(original 36-38, comment and response)

*Considering that isostasy is one of your main points in the article, being affected only by local erosion is not possible unless elastic thickness equals zero. I do see your statement that the radial drainage pattern moves eroded material away from the massif. Yes, this is convenient for* *setting up a field site with some symmetry. However, this does not change the fact that erosion and/or deposition across the surrounding regions will also unload (or load) the nearby lithosphere.*

*I believe on reading your response that you meant this to be about erosion and material transport.*

*My suggestion here is that you revise this writing to note that the massif is externally drained, with all drainage originating from within the massif.*

46–49 (original 39 comment).

*In your response, you note that large basins may be hard to sample. However, due to the approximately fractal structure of drainage basins, even high-order rivers have 1st & 2nd order tributaries immediately adjacent to them that typically would enable an approach similar to yours. (You may wish to review the literature by Shreve and Rodriguez-Iturbe, as a start.) This property of rivers means that one would not, as you state, require in other field settings to make measurements at watershed heads, which I think we both can agree would not be so useful for catchment-averaged 10Be erosion-rate measurements. Therefore, although you do (positively) take advantage of a benefit of your field site, this does not (negatively) mean that other field sties do not offer this possibility.*

*Regardless of this, because your new text notes that you sample small watersheds (rather than that there is something special about your basins in providing these small watersheds), I think it is mostly okay. However, I would suggest that instead of leaving the reader to guess the meaning of "etc.", you please write the factors that you consider to be important.*

131–132 (original 98)

*Thank you for defining "rasa".*

*I have a bit of advice about your response. You write in response:*
"That is indeed a rather common word in coastal geomorphology. Anyway, we added “(sea-driven planation)” for more clarity for non-geomorphologist readers"

*I in fact have quite a deep background in geomorphology, but work primarily in North America, where we use the term "wave-cut platform". I imagine that you want this to be understandable by an international community. I invite you to be more open and curious about knowledge held by other members of the scientific community.*

From our original exchange,

"My comment: 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.

Your response: Indeed, among many. We have added the reference"

*The Granger et al. (1996) paper established the method of cosmogenic radionuclides for catchment-averaged erosion rates. This, I believe, does not make it "one among many". I am glad that you have added it.*

167–168 / 188–190.

*Please fully list the assumptions that you apply. Giving some subset of them via "e.g." will not be enough for readers who are not already familiar with CRONUS-EARTH. One of the important ones that is not stated is that the sand is supplied by continuous, gradual erosion as opposed to through deep-seated processes such as landslides – as noted in a slightly different context in my earlier comments to the authors.*

182–185.

*Now that the supplementary data are available, which were necessary for me to evaluate your article, I can ask some questions about the outcrop samples. These questions are significant, and run counter to your rebuttal that the methods are quite standard and therefore do not require explanation.*

*First, it is not clear to me that you are able to differentiate between exposure and erosion rate with just these surface samples. Could you explain how you constrained prior exposure history and its potential impacts, or which assumptions you have made? (It seems that you have assumed steady erosion and no prior exposure, and this is important to state as well.)*

*Furthermore, you note that the outcrop samples are likely to provide a lower bound to erosion rates because their erosion is controlled by weathering processes. However, you also assume that denudation rates are at steady state. Combining these two statements implies that the topography is not steady – that is, that relief is increasing. This is not the standard geomorphic assumption, which is one of topographic steady state. Therefore, it may be worthwhile discussing this further. Indeed, by assuming that hilltop outcrops are eroding more slowly than the catchments as a whole, the authors are already assuming that tors may be gradually generated.*

*Third, 10 cm is very thick for an outcrop CRN sample. Typically, one tries to chip off / remove the first ~1–2 cm, and sometimes remove sample from places where deeper rock breaks off.*

*Could you provide the methods of your sample collection and your rationale for taking such a deep sample?*

10Be data in Supplement
(original line 168)

*Thank you for adding the supplement. I have a few and concerns:*

*First: Please reference the supplement in the main text.*

*Second: You use a uniform density of 2650 kg/m$^3$. This implies that the sediments are eroding from bedrock that is pure quartz with no porosity. However, your geological map (Your new Figure 3 lower left) indicates that sandstones, granites, gneisses, and perhaps other rock types supply the sand. Depending on the porosity of the sandstone and the composition of the gneisses, this could mean that the densities range significantly (maybe 2000–2900 kg/m$^3$, depending on porosity and composition) and would vary from catchment to catchment. Correcting for this may adjust your erosion rates by ~20% or so, which is significant. Could you perform this correction?*

222–224
(Original 181–183)

*Citing Carretier et al. (2009) and van Dongen et al. (2019) and noting that you do not consider the question of grain size is helpful.*

*In your response, you note that the grain-size effects are recording stochastic variability. Therefore, your statement that "we consider this discrepancy as a stochastic process" contradicts the references that you cite.*

*I suggest that you simply be straightforward. If you want to include these samples, note that you are including them although there are reasons why they might depart systematically from trends measured in sand. You could perhaps use some of the detailed analyses of van Dongen et al. (2019) as a guide to your reasoning. Clear statements of which data are being included and why are important.*

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*For the purely elastic case, I would remove "Days to years". Pure elasticity is instantaneous, so I would just note that this is the response to instantaneous unloading. This also frees you from potential worries about variable rheology of the mantle. In some places on Earth, the viscous* *response time is short enough that viscous effects would be seen after years (e.g., Dietrich et al., 2010).*

*Dietrich, R., Ivins, E. R., Casassa, G., Lange, H., Wendt, J., & Fritsche, M. (2010). Rapid crustal uplift in Patagonia due to enhanced ice loss. Earth and Planetary Science Letters, 289(1-2), 22-29.*

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*Airy or Pratt isostatic response implies not only sufficient time (relative to the mantle viscous relaxation time), but also significant spatial extent (or a weak enough lithosphere) such that flexural support is negligible.*

356–357.

"For intermediate time scales of thousands of years to million years"

*Two issues here.*

*First: The longest mantle relaxation times (e-folding) on Earth are <10 kyr. Therefore, "millions" is not intermediate. This then brings into question your statement that one uses more complex rheologies over these time scales. Typically, for most of the time range that you indicate, one tries hard not to do extra work and simply assumes a fully relaxed mantle.*

*Second: Flexural isostatic effects, at least as modeled using thin elastic plates, alter the spatial distribution of deformation but not the time scales of that response. Therefore, your sentence is stating that you use a spatial filter to address concerns over a specific time scale. This is not physically sensible.*

*This "second" point reiterates something that the authors note not understanding in my earlier referee comments, "First, the elastic plate is about the wavelength of the deformation and not about the time scale." I hope that this clarifies, considering that the article still confuses spatial and temporal scales.*

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*This follows the discussion in the first round of review:*
Me: "317. gFlex is a 2D model: it does not resolve variations in the vertical."
Authors: "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"

*Before any further discussion, I hold firm that gFlex as it is implemented in the present article is a 2D model, with no room for discussion.*

*I will briefly explain the dimensionality of numerical models and associated conventions. gFlex is a two-dimensional model because it contains information on two spatial dimensions. One of those pieces of information is deflection. Indeed, this can be visualized in a third dimension, as can any distributed parameter. However, this does not make it a three-dimensional model.*

*A three-dimensional model would contain spatial discretization in three dimensions – that is, it would resolve multiple depth increments in addition to the two horizontal dimensions.*

*"2.5D" is used as a shorthand when something is parameterizing a true 3D model into a 2D model. While mathematically imprecise, this term is used and many find it a useful shorthand. This might indeed be appropriately applied to gFlex if the elastic thickness varied spatially. In this case, the Te parameter field would be taking the place of a more complex 3D calculation. However, your implementation has a uniform Te, and so "2.5D" does not apply here.*

*Finally, I am a bit surprised that you have chosen to tell me that I am incorrect about the dimensionality of gFlex, considering that I wrote it. Please consider this if tempted to push back once again.*

378.
*What is "analytic" mode in gFlex? The approach that you noted before ("SAS") is not a pure analytical solution, but rather consists of (numerical) superpositions of (analytical) Green's functions. Please also describe what the boundary conditions are.*

378.
*As a purely elastic model, gFlex produces elastic solutions. It cannot produce velocities. This requires coupling it to a viscous rheological model.*

*This is in fact a shortened version of the same comment that I made before. I reproduce it here alongside the authors' response:*

Me: "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!"

Authors: "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 106 years, the viscous relaxation seems adding too much unnecessary complexity into the model.

*The authors respond that the mantle should be fully relaxed. In this case, if a load is given to gFlex, it cannot provide an uplift rate. It can only provide an absolute amount of deformation. Perhaps the authors have used gFlex to filter assumed constant erosion rates into the distribution of uplift rates – and it seems that this might be the case. (Indeed, providing a change of load per unit time and assuming that this was slow compared to a mantle response time would give an uplift pattern.) However, this is not the "standard" use of gFlex (though I think should work), and they have not updated this text and therefore left their methods unclear.*

*From here forward, I will give the authors the benefit of the doubt and assume that they used gFlex to spatially filter their erosion rates into isostatic uplift rates. I would encourage the authors to actually state their methods.*

383–384.
*I appreciate that your models cover the observed range of elastic thicknesses. When a gradient in elastic thickness exists, however, nonlocal stress transfer can cause elastic responses that differ from a simple comparison of the deformation with uniform elastic parameters. Can you confirm that either the parameters are quite close to being constant or note how your uniform-Te assumption differs from the literature (but you find it appropriate anyway)?*

384.
*GPa is not a unit of rigidity. Might you mean the Young's modulus?*
*Furthermore, do you have a reason to use 100 GPa for Young's modulus? A citation would help guide the reasoning behind this decision.*

387–388.
*The authors state that, "Based on the response pattern of a 25-km-thick elastic plate (Fig. 8), we consider that the predicted uplift rates are valid up to ca. 50 km of the model eastern border"*

*This seems quite short to me for an elastic thickness of 25 km, despite the fact that the authors state in their response to my referee comments that "The limits are indeed associated with roughly one flexural wavelength at least." (More description follows.) Therefore, I have redone the calculation.*

*I use Te = 25 km and the authors' other parameters: mantle density = 3200 kg/m3, assume Young's modulus is 100 GPa, assign 0.25 to Poisson's ratio, and assume that rho_fill = 0 because the field site is subaerial. I consider a point load whose impacts are distributed in 2D; this should give a shorter flexural wavelength than a line load. From this, I compute a flexural wavelength of **288 km**. The authors' statement is false.*

*However, while a full flexural wavelength is an appropriate distance to pad the boundaries, one could obtain only slightly contaminated results using the distance to the first zero crossing. This would be the equivalent in a foreland basin setting of the distance after which no further foredeep subsidence occurs; beyond this, the forebulge would be uplifted. (This is purely for illustration; I realize that the present situation is about uplift close to the erosion rather than subsidence close to the load.) I then compute this and obtain **108 km**.*

*The authors' Figure 8 supports my unfortunate finding that their domain boundaries are impacting their results within the field area. The fact that there is an uplift high atop the massif could be – and likely is – because there are no loading effects assumed outside of the model domain, therefore necessarily placing the uplift high somewhere within, and close to the center of, the model domain.*

*This is quite unfortunate, so I will step back and discuss the concern in general.*

*First, despite my prior request (during the last round of review) that the authors consider flexural effects and the impacts of the lack of loads beyond the model boundaries, the authors have retained the same model domain.*

*To the authors' credit, they have justified it by positing a flexural wavelength (2D) of 50 km. Unfortunately, I cannot find anything supporting the authors' "50" km claim, and found that it significantly underestimats the flexural wavelength.*

*In a later response (to my comment on the pattern of uplift from the "random" erosion rates), the authors elucidate their reasoning:*
"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)."
*This vagueness is not necessary: one could specify a 2D flexural wavelength easily for each of the elastic thicknesses (15, 25, 35 km). Indeed, I include a code utility in gFlex to make these calculations.*

*Unfortunately, this flaw means that, as I indicated in the last review, the authors' boundaries are too close to their study area.*

*Fortunately, many boundaries look to be at least 100 km away from the edges of the map (Fig. 8). Assuming that the map boundaries align with the model domain, the results might marginally work for the 15- and 25-km Te cases. Indeed, in the 15-km Te case, the contours of flexural uplift run essentially parallel to the coast. This then seems sensible, with more distance to integrate erosional isostatic effects with further distance inland.*

*Nonetheless, the remainder of this study, including the conclusions drawn, retain the significant question of boundary effects contaminating the model results. I remain somewhat convinced that the authors could be correct and that observed uplift is due to isostasy. However, despite some improvements in providing methods and considering boundary effects, this paper does not stand on its own in demonstrating this due to incomplete descriptions of data and methods and unfortunately incorrect or unstated descriptions of the model parameters and processes. Furthermore, my comments on this revised draft remain on the same themes as my original ones; this is due to the authors' significant pushback and to their unfortunate large underestimate of flexural wavelength as a function of elastic thickness.*

*At this point, I have skimmed the remainder of the article and think that the authors have a way forward. However, since the model boundary problem will have to be addressed before any meaningful analysis of the model results can take place, I do not think that it would be profitable at this time to make significant comments on this portion of the manuscript.*