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Pr. V. Vanacker
Editor
Earth Surface Dynamics

Dear Pr. Vanacker,

Please find our response to reviewers comments on the manuscript entitled *Hillslope denudation and morphologic response to a rock uplift gradient* (esurf-2019-50), on behalf of myself and co-authors.

We thank all 3 reviewers for their highly valuable and constructive comments on our manuscript. Based on these reviews the main changes are the following.

- We have clarified an important point concerning the response of the fluvial network to the rock uplift gradient : we do observe a response, but the pattern is less resolved than what we obtain with the hillslope-based metrics. A new figure (8) has been added to better highlight this point (correlation between k_{sn} and E^*).
- We have reorganized and expanded the *Methods* section (presentation of the river profile analysis and the modelling approach).
- We have reorganized the structure of the discussion (merging of the previous 5.1 and 5.3 subsections, displacement of some of the 5.3 subsection into the *Methods*).
- The data table have been expanded and reorganized into two different tables.

We will provide an edited version of the article where important additions and modifications are highlighted in red. We have complied with all the minor comments and suggestions relative to improving the text (spelling, formulations, typos) that were kindly provided by the reviewers, so we do not elaborate on the resulting modifications hereinafter, and, for the sake of readability, these small edits are not highlighted in the revised version of the manuscript.

Best regards,

Vincent Godard

Editor (Veerle Vanacker)

Dear authors,

By now, we have received three reviews of your paper. The three reviewers agree on the high quality of the presented work, and they particularly appreciated your integrated work combining high-resolution topographic analyses, CRN data and numerical modelling. The reviewers recommend moderate revisions, and I consider the following issues to be the most critical ones:

(1) While the hillslope analyses are clearly described in the methods' section of the manuscript, this is less so for the stream channel analysis and modelling approach. I refer here to the comments of Reviewer#1, and comments 2 and 3 of Reviewer#2 that will require further elaboration of methodological aspects.
As described in the response to reviewers 1 and 2 a detailed presentation of the stream profile analysis is now provided in the *Methods* section and the presentation of the modelling approach has been expanded and moved to the *Methods* section.

(2) In the paper, you mention that the "hillslope analysis allows to resolve variations in rock uplift, that would not be possible to resolve using stream profile analysis". The three reviewers posted some critical notes here. Reviewer#1 makes the remark that hillslopes might have a longer memory than stream channels because of the differences in response timescale. A related comment is made by Reviewer#2 who points to the possibility of having diachronic response to tectonic perturbations within larger watersheds. Reviewer#3 asks for a discussion of the assumptions of the stream profile analyses that are designed for detachment-limited bedrock streams that are in topographic steady state with respect to rock uplift.
This aspect required indeed some clarification, as our initial presentation was perhaps ambiguous. As explained in our response to the reviewer below our main point is that, for short-wavelength variations in tectonic forcing, the far higher density of information offered by hillslope analysis allows a better sampling of the underlying rock uplift pattern. The change in rock uplift along our transect is actually observable with our k_{sn} measurements on figure 7. We have added a new figure 8, which allows to observed the existence of a significant correlation between fluvial (k_{sn}) and hillslope (E^*) -derived proxies for rock uplift. The text has been edited to remove this initial ambiguity.

(3) Reviewer#3 suggests to clarify that this study mainly addresses "the short-wavelength uplift patterns that are superimposed on the regional pattern". This element can be further elaborated in the discussion. More details are given in the reviewers' comments.
This difference between the short- and long-wavelength deformation patterns has been highlighted in more details at several places in the text.

Please document the modifications in term of content and line numbers.
Looking forward to receive your revised manuscript.

Reviewer #1 Martin D. Hurst

The manuscript published for discussion by Godard et al. (2019) focuses on investigating the topographic signature of hillslopes and their variation with denudation rates in order to identify the spatial distribution of uplift and infer the structural control on this distribution via inverse modelling. The results tally well with a range of independent Geological observations. This is an ambitious and exciting piece of research that with some further clarification should be an excellent contribution to ESURF. I recommend publication subject to addressing the following comments and concerns:

Main comments

Apart from hillslopes that terminate at fault scarps or at the coast, it is channels that set the base level conditions for hillslopes, and therefore hillslopes provide information about the history of channel erosion, not directly about uplift (without assuming or demonstrating a relationship between channels and uplift). The physical link between uplift, channels and hillslopes needs to be more clearly identified and discussed. I think expressing this more clearly throughout the manuscript will be beneficial as it has important implications for your results and interpretations. It seems paradoxical at first that hillslopes can record a denudation signal (and by inference, uplift) that is not also clearly identifiable in channel profiles. However, this serves to highlight that hillslopes may have a longer memory of past landscape development. So, while the channels may harbour little evidence of spatially varying uplift, the hillslope signal could provide evidence that some adjustment to uplift has previously taken place, and the hillslopes are still relaxing (as suggested by the E*R* relationship identified in your paper). Roering et al. (2001) developed an expression for the response timescale of nonlinear diffusion-like hillslopes that suggests response time is much longer for low erosion rates (and by inference, during relaxation), and this tallies with the relatively long decay signal we (Hurst et al., 2013) found at Dragon's Back Pressure Ridge. So I think you can say that hillslopes have the potential to record information about transient landscape dynamics either when channels have had time to adjust but hillslope morphology may still be responding.

Right. We think that there was some ambiguity in the initial presentation of our results. As our focus throughout the manuscript is on hillslope morphology, we probably did not invest enough time in the presentation of the methods and description of the results derived from river profiles morphology (as rightly pointed out by the reviewer in the next comment), and this might yield the misleading impression of a lack of response of these river profiles. Actually, as was briefly mentioned in the initial ms, figure 7D shows that k_{sn} values are higher in the northern part of the transect, but do not show as clear a progressive evolution as the hillslope-derived metrics. The overall message we want to convey is that both systems are responding to spatial changes in rock uplift, but the short wavelength variation, associated with a single structure, which occurs along our transect are much better captured by hillslope metrics, due to the orders of magnitude difference in the density of information they provide. We have made several edits throughout the text to clarify this point (see new figure 8 for example).

The channel profile analysis is not presented with the same degree of rigour as was the case for the hillslope morphology in the methodology. I realise there has been a number of recent studies that have addressed the inference of tectonics from channel profile morphology but I think more explanation is needed relating to the approach to constraining theta and ksn. This is particularly the case as the value of theta chosen may affect whether there is a systematic variation in channel

We have expanded the *Methods* part to provide a better presentation of the fluvial metrics used here. We have also added a new figure with a scatter plot of E^* as a function of k_{sn}, showing that these two metrics are significantly correlated. In line with previous comment, we realize that our presentation of the k_{sn} data was initially ambiguous, we do not claim that k_{sn} does not display any variations along our transect (the northern basins display higher values as initially stated), but that there is an important scatter in the such data, making the analysis of the pattern difficult, whereas the much higher data density distribution of the hillslope metrics yield a much more resolved pattern. We have modified our presentation of the data to make this point more apparent. We have also slightly changed the reference value of theta. While it obviously changed the absolute k_{sn} values, it had no influence on their relative distribution.

We have moved and expanded the description of the approach we use to interpret the E^* pattern into the *Methods* section (*Surface deformation modelling* subsubsection). However, we kept most of the corresponding results in the discussion as moving the outcome of the modeling into the *Result/Data* section does not seems appropriate for the following reasons:
  • the first part of the discussion is devoted to argument in favor of a tectonic origin for the observed pattern and to weigh the merits of various alternative hypotheses. As the most important assumption behind the model is that the surface erosion patterns are driven by differences in tectonic rock-uplift, presenting the modeling results earlier would result in an awkward inversion in the progression of the ms.
  • The *Results* section has been renamed *Data,* in order to better reflect that it is devoted to the objective presentation of the primary observations made during this study (topographic metrics and CRN-derived denudation rates), with no interpretation at this stage. The modelling results clearly do not belong here.

We have restructured the discussion, with a first subsection incorporating subsections 5.1 and 5.3 of the original ms. We also reran the whole MCMC analysis after a slight correction to the definition of our likelihood function, yielding narrower marginal pdfs.

Done

Done

Done

Done

L7-8: folds and thrusts plural
Done

L9-10: CHT and non-dimensional
Done

L10: delete "a" systematic…
Done

L12: allows "us" to propose
Done

L16: allows "us" to resolve
Done

L22: singular "external forcing and tectonic . . ."
Done

L24: highlight that channels set baselevel conditions for hillslopes
Done

L26: unclear what "these two types if forcings" refers to. Restate tectonic and climatic here.
Done

L32: tectonically spelling
Done

L39: Duvall had some co-authors (et al.).
Done

L44: Unclear what you mean by the "planar structure of river network", do you mean planform?
Done

L49-50: delete "rock uplift rates", hillslopes respond to channels
Done

L53: typical lengths of hillslopes, could cite Grieve et al. (Grieve et al., 2016c) heresince they investigated the variability of hillslope lengths in a variety of landscapes (ascited later on).
Done

L58-60:Phrasing is a little unclear, could develop these ideas more logically/progressively.
Done. Sentence reorganized and simplified.

L62: which can be used to infer patterns of uplift
Done

L90: Plateau should be proper noun?
Done

L93: Replace "At last", with "lastly"
Done

L94: "Constrain" should be "constraints", plus comma needed at the end of this line.
Done

L98: Figure should be capitalised? Or at least should be consistent throughout thepaper, please check.
Done

L110: Velensole Conglomeratic Formation all beginning with capitals
Done

L159: Refer to Fig 2 here.
Done

L162: qs has no units (units later provided on diffusion coefficient), need to be consistent.
Done

L167: Derivation and presentation of Eq2 would make more sense if the Exner equation had been presented up front, rather than just referred to in words straight after.
Done

L171: suggest you only need the Roering reference here.
Done

L184-185: Cool! Would be great to make these tools available to the community.
Some of these are almost packaged as Grass modules and we plan to put them on the Grass add-ons page, but we need to simplify the input/output and write proper documentation before.

L188: by "planar" I think you mean "planform"
Done. Changed to contour curvature

L191: why this size of window? Might be worth looking at the (Grieve et al., 2016b)paper on grid resolution here. This paper is in your ref list but can't find cited in text.(Lashermes et al., 2007) were the first to consider this I think.
Done. Citation added and text amended.

L193: Grieve paper year is 2016 not 2015.
Done

L195-197: Less than three lines to describe channel profile analysis does not do it justice. Need to explain in more detail, for example what is Chi and why useful. Howwas concavity determined? Not as much detail as hillslope metrics perhaps neededbut some more info required here.
Done, a dedicated subsubsection in the Methods is now devoted to the presentation of the underlying theory (see reply to main comment above).

L199: "allow US to identify . . ."
Done

L200: suggest deleting "and under a steady-state assumption" since transient landscapes can also be used to interpret rock uplift distribution (though admittedly it's trick-ier).
Done

L202: refer to Fig 2A here
Done

L204: delete "a". Replace "drain" with "channel"
Done

L235: density of soil is 2.5 (g/cm3 presumably?)? Seems high. Did you measure it?
It is indeed higher than typical soil. We did not measure the density, but use this high value, because the regolith is mostly constituted of sandstone and limestone clasts.

L235: were hilltops plunging at all? Might require some adjustment if so.
No, one of the main criteria used when selecting the sampling site was for them to be located along near horizontal ridgelines (information added).

L241: Present the panels in Figure 6 in order A-C or rearrange figure 6 to match the order you want to report in the text.
Done. Insets of the figure reordered.

L243-244: Need reference to Figure 6A here. SC 0.6 needs justification. I would suggest 0.52 is quite a bit lower than values reported elsewhere. Can you look to previous studies for support. If your landscape doesn't contain hillslopes with mean slope approaching SC your estimates will be biased low.
Done. Further justification for the value of S_c added in 4.1 (specific nature of the regolith made of cobble sized clasts with little extra cohesion added by sparse vegetation). Northern most catchments display almost linear hillslopes over more than half of their length.

L246: Fig 7A
Done

L247: Add Fig 7B at end of line
Done

L247-251: refer to figure and panels here
Done

L255: Why was this reference value chosen? What are the mean/median values?
The reference value was changed to 0.25 (average value = 0.24), this is now indicated in the text.

L258-259: Could this (null) result be sensitive to the choice of reference concavity? Would be worth checking.
The initial presentation of the k_{sn} results was awkward, there is indeed a slight increase along the transect (northern values higher), which is blurred by the scatter in the data. We have modified this in the text and also introduced a new figure showing a positive correlation between E* and ksn. The relative values of k_{sn} (higher at the northern end of the transect) were not affected by the change from 0.3 to 0.25.

L265: Fig 9 appears before Fig 8
Done

L279-280: Can you present the data to demonstrate this lack of elevation change?
Done. Average elevation and relief of catchments indicated.

L283: No consideration of any potential variation in vegetation or land use here (and not mentioned in the earlier section introducing the study site).
Done. Information about homogeneous vegetation provided.

L313: replace "eventually" with "likely"
Done

L314: could you colour code these points by distance along the transect?
Done

L317-318: even dropping Sc this low all the average points sit below the steady state curve which only strengthens your claim.
We are not sure the slight deviation from the steady state curve can be considered significant in this case.

L321-322: Again, concern about sensitivity relative to choice of m/n
See response to main comment above. We tested another value of m/n, which did modify the absolute k_{sn} values but not the relative variation.

L331-336: I would suggest that the key difference between the two is that along the Bolinas Ridge we see evidence for active channel transient response suggesting that channel adjustment is ongoing, unlike at your site.
Done.

L380-384: This paragraph is like a flash in the pan but there is a lot of important meat here. I would suggest that the modelling approach needs to be explained in full in the methods section and results presented in the results section, prior to discussion. What is MCMC acronym not defined? Which four parameters are free parameters? How was the ensemble set up to ensure full exploration of the parameter space? (flexibility of Markov Chain and acceptance rates). How quickly did the model converge on a solution? The results of this work are interesting and novel and deserve a more thorough treatment, which will require some restructuring.
Done, see reply to main comment above. A specific subsection in the *Methods* is now dedicated to the presentation of the approach.

L404-410: Repetition from intro.
Right, but this is one of the key idea (combination of high-res morphological analysis with geochronology data) that we want to put forward in this ms, and we feel it is important to come back to it here.

L410: Acronym DTM has already been defined.
DEM? Done

L424-425: (Grieve et al., 2016a) is perhaps an important reference here
There is no explicit comparison of CRN data with topographic metrics in this paper?

L431: What evidence is there for human disturbance on the hilltops? Might this also have implications for hilltop curvature?

Done, information added to the main text. There were small cobble-made walls close to the sampling site, and in some part shallow deflection of the surface, which were possibly due to excavation (and thus exhumation of lower concentration material). the typical wavelength was <1m and they are unlikely to affect the measurements of curvature done here.

Done. Vegetation cover is homogeneous along the studied transect, this is now indicated in the first part of the discussion.

That's what we did, but we are using the differential uplift of 20 mm/ka between the S and N parts of the transect as an estimate of the vertical displacement across the fault (corresponding figure modified for clarity). Converting into slip rates give 23 mm/yr, given the large uncertainties at this stage of the calculation, we report that as ~20 mm/ka in the text. The text has been edited for clarification.

Vegetation cover is now mentioned in the discussion. We only have access to the DSM built from last returns (IGN RGE national grid), and no way to assess vegetation height from these data.

Done

The axis of the ridge (where the sampling site is located) is actually slightly off to the left (and on the right on panel D). We tried as much as possible to select such configuration for our sampling sites. The small paths that sometimes follow the hilltops did not show any sign of significant excavation or mobilization of sediments.

We stopped the flowlines at the edge of the floodplain (information added to methods section)

Done

Done. A new subsusbection in methods is dedicated to the presentation of the river profiles analysis.

Done. The presentation of the results has been organized into 2 tables for the sake of readability. Most of the technical information, initially presented in the text, has been moved to the tables captions for easier reference.

Reviewer #2 Marta Della Seta

In the manuscript entitled " Hillslope denudation and morphologic response across a rock uplift gradient" Godard et al. present the results of a very interesting methodological test focused on high resolution analysis of hillslope morphology as proxy for uplift gradients. The analyses are supported by independent geological and geochronological data and the results of this research undoubtedly contribute to outline the potential of the very dense information associated to hillslope morphometry from high resolution DTMs to record tectonic rates. The manuscript is well-written, overall well-structured and I recommend publishing it in Earth Surface Dynamics, after minor revisions, according to the following general and line-by-line comments:

GENERAL COMMENTS:
1. The interpretation of the factors controlling hillslope morphology (I suggest using "morphometry") is certainly supported by strong independent geological constraints on deep structures responsible to variable surface uplift along the transect used for the analyses. Nonetheless, this part of the discussions should clarify better the complete set of factors controlling the hillslope morphometry. For example, despite the climatic and lithological homogeneity, could the catchment size/shape play a role in the results obtained? Given that I'm convinced that high resolution DTMs are incredible sources of morphometric data to be interpreted in the light of the theoretical landscape evolution (Vergari et al., 2019, DOI: 10.1002/esp.4496), the high resolution itself imply that your DTM records very local surface features: despite the binning, how can you exclude that the results of your morphometric analyses along the transect are affected by the occurrence of local processes related to the hillslope-channel dynamics?

We do not observe any systematic trends or differences related to catchment size in the various plots, with smaller or larger catchments behaving as distinct groups. See for example the new figure 8 where symbol size is a function of catchment size (information about catchment area has been added to the caption of the figure).

In most case, the calculations operated on the high resolution DTM are done by first fitting a quadratic surface over a circular neighborhood and then performing the operation of interest on this surface, as for example calculating the hilltop curvature (see Hurst et al., 2012 for example) (see Methods sections). In this case the size of the calculation window is 30 m, which has the effect of filtering the very local features mentioned.

2. Section 5.3 shows new data coming from the application of a dislocation model to predict surface deformations associated to tectonic structures. Therefore, I suggest moving it to the results, after having better explained the method you used.

Following a similar comment by reviewer #1 we have reorganized the presentation of this modeling aspect of the study, with a dedicated part in the *Methods*. However, for reasons developed above, we strictly restrict the *Data* section (previously *Results*) to the factual presentation of primary observations acquired in this study (topographic analysis and cosmogenic nuclides) and keep the modeling results in the *Discussion*, as these models are intended to support the interpretation and discussion of the data.

3. If I did not misunderstand, your inferences about the lacking record of the uplift gradient in the river long profiles is based on the analysis of all the catchments, so why do you show the profiles and CHI-plot just for a single catchment (fig. 5)? In my opinion this part should be better presented in order to strengthen the implications of your results, which concern the transient conditions recorded in the hillslope morphometry when the main trunks and tributary channels already adjusted to tectonic

Following similar comment by reviewer #1 we have reformulated the presentation of the results pertaining to river profile analysis. As stated above we have added a new figure showing that E^* and k_{sn} are significantly correlated. As explained above, our initial presentation of the k_{sn} data was ambiguous, as k_{sn} display variations along our transect (the northern basins display higher values as initially stated), but that there is an important scatter in the such data, making the analysis of the pattern difficult, whereas the much higher data density distribution of the hillslope metrics yield a much more resolved pattern. Citation to Demoulin (2012) added to the introduction.

Right, but such remark could be made for any type of geomorphological investigation over large areas. We have edited the discussion to better underline that this is a specificity of the studied area.

The mentioned figures have been edited for better readability.

We kept _morphologic_ in the title, because _morphometric_ was, in our sense, specifically referring to the measurement of the properties and was narrowing down the meaning much more than what we want to convey, whereas _morphologic_ has a broader sense, referring to the general structure, evolution and behavior of the hillslopes.

No, the conglomeratic formation is the bedrock currently undergoing active erosion and providing the regolith mantling the hillslope

Done

There is no explicit references to chi calculations here, but we have added more references in the methods.

Done

Done

Done

All our field observations show the existence of this mobile regolith cover on the hillslopes, such as the main limiting factor for hillslope evolution is the capacity to transport this material. This is now highlighted in the text.

Done

Done

Very small tributaries are not expected to collapse perfectly on the linear trend in chi-z space, due to change in dominant incision processes (increasing colluvial contribution for small channels), and some amount of dispersion around the main trend is usually observed. The main information here is the very good linearization of the main trunk profile. This information was added to the presentation of the data.

Done (sentence actually modified as a response to rev #1 comment)

Done

Done (the range was actually excluding sample P)

Done (lower uncertainty)

Done. Average elevation and relief of catchments indicated.

Done. sentence modified for clarity

E* is increasing from ~6 to its max value close to 10 in ~4 km, with a progressive evolution across this range, so we do not consider this change to be abrupt such as the one associated with a sudden offset. *Gradual* changed to *progressive.*

From the observation of Figure 1 of Hurst et al. (2013), it does not appear that L_H is varying as much as C_{HT} along their transect, and does not seem to follow the major

increase and then decrease in C_{HT} either. In Hurst et al. (2013) most of the studied hilltops are also oriented perpendicular to the transect.

L356: does not depend
Done

L375: you'd better discuss the assumption of a single planar dislocation in an elastic medium used in tour modelling
Following similar comment by reviewer #1, all the presentation of the modelling approach has been moved to the Methods section (3.3), where the underlying assumption are now presented in more details.

L393: formations underwent long-wavelength
Done

L413: . . .providing the framework
Done

L431: did you evaluate the possibility of other disturbances in your catchments before performing CRN analyses?
All the other sites were distinctively free of significant close disturbances. A benefit of sampling at hilltop sites instead at collecting sediments at outlet sites, following the widely used approach to derive Catchment-Wide Denudation Rates (CWDR) is that we are not concerned about the usual problems arising at the catchment scale (representativity of stochastic inputs, spatial variability of source material, variability of quartz content at the scale of the catchment, etc …). Another benefit is that it allows to make a direct association between the denudation rate at the sampling site and the morphological properties measured in its direct neighborhood (instead of relying on spatially averaged metrics in the case of CWDR).

L478: maybe comparison instead of confrontation
Done

Reviewer #3 Peter van der Beek

Godard et al. exploit a high-resolution Lidar-derived digital terrain model to explore the hillslope response to a rock-uplift gradient in a well constrained, climatically and lithologically homogeneous setting: the Valensole Plateau in SE France. In particular, they show that systematic variations in ridge curvature and a derived no-dimensional erosion rate can be linked to subtle tectonic uplift of the northern part of the plateau, which they proceed to model with an elastic-dislocation model as resulting from compressional reactivation of a pre-existing normal fault in the basement underlying the plateau. Finally, they use cosmogenic 10Be and 26Al measurements of ridge erosion rates to dimensionalise the analysis and provide estimates of the rates of shortening, uplift and erosion of the study area.

This manuscript will make an excellent contribution to ESurf. It shows how high-resolution DTM data can be used in conjunction with cosmogenic nuclide data to provide detailed inferences about landscape response to variable uplift rates and shows a way forward in morphotectonic analysis. The analysis is very complete, as the authors assess both hill-slope and channel response, include cosmogenic nuclide data to obtain actual rates, and interpret the results in terms of a tectonic driver using a numerical model inversion. It is therefore timely and of broad interest to the ESurf readership. It is also (mostly) well written and illustrated. I therefore recommend this be accepted pending minor/moderate revision.

Specific comments:
There are a few issues the authors could address in more detail in a revision:

First, the uplift gradient discussed in the manuscript is superimposed on a longer-wavelength uplift gradient that is clearly recorded in both present-day vertical motions as recorded by permanent GPS stations (e.g. Noquet et al., 2016), and deformed geomorphic surfaces such as the Valensole plateau and the alluvial terraces of the Durance River (e.g. Champagnac et al., 2008). While this is noted in passing in the manuscript, the tilting of the Valensole plateau would lead to increasing uplift/erosion rates toward the northeast even in the absence of an active fault. It could be made clearer in the manuscript that the authors are investigating a shorter-wavelength uplift pattern superimposed on the regional pattern. In keeping with this, if the interpretation of the authors is correct, the Durance river terraces, acting as passive markers, should also record the fault offset. Whereas the long-wavelength tilting is recorded by these terraces (e.g. Champagnac et al., 2008 and references therein) it is not clear whether the faulting is. The terraces are reported to be up to 1 Ma old, i.e. up to about half the age of the surface of the Valensole plateau, and should therefore record up to half the offset of the surface (estimated at ~30 m from Figs. 2 and 10), largely sufficient to be recorded by the high-resolution DTM. This aspect could be looked at, and the implications of the surface geometry of the terraces discussed, in some more detail.

Done. The differences between the short-wavelength and long-wavelength analysis are now underlined more clearly in the *Settings* and *Methods* sections.

The high resolution DTM inspection shows that actually the morphological expression of these Higher Terraces of the Durance (in particular HT2 from Dubar) as flat gently-dipping and low concavity surfaces is very limited due to the erosion of the plateau margin, and they are only present as very small discontinuous surfaces on some hilltops (which we excluded from our analysis), which are much smaller than what is mapped in Dubar (1984) or on the BRGM geological map (Fv). In particular these remnants are virtually absent from the northern part of the transect, making the detection of the finite deformation as recorded by the upper surface impossible.

Along the Durance River, only the younger "Riss and Wurm" terrasses show preserved flat top surfaces. On its profiles, Dubar (1984) considers alluvial deposits of the "High Terraces" that do not show preserved terrace surfaces. The IGN 1m DEM clearly shows that, apart from localized and discontinuous remnants, there are no extensive flat top surfaces preserved in the studied area. Therefore, the HT1, HT2, HT3 and HT4 terraces of Dubar 1984, which correspond to the Fv and Fw formations on the BRGM geological map, cannot be used for accurate morphological analyses. The only surface that we could confidently use in this study is the Fu top surface of the plateau (which is plotted on figures 2 and 11). This has been indicated in the *Settings* section.

The analysis of the river profiles implicitly assumes they behave as detachment-limited bedrock streams and that they have reached steady state with respect to uplift. Neither assumption is immediately obvious: given that these streams incise relatively non-consolidated coarse conglomerates, they could very well behave as transport-limited streams; and given the probably recent onset of faulting below the Valensole plateau, they may not have equilibrated to the uplift regime yet. Therefore, the fact that the stream-profile analysis led to less clear constraints than the hillslope analysis could be due to inadequate theory as much as insufficient resolution. Again, this aspect could be discussed in some more detail.

Done. The question of transport-limited conditions is now addressed in the discussion (5.2). We note that a prediction of such model would be some degree of sensitivity of concavity to rock uplift (e.g. Wickert & Schildgen, 2019) which is not observed with our dataset. Concerning a possible transient response, all rivers and tributaries display regular concave-up profiles, which collapse on single linear trends in chi-z space and so no evidence for such adjustment. At last, while the conglomerate weather progressively on hillsopes producing a continuous regolith cover, the initial un-weathered bedrock is highly resistant, as exemplified by the famous steep cliffs near Les Mées village, directly north of the studied transect. The corresponding information has been added to the *Settings*.

The discussion regarding whether the hill slopes have reached steady state (Fig 8) is not very clear. Some explanation of why R* and E* should vary as indicated in Fig. 8 in case of steady state appears to be lacking.

Done. Explicit reference to equation 8 is now made in the text and the caption of the corresponding figure.

Finally, the procedure to extrapolate locally measured erosion rates (Fig. 11) appears a little convoluted. I understand the authors' rationale for doing this, as it is not obvious in how much the measured erosion rates are representative, but the resulting pattern could be perceived as being somewhat removed from the actual data. A potential solution to this could be to simply plot the measured erosion rates at the corresponding locations in Fig. 11, so that it is easy to assess in how much the followed procedure modifies these measured rates.

Done. The corresponding figure has been modified accordingly. The overall increasing trend is preserved with the at-site denudation rates, with some scatter corresponding probably to short-wavelength variability in denudation rates. As indicated in the figure caption, these rates directly measured at the samples sites are systematically higher than those recalculated from spatially averaged curvature values. This is a direct consequence of a sampling bias toward high curvature ridges, which provided better conditions in the field. Assuming that equation 3 holds over the observed range of curvature (which should be the case if transport-limited conditions are maintained, as we observed at these sites) this should have no incidence on the calibration of the diffusion coefficient and hence on the computed continuous denudation transect (now in orange on the figure).

Although the writing is overall of good quality, a number of minor English-language issues distract somewhat from the science. These include frequent singular/plural confusions, illogical comma use and awkward sentence structures (placement of adjectives, etc.). I am returning an annotated manuscript directly to the authors so they can correct these minor issues.
Done

Figures are mostly clear and well drafted. I would suggest use of another colour than yellow for symbols or profile lines, as I found these very hard to read (e.g. in Fig. 2B, 8, 10A and B). Figure 2 A and C need a (larger) N-arrow to indicate they are rotated nearly 90 ∘ ; labelling the Durance River would also help.
Done