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Interactive comment on "Hillslope denudation and morphologic response across a rock uplift gradient" *by* Vincent Godard et al.

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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:

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Discussion paper

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?

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.

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 perturbations (in case of low tectonic rates/relatively old tectonic input). I suggest you refer also to the papers by Demoulin (2011 doi:10.1016/j.geomorph.2010.10.033, 2012 doi:10.1029/2012GL052201), who based his landscape metrics on the diachronic response to tectonic perturbations by main trunks, tributaries and hillslopes within catchments.

4. In the discussions you state that “In the absence of any climatic, litho- logic or vegetation gradient, the observed increase in hilltop curvature, hillslope relief and normalized erosion rate points to a coincident increase in rock uplift”. This seems to be a weakness of the method, since often such a homogeneity of climatic, lithologic and vegetation cover factors is lacking, especially when dealing with areas with regional extent. Maybe you’d better discuss this point before the conclusions.

5. Some figure are too small to be readable (Figs 1, 6, 11).

LINE-BY-LINE COMMENTS:

Title: I suggest changing “morphologic” with “morphometric”

L13: maybe you mean “eroded” conglomerate

L26: types of forcing

L42-45: references for CHI maybe deserve to be cited

L70: correct “hilltope” in “hilltop”

L129: Myr, not M.yr. Moreover, check the uppercase for East and West (uppercase not used in other sentences, please homogeneize)

L131: Lambruissier not readable in Fig. 1

L151-153: The reason why the hillslopes can be considered as regolith-mantled and transport-limited systems is not so clear to me.

L165: I’m not sure that “shallow” is the right adjective for slope.

L246: South to North, maybe?

L252-254: What stated is not so clear in Fig. 5: first of all because here are reported data only from a single catchment; secondly, not all the profiles shown have a concave-up shape (linear CHI-transformed shape).

L256: South to North, maybe?

L265: Figure 9 is cited before Figure 8, maybe better inverting their numbering.

L265-266: The maximum denudation rate obtained from 26Al as declared in the text does not fit with the data plotted in Fig. 9B (here, sample P seems showing maximum denudation rate obtained from 26Al >88 mm/ka).

L270: You should motivate the choice of considering only 10Be.

L279: what do you mean with "important" when referring to range in elevation of the catchments from South to North?

L307: Maybe "The uplift itself is due to a long. . ."

L324: E* and CHT do not show a gradual increase as stated, rather an abrupt increase.

L341-341: maybe the lack of a correlation between LH and CHT differently from Hurst et al. 2013 could depend on the fact that in the submitted manuscript the metrics are measures along a narrow profile transverse to divides.

L356: does not depend

L375: you'd better discuss the assumption of a single planar dislocation in an elastic medium used in tour modelling

L393: formations underwent long-wavelength

L413: . . .providing the framework

L431: did you evaluate the possibility of other disturbances in your catchments before performing CRN analyses?

L478: maybe comparison instead of confrontation

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