Earth Surf. Dynam. Discuss.,
https://doi.org/10.5194/esurf-2019-50-RC1, 2019

Earth Surface Dynamics
Discussions
Open Access

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

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Received and published: 25 November 2019

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

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Discussion paper

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.

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 steepness. Could you directly compare channel and hillslope metrics on a catchment by catchment basis to show the presence or absence of a relationship?

The modelling approach used to identify structural control on rock uplift is poorly presented (despite a nice figure). Seems like an after-thought buried in the discussion but to my mind is one of the key novelties of the paper. I recommend that the modelling should be described in much more detail in the methods section and results presented and then discussed appropriately. This will require some restructuring of the paper but I think will lead to a more robust presentation.

Line-by-line comments and editorial corrections/suggestions

Title: suggest “response to a rock uplift gradient”

L2: replace “as” with “since”

L3: channels set baselevel for hillslopes, uplift may set baselevel for channels.

L6: cosmogenic nuclide-derived

L7-8: folds and thrusts plural

L9-10: CHT and non-dimensional

L10: delete “a” systematic. . .

L12: allows “us” to propose

L16: allows “us” to resolve

L22: singular “external forcing and tectonic. . .”

L24: highlight that channels set baselevel conditions for hillslopes

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

L32: tectonically spelling

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

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

L49-50: delete “rock uplift rates”, hillslopes respond to channels

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

L58-60: Phrasing is a little unclear, could develop these ideas more logically/progressively.

L62: which can be used to infer patterns of uplift

L90: Plateau should be proper noun?

L93: Replace “At last”, with “lastly”

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

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

L110: Velensole Conglomeratic Formation all beginning with capitals

L159: Refer to Fig 2 here.

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

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.

L171: suggest you only need the Roering reference here.

L184-185: Cool! Would be great to make these tools available to the community.

L188: by “planar” I think you mean “planform”

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.

L193: Grieve paper year is 2016 not 2015.

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. How was concavity determined? Not as much detail as hillslope metrics perhaps needed but some more info required here.

L199: "allow US to identify. . ."

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 trickier).

L202: refer to Fig 2A here

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

L235: density of soil is 2.5 (g/cm3 presumably?)? Seems high. Did you measure it?

L235: were hilltops plunging at all? Might require some adjustment if so.

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.

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.

L246: Fig 7A

L247: Add Fig 7B at end of line

L247-251: refer to figure and panels here

L255: Why was this reference value chosen? What are the mean/median values?

L258-259: Could this (null) result be sensitive to the choice of reference concavity? Would be worth checking.

L265: Fig 9 appears before Fig 8

L279-280: Can you present the data to demonstrate this lack of elevation change?

L283: No consideration of any potential variation in vegetation or landuse here (and not mentioned in the earlier section introducing the study site).

L313: replace “eventually” with “likely”

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

L317-318: even dropping Sc this low all the average points sit below the steady state curve which only strengthens your claim.

L321-322: Again, concern about sensitivity relative to choice of m/n

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.

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.

L404-410: Repetition from intro.

L410: Acronym DTM has already been defined.

L424-425: (Grieve et al., 2016a) is perhaps an important reference here

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

L435: What about vegetation as a control on D? Any spatial variation in vegetation?

L450: It seems to me that the absolute throw rate on a fault dipping at ∼60o should be larger than the vertical uplift (inferred from denudation) component, not the other way around. An uplift rate of 50mm/ka would imply a throw rate of ∼57mm/ka

L464: No discussion of vegetation gradient. Can you get above ground biomass from the LiDAR?

Figure 2: Panel A has no N arrow, and north arrow on panel B is hard to see.

Figure 3: Panel C might suggest some human influence on hilltops (humans have a habit of putting paths along hilltops).

Figure 4B: Cannot see flow lines above flood plain

Figure 6: reorder panels as per comments above

Figure 7: subscript on Ksn axis label. I would suggest sticking to either theta OR m/n, not both. Discussion of n values later would suggest the need for the latter. Stream power incision model has not been presented clearly.

Table 1 and 2: suggest following reporting recommendations of (Frankel, 2010).

References referred to in review:

Frankel, K. L.: Terrestrial Cosmogenic Nuclide Geochronology Data Reporting Standards Needed, Eos, Trans. Am. Geophys. Union, 91(4), 31–32, doi:10.1029/2010EO040003, 2010.

Grieve, S. W. D., Mudd, S. M., Hurst, M. D. and Milodowski, D. T.: A nondimensional framework for exploring the relief structure of landscapes, Earth Surf. Dyn., 4(2), doi:10.5194/esurf-4-309-2016, 2016a.

Grieve, S. W. D., Mudd, S. M., Milodowski, D. T., Clubb, F. J. and Furbish, D. J.: How does grid-resolution modulate the topographic expression of geomorphic processes?, Earth Surf. Dyn., 4(3), 627–653, doi:10.5194/esurf-4-627-2016, 2016b.

Grieve, S. W. D., Mudd, S. M. and Hurst, M. D.: How long is a hillslope?, Earth Surf. Process. Landforms, doi:10.1002/esp.3884, 2016c.

Hurst, M. D., Mudd, S. M., Attal, M. and Hilley, G.: Hillslopes Record the Growth and Decay of Landscapes, Science (80-.)., 341(6148), 868–871, doi:10.1126/science.1241791, 2013.

Lashermes, B., Foufoula-Georgiou, E. and Dietrich, W. E.: Channel network extraction from high resolution topography using wavelets, Geophys. Res. Lett., 34(23), doi:10.1029/2007GL031140, 2007.

Roering, J. J., Kirchner, J. W. and Dietrich, W. E.: Hillslope evolution by nonlinear, slope-dependent transport: Steady state morphology and equilibrium adjustment timescales, J. Geophys. Res., 106(B11), 26787, doi:10.1029/2001JB900018, 2001.

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