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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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General comments:

Godard et al. exploit a high-resolution Lidar-derived digital terrain model to explore the hill-slope 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 non-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

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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.

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 hill-slope analysis could be due to inadequate theory as much as insufficient resolution. Again, this aspect could be discussed in some more detail.

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.

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.

Technical corrections:

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.

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.

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