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Earth Surface Dynamics Discussions

Interactive comment on "Patterns of landscape form in the upper Rhône basin, Central Swiss Alps, predominantly show lithologic controls despite multiple glaciations and variations in rock uplift rates" *by* L. A. Stutenbecker et al.

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1. General comments

This study argues for the role of lithology (a proxy for rock mass strength and therefore erodibility) in governing patterns of erosion and shaping landscapes of the upper Rhône basin, central Alps. The study area is divided into three 'litho-tectonic' units: External Massifs, Penninic Nappes, and Helvetic Nappes, and then standard spatial analyses are applied to examine precipitation patterns, river long profiles, catchment

hypsometry, and hillslope and valley morphometry. In essence, long profile convexity (among other indices) is interpreted as evidence of landscape transience and such transience is most clearly expressed in the External Massifs, comprising crystalline basement rocks of presumably low erodibility. By contrast, the presumably weaker limestones making up the Helvetic Nappes are observed to have the most concave profiles. It is argued that other measures, such as precipitation patterns, glacial inheritance and rock uplift patterns fail to explain the observed topography.

In my view the study approach could gain from some restructuring in favour of more explicit hypothesis testing. As it currently stands the results are presented and then the focus on lithology emerges in an overly inductive fashion. I suggest the authors explicitly state a series of research questions from the outset. The authors state that the perturbations driving landscape transience is either of tectonic or glacial origin; however, I find it hard to accept that it is not possible to separate the two controls - at least in a few cases that could be explored in some detail. Further, given that 'response/relaxation time' to perturbation is so central to this study, it seems appropriate that some effort were made to discuss the actual mechanisms, such as knickpoint retreat, which determine the transient evolution of the landscapes over time.

Substrate controls on landscape morphology have probably received less overall attention than climate and tectonic drivers because erodibility is difficult to quantify and its relationship to lithology is not straightforward. This MS does not discuss or explore this issue; rather it is simply asserted that the three-way subdivision is a reliable proxy for substrate erodibility. This might indeed be the case but in my view it should be somehow demonstrated. Furthermore, the existence of sub-catchment scale lithological influences on long profiles and hypsometry is not considered. Previous studies have presented detailed analyses of such controls very effectively using simple morphometric indices (e.g. Duvall et al. 2004). On this note, the reliance upon qualitative summaries of the results is a major shortfall. Interpretations would be better demonstrated via statistically significant splits among the morphometric indices from the three

'litho-tectonic' units. The case is not helped by the incomplete presentation of results: 55 catchments were analysed but only some, possibly the best-behaved examples (Figs. 7, 8), are presented. I suggest the authors make full use of the Supplementary section in order to fully disclose their tabulated and plotted data, while also keeping the MS uncluttered. This will provide a rich contribution for others to follow and is consistent with ESurf's spacious online capacity.

Lithological control on steep landscapes is an issue that workers beyond the Alps have also considered and I suggest the authors acknowledge more studies from orogenic and postorogenic terrain elsewhere. The reference list is on the thin side and in need of bolstering, especially in light of several important studies on the role played by differential erodibility in mountain terrain and how that might 1) modify rates of knickpoint retreat and therefore communication of new base level information; 2) influence the activity of landsliding from steep hillslopes; and 3) affect sediment flux exiting the orogen. Some important omissions that spring to mind are Molnar et al. (2007, JGR); Korup (2008, GRL); Korup Weidinger (2011, GSL); or even Scharf et al. (2013, Geol.) - okay, I admit this is one of mine!

Finally, perhaps the title could be rejigged to something like: "Lithological controls preserve landscape transience in the upper Rhône, central Alps".

2. Specific comments [page:line]

[1062:11] 'Lithological architecture' is not examined in this study; I suggest replacing this phrase with 'lithology'.

[1062:21] Specify which perturbations are being referred to here: tectonic, glacial or both? And is it possible that some perturbations have their origins outside a given litho-tectonic unit?

[1063:7-10] 'Threshold topography' is a rather theoretical concept that has undergone some development over the last 20 y, as has 'topographic steady state'. It would

be useful to clarify the meaning of both these terms especially with regards to hillslope morphology and the mechanisms at play. This explanation should link forward to [1069:13].

[1063:11-13] This so-called 'coupling' between climate and denudation is complicated to the extent that one might question whether it really exists in any direct way. Perhaps refining the example would help: e.g. 'increased orographic precipitation can drive higher river discharges that in turn tend to enhance rates of fluvial channel incision'.

[1063:22] This binary-style of argument (climate vs tectonics) is not very useful and we all should be striving to get beyond it. Climate and tectonics encompass a whole suite of mechanisms that operate over different temporal and spatial scales. Writing in these overly general terms doesn't add much.

[1063:24-27] The point that mineral cooling ages record periods of accelerated uplift, which coincide with higher sediment flux into the foreland is completely circular. How is this an argument for tectonic control on denudation? The cooling history might be equally well explained by the onset of a wetter or colder or stormier climate. Please rephrase this point.

[1064:8] It is fair to say that lithology (or more correctly, rock mass strength as it affects erodibility) has received too little attention. It might be useful to state why this is ... probably because substrate erodibility is a difficult property to quantify and it has a complicated relationship to lithology. I suggest that the authors acquaint themselves with the key early work: Hack 1957, USGS-294-B.

[1067:3] Seismic activity recorded over what time interval, 2001-08 as shown in Fig. 2? Not a big sample, is it?

[1067:22-24] Far more important that the current spatial distribution of ice cover are the differences that have occurred over the last several glacial cycles; i.e. the timescale over which the valley long profiles and general landscape has been shaped. What can

be said about how the proportion of ice cover has varied between the litho-tectonic units over this time scale? As discussed later [1078:14] the LGM glaciation engulfed the entire Rhône basin with thick ice in every tributary.

[1068:15-16] Please describe 'annual 90

[1069:5] 'Channel reorganisation' means?

[1069:7-9] This is not strictly correct and depends upon the spatial pattern of erosion. Please clarify this statement and give some reference of support. Change 'adapt' to 'adopt'.

[1069:13] Hillslope adjustment is central to the notion of 'topographic steady state' and should really be acknowledged here (cf. note above).

[1069:17] Such Davisian terms as 'maturity' and 'youth' do not serve any useful function in modern quantitative geomorphology. It was fine for Strahler, he was still operating in a largely Davisian paradigm (pre-Hack), but not today. Replace 'progressing' with 'progressive'.

[1069:19-23] It's clear that glacial erosion might be focused at specific elevations reflected in hypsometry, but not so concerning tectonic or climatic controls. Please rephrase.

Since Strahler there have been several important studies of hypsometry that are not acknowledged here. Given that hypsometry is rather central to this study I suggest the authors consider studies that have examined, for instance, the importance of catchment shape on the H curve, the scale-dependence of HI in transient vs steady state settings, the effects of lithology etc., as discussed in Willgoose Hancock (1998, ESPL), Hurtrez et al. (1999, ESPL), and Cheng et al. 2012, Geomorphology).

[1069:1] What does 'topographic state of a catchment' mean?

[1069:3-8] 'Lithological controls' occurs in the MS title, yet there seems to be no explicit

analysis of the well known influence of lithology on hypsometry. I suggest the authors reframe this oblique approach in favour of a study setup that tests directly the influence of lithology on topography, landscape response etc.

[1069:11] How are these topographic properties actually measured and what are the measurement uncertainties?

[1070:21-22] Again, what threshold mechanism is being invoked here: internal friction within the hillslope? If so, how does a hillslope develop beyond its threshold? An oversteepened slope means? Overhang? The use of these terms has developed somewhat since Burbank et al. (1996) and it would be useful for the authors to reflect on these developments (see for instance Korup Weidinger 2011, GSL).

[1070:24] The idea of rates of denudation exceeding rock uplift in an orogenic setting is an interesting one. How would this happen exactly and at what scale? Replace 'progressing' with 'progressive'.

[1071:2] The preceding text implies a connection between threshold slopes and rock mass strength, but here lithology seems to be standing in as a proxy for the latter. What is the relationship between lithology and rock mass strength in the study area and how has this relationship been determined beyond simple qualitative generalisations?

[1071:11] How is valley width measured and what are the uncertainties involved? Uncertainties associated with the topographic analyses seem to have been ignored. Are they negligible? The relevant assumptions folded within the ArcGIS and TopoToolbox-driven analyses could be presented in brief supplementary note.

[1072:5-8] The algorithm used to interpolate the precipitation data into a grid-based dataset presumably involves a strong topographic component. I suggest some comment on how this might affect their analysis of orographic precipitation patterns.

[1072:9] Precipitation-driven erosion processes sound like rain-splash to me, whereas presumably fluvial erosion is meant. Fig. 3 needs to needs to be enlarged.

[1072:14-21] Some consideration of measurement uncertainties would be welcome here and is necessary in order to make such interpretations.

[1072:18-21] I suggest rephrasing this last sentence.

[1072:23] It's not clear what is meant here by 'external perturbations', but would lithology also be expected to play a role in determining long profile shapes etc? Cf. my earlier comment concerning testing the role of lithology more explicitly.

[1073:1-2] These qualitative descriptions and by-eye assessments are needlessly imprecise when there are simple methods available to quantify profile concavity, θ, including segments of LPs specifically due to lithological controls (see Duvall et al. 2004, JGR). I suggest the authors adopt a more quantitative approach. This will enable them to show the relationships more convincingly and avoid the vague statements given in 17-24.

[1073:11-13] This seems a very inductive approach. I suggest restructuring into a more hypothesis driven setup.

[1073:13-14] Recent glaciation means? As I note above, the point is to establish whether the proportion of ice cover has varied between the litho-tectonic units over the time scale that is relevant to the shaping of topography; i.e. probably since the MPT.

[1074:9-10] Many of these flat reaches are very likely to be sediment fills backed up behind overdeepenings. The flatness is therefore probably a function of postglacial sedimentation, not glaciers.

[1074:16] Are these floodplain sections overdeepenings? Sediment-fills should ideally be excised from the hypsometric analysis because in some cases they can be hundreds of metres deep and therefore misrepresent or bias the elevation distributions in the hypsometry.

[1074:23-28] Again, such qualitative descriptions are misplaced and compromise much

of the interpretations. It is not sufficient to show three ideal examples (Fig. 13) in support of the preferred interpretation. Fig. 13a does not look 'more or less normal' as described (line 24).

[1075:9] Is there a theoretical reason for a linear relationship? Some background is required here (the scale-related issues with hypsometry that are well studied by previous workers but not acknowledged here).

[1075:14] Why assume a non-linear relationship when there may simply be no scaling relationship?

[1075:19] Simply deleting the ice masses from the DEM would alleviate this pleading explanation.

[1076:2-11] Rather than plucking out some ideal examples in support (Fig. 16), I suggest a more quantitative treatment of the results would be more effective and more convincing.

[1076:13] What is presented here is essentially an analysis of digital elevation data, not a geomorphological analysis. That would entail exploring the relationships between forms and the processes responsible for them and I don't agree that this MS does that.

[1076:15] This is an important point: what are the differences between the main litho-tectonic units? Perhaps I missed it earlier but I cannot find where the authors explicitly state these. The lithological generalisations given in Section 2.1 are not really adequate. One key question might be: is the intra-unit variation in erodibility less than the inter-unit differences? If so, good, but the authors need to somehow demonstrate this to be the case.

[1075:16-24] This concept of 'maturity' is not useful in my view. For instance, how would one differentiate low-maturity in strong rocks from high-maturity in weak rocks? One would need information on the timing and source of the perturbations, neither of which seem to be available for this study area. The two properties obviously corre-

late; perhaps the authors might reflect on the 'relaxation time' concept instead (sensu Brunsden Thornes 1979, TIBG).

[1075:24] 'As shown before, this. . .'. I am unclear what is being said here.

[1077:2] The V-shaped valley morphology says more about the absence of glacial erosion than it does about the speed or strength of fluvial incision. V-shaped valleys can have incision rates < 2 m/Myr.

[1077:13-16] The regional ELA refers to the elevation of the former ice surface, which stood many hundreds of metres above the valley floor. Why would knickpoint elevations match the ELA?

[1077:20] What is a 'slope-by-elevation analysis'?

[1077:17-19] I do not see why one would expect any difference to exist. The recent deglaciation limit is irrelevant to the long-term topographic development dealt with here.

[1077:20] Not sure I follow the logic here. Heavy rainstorms might strip regolith from hillslopes but I question whether this would be evident in the large-scale hypsometry. Hillslope morphology is more a function of susceptibility to bedrock landsliding, which has an indirect relationship to precipitation, if any. I wonder whether there might be structural differences between the Helvetic nappes and the External massifs, such as fracture density. That's a term I would like to see in this MS.

[1078:7-10] Recent exhumation (rock uplift) does not necessarily drive surface uplift. Is this conflating rock uplift with surface uplift here?

[1079:24 to 1080:10] This section goes off in the wrong direction. Linking response/relaxation times to time scale of causation is a flawed approach in my view. Exhumation is not really a 'forcing mechanism'; it is a long-term response measurable over long time spans. Yet bedrock landsliding, which is a rapid and short-term process might be the most important driver of exhumation on the slopes. Glaciation has also operated over several millions of years. Even though glacial advances span just a few

tens of thousands of years, subglacial erosion is slow and incremental. Seismicity is short-term, but it has operated over long periods. This section needs to be thoroughly revisited.

[1080:11-18] Here the authors finally get around to stating what should have framed the study from the outset: the hypothesis of lithological control on topographic development should be opening the Discussion, not only closing it. Moreover, the link to Snyder's (2000) conclusions needs quite a bit more bolstering; response time scales depend upon a whole range of climatic, topographic, and substrate factors. The authors could expand on this point.

[1081:1-] 'Lithological architecture' is not dealt with here: all the Figures are presenting morphometric data. The Conclusions listed are interesting, but forced. Some major revisions are necessary to have these lead more naturally from the empirical data.

[Fig. 12] What is 'average' here? Does it include or exclude the full dataset?

[Fig. 14] Do these colours denote something? Please state what. Are these least-squares regressions? Some more information would be very helpful.

[Fig. 16] Are these examples chosen randomly?

[Fig. 17] 'Large' and 'small' catchment means?

3. Technical points [page:line]

[1062:3] Replace 'term' with 'turn'.

[1062:6] Replace 'on' with 'to'.

[1062:6] Replace 'variability' with 'variation'.

[1062:10] Delete '-large'.

[1062:15-16] 'analysis . . . shows', or 'analyses . . . show'.

[1062:20] 'and contains some of the highest'

[1062:23] ‘less steep slopes and’

[1063:17] ‘sliding rates’.

[1064:13] ‘paid to’.

[1064:15] ‘intensively’.

[1064:21] Better to break up this 5-line sentence.

[1066:16] ‘exhumation’.

[1066:18] delete ‘in’.

[1066:18-21] Rephrase to clarify ‘related ages’.

[1066:24] Rephrase ‘GPS bedrock measurements’.

[1067:16] Replace ‘flew’ with ‘drained’

[1067:19-26] I suggest you tabulate this information, noting just the ranges here.

[1068:4-12] Very long sentences are better broken up.

[1068:24] ‘on an annual’.

[1069:10] Remove ‘a river’, and ‘associated with’ is better.

[1069:15] I suggest this first sentence be deleted.

[1069:20] Replace ‘yielding in’ with ‘reflecting’.

[1077:6] I suggest the term ‘low-slope reaches’ rather than ‘plateau’ here and below.

[1078:4] Replace ‘overpressured’ with ‘pressurised’.

[1080:16] ‘easily’.

I hope these suggestions will assist the authors with getting this MS into shape.

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