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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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First, we would like to thank J.D. Jansen for his constructive and very careful review. We incorporated his specific comments and technical points without further questioning and changed the manuscript accordingly. Apart from these specific comments, the referee's main request was to restructure the manuscript in a way that would allow more specific hypothesis testing on the role of the other possible control factors besides lithology (precipitation, uplift, glacial forcing). In this context, the referee challenged our qualitative analysis and asked for a more quantitative approach, which we have done.

We now base our analyses on previous studies where uplift (long- and short-term), glacial inheritance, precipitation and erosional resistance of the underlying bedrock have been invoked to explain the landscape's characteristics, expressed through variables including e.g., mean elevation, hypsometry, relief, hillslope gradients and convexities of stream profiles. We test these relationships through correlation and statistical analyses, and we conclude that among driving parameters, variations in erodibility, which we have measured based on the erodibility map of the Swiss Alps by Kühni & Pfiffner (2001), explain most of the morphometric variations that we can observe within the Rhône basin.

In particular, in order to achieve this, we proceeded in three steps:

1) We quantified erodibility, amount and intensity of precipitation, glacial inheritance and uplift for each basin by extracting mean annual precipitation values, the average daily 90th percentile of precipitation, LGM ice thickness, recent surface uplift, long-term uplift (based on apatite fission-track ages) and erodibility from the dataset of our original manuscript. We extracted these values for each basin. Instead of lithology, we now use the more specific erodibility, which we based on three erodibility classes based on the erodibility map of Kühni & Pfiffner (2001).

2) We then summarized quantitative values for five topographic variables (mean elevation, hypsometry, relief, hillslope gradients, convexity of the river long profiles). We tabulate these topographic variables in an additional table for each tributary basin to make our analyses more transparent.

3) For each of the five controlling variables and mechanisms described in 1), we plot all five topographic attributes outlined in 2) as boxplots. This allows the reader to see that there are significant topographic differences between the three erodibility classes and also within the three defined uplift/exhumation classes. For LGM thickness and precipitation, the correlation to the landscapes' metrics is less pronounced. Then, we run a linear discriminant function analysis to explore whether the tributary basins

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are classified correctly on the base of the five topographic attributes. We found that erodibility serves best to group the tributary basins according to their morphometric variables outlined in 1).

We hope this summary of quantitative data, and the statistical testing of the relationship between observed attributes (topographic measures) and explanatory variables (controlling factors) will convince the referee.

In the following, we will answer to all other comments. The reviewer's comment is marked with a "R:", while our answer is marked with an "A:".

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

A: Done

R: [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?

A: Both, we have specified that. And yes indeed, we have specified these (e.g., large-scale uplift, but this will not change the conclusions of our work).

R: [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].

A: Done.

R: [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'.

A: Done

R: [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.

A: Indeed, we did not intend to oppose climate and tectonics here. We apologize for the misleading term "in contrast" and modified the text accordingly.

R: [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.

A: We are aware of the controversy concerning this point. However, besides a possible climatic driver, there have been studies, which related the 5 Ma increase of uplift (and erosion) to deep-crustal processes such as slab unloading. We added four more relevant works for clarification (Lyon-Caen & Molnar 1989, Schmid et al. 1996, Pfiffner et al. 1997, Sue et al. 2007)

R: [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.

A: That is probably a valid explanation why lithology was not considered in many studies. Especially on a sub-basin scale it is indeed a complicated task to do, if one con-

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siders soil cover, the strike and dip of strata, schistosity, fracture density and so on. But for our scale of investigation we think it is rather straightforward to state that there should be a significant difference between the erodibility of carbonates and granites. Furthermore, we added the erodibility map published by Kühni & Pfiffner (2001), which is valid on a larger scale, and is based on the geological and geotechnical maps of Switzerland (Niggli & de Quervain, 1936), and calibrated with a smaller-scale erodibility map by Jäckli (1957). We acknowledge that we have not given sufficient credit to these papers and have improved the revised text accordingly.

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

A: That is true. Displaying the entire set of available data (250-2008) would just lead to a complete overload in this figure, but the main conclusion doesn't change much. Seismic activity is focused in the zones described. However, we will add the entire dataset to the appendix in a separate figure.

R: [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.

A: We addresses this point by adding the LGM ice thickness (mean values per tributary basin) into the statistical test described above.

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

A: It is the 90th percentile of daily precipitation computed over each single year of the data record and afterwards averaged over the 52 years record. The terms "annual" and "total daily precipitation" were added referring to how values are computed. But we realized it could be confusing, therefore we changed it in "90th percentile of daily precipitation on annual basis".

R: [1069:5] ‘Channel reorganisation’ means?

A: We actually mean ‘reorganization of the channel network’ through migration, abandonment and cannibalization of channels like described in Willet et al. (2014). We added this reference here and rephrase the text.

R: [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’.

A: Done

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

A: Done

R: [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’.

A: We modified this part.

R: [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,

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ESPL), Hurtrez et al. (1999, ESPL), and Cheng et al. 2012, Geomorphology).

A: We have improved this section to the extent that it is in line with the major scope of this paper. However, we emphasize that hypsometry is only one of five variables that we consider here, therefore we decided to address these points/papers only to the extent that they are of relevance for our study.

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

A: We rephrased this part.

R: [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.

A: We hope we addressed this point with our new approach, please see first sections.

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

A: We now described this more specifically in the methodology chapter.

R: [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).

A: These relationships has been discussed and addressed by Norton et al. (2010, Geology) who also focussed on the Alps. We basically applied their concepts and categorization, and improved the text accordingly.

R: [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? This is indeed an interesting point and has been addressed by previous studies, but apparently no final conclusion has been found. We thought that it is beyond the scope of our paper to solve these relationships in a proper way for the Rhône basin and have therefore decided not to enter into this discussion. Replace 'progressing' with 'progressive'.

A: We have replaced the terms as suggested.

R: [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?

A: We addressed this point mainly by introducing the erodibility map of the Swiss Alps by Kühni & Pfiffner (2001, Geomorphology), which is based on an older geo-technical map of Switzerland (Niggli & de Quervain, 1936). We clarified that we used their erodibility classes as a base for our categorization.

R: [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 TopoToolboxdriven analyses could be presented in brief supplementary note.

A: In the light of the new structure of the paper we decided to eliminate the valley width measurements, since they could not convince both reviewers. Also, this variable does not offer an added value to the revised version of the manuscript.

R: [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.

A: As suggested by the reviewer, the interpolation scheme accounts for the strong relation between elevation and precipitation. The spatially distributed product of MeteoSwiss actually reflects the altitudinal gradients that characterize precipitation in mountainous regions (Schwarb, 2000). We wanted to focus on the fact that it is not possible to identify any specific spatial pattern in the precipitation field, but the one related to elevation, which is expected.

R: [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.

A: With "precipitation-driven erosion processes" we intend to consider both the direct effect of rain-slpash and the indirect effect of increased fluvial erosion due to rainfall events (higher discharge and higher transport capacity).

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

A: This part has been modified in the revised manuscript. However, we consider important to provide the reviewers with an overview of the accuracy of the RhiresD product, used in the present analysis. The accuracy depends both on the accuracy of the underlying local measurements (measurement errors) and on the capability of the interpolation algorithm to reproduce precipitation in areas that are not covered by measurement stations. The systematic error in Switzerland, which is mainly due to wind-induced gauge under-catch, was estimated to range from about 4% at lower elevations during summer to even more than 40% at higher elevations during winter (Sevruk, 1985). As indicated by MeteoSwiss in the product's documentation, the error of the interpolation scheme depends on the user's interpretation. If gridpoint values are considered as representative of local point estimates, the errors are considerable (standard error equal to a factor of 1.7 and 1.3 respectively for light and heavy precipitation). Whereas, if gridpoint values are considered as area-mean values, as in the case of the present work, the error is significantly smaller (in the order of 5-30% for mean values over 15x15 km2 areas for intense precipitation events).

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

A: We modified this part.

R: [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.

A: These are the driving forces and related variables. We hope we covered this point in the restructured version.

R: [1073:1-2] These qualitative descriptions and by-eye assessments are needlessly imprecise when there are simple methods available to quantify profile concavity, _, 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.

A: We apologize. We used a more quantitative approach in the revised manuscript.

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

A: This problem should be solved in our restructured version.

R: [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.

A: We have solved this by considering the LGM ice thickness.

R: [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.

A: see answer below.

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R: [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.

A: Unfortunately there is not detailed information about the sediment thickness in the tributary basins available yet. These data exist for the main Rhône valley, where the maximum thickness ranges between ~400 m in the upper part (between Brig and Sion) and up to 900 m in the lower part (between Sion and Martigny). We would assume it is less in the tributary basins, because the floodplains are smaller than in the main Rhône valley, and the valleys are in general narrower. But this would be definitely worth looking at. It is just too much for this paper since this requires geophysical surveys or drillings, which we don't have, unfortunately. However, since we have restructured the paper (see introductory comments above), the paper became a shift in the way of how we interpret the morphometric properties of the Rhône tributary basins. In context, the point raised by the reviewer became obsolete.

R: [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).

A: We modified this section accordingly.

R: [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).

A: We did not want to state that there is always a linear relationship between the hypsometry and the basin-size. In the study area it was just noticeable that (1) there is a scaling dependency, and (2) that it seemed almost linear for some of the basins. We included the scaling dependency into the discussion now.

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

A: see answer above.

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

A: Thanks for the advice, we excluded the glaciers from the DEM and updated the slope values accordingly.

R: [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.

A: We hope we have properly addressed this point in our restructured version.

R: [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.

A: We changed this expression accordingly.

R: [1076:15] This is an important point: what are the differences between the main lithotectonic 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.

A: See before [1071:2]. We addressed this point mainly by adapting the rock erodibility classes defined by Kühni & Pfiffner (2001, Geomorphology), who compiled an erodibility map based on an older geo-technical map of Switzerland (Niggli and de Quervain, 1936, and an updated version from the Federal Statistical Office). We clarified that we

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used their erodibility classes as a base for our classification.

R: [1076:16-24] 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 correlate; perhaps the authors might reflect on the ‘relaxation time’ concept instead (sensu Brunsden Thornes 1979, TIBG).

A: We have rewritten this section for clarification.

R: [1076:24] ‘As shown before, this. . .’. I am unclear what is being said here.

A: We rephrased this part.

R: [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.

A: The V- and U-shaped valley form is excluded now from the paper.

R: [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?

A: This comments is linked with the question of why knickpoints particularly in the tributary basins from the south (i.e., the Penninics) are linked with terminal moraines, which in turn coincide with the elevation of the LGM ELA (Ivy-Ochs et al., 2008). Our interpretations of these observations have originally been inspired by the work of Oliver Korup (Korup and Montgomery, 2008, Nature). However, as mentioned above, we have re-structured our paper with the consequence that this particular point does not need to be addressed in the revised version. Indeed, a full discussion of this yet to be solved problem would require a paper of its own that will be based on geophysical surveys.

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R: [1077:20] What is a ‘slope-by-elevation analysis’?

A: We refer here to the work by Robl et al. (2015), but we acknowledge that reviewer Jansen has not been convinced that this concept, although valid for the scale of an entire orogen (Robl et al., 2015), can be applied to the Rhône basin. We thus decided not to include these information in our paper.

R: [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.

A: We excluded the recent glaciation from the paper.

R: [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.

A: We used precipitation as proxy for a climate variable operating on longer term. We fully agree that data on structural difference would be really helpful in this context, but this information is not available at the level of details required by the reviewer. However, it is, to some extent, contained in the erodibility map by Kühni and Pfiffner (2011).

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

A: Not necessarily, but for the study area several studies have linked the relatively recent exhumation to the development of the current surface topography (e.g., Maurer et al. 1997). However, we try to separate these two terms better.

R: [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

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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.

A: Done. We have rephrased the section for clarification.

R: [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.

A: We restructured the discussion accordingly.

R: [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.

A: We hope we addressed this problem with the new approach (see our first paragraphs).

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

A: This figure is not part of the paper anymore.

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

A: This figure is not part of the paper anymore.

R: [Fig. 16] Are these examples chosen randomly?

A: This figure is not part of the paper anymore.

R: [Fig. 17] ‘Large’ and ‘small’ catchment means?

A: This figure is not part of the paper anymore.

3. Technical points [page:line]

R: [1062:3] Replace ‘term’ with ‘turn’.

A: Done

R: [1062:6] Replace ‘on’ with ‘to’.

A: Done

R: [1062:6] Replace ‘variability’ with ‘variation’.

A: Done

R: [1062:10] Delete ‘-large’.

A: Done

R: [1062:15-16] ‘analysis . . . shows’, or ‘analyses . . . show’.

A: Done

R: [1062:20] ‘and contains some of the highest’

A: Done

R: [1062:23] ‘less steep slopes and’

A: Done

R: [1063:17] ‘sliding rates’.

A: Done

R: [1064:13] ‘paid to’.

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A: Done

R: [1064:15] ‘intensively’.

A: Done

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

A: Done

R: [1066:16] ‘exhumation’.

A: Done

R: [1066:18] delete ‘in’.

A: Done

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

A: Done

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

A: Done

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

A: Done

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

A: Done

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

A: Done

R: [1068:24] ‘on an annual’.

A: Done

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

A: Done

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

A: Done

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

A: Done

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

A: Done

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

A: Done

R: [1080:16] ‘easily’.

A: Done

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