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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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We would like to thank Simon Brocklehurst for his review of our manuscript. His main critical point is that the methods and results are presented in an uneven way and that the results are not convincing in the way they are presented. This overlaps partially with the critical points by J.D. Jansen. We hope that the incorporation of some statistical analysis and a generally more quantitative approach addresses this problem in a sufficient way.

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

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 that this new approach, which is less inductive and more quantitative, will convince the reviewer. In the following, we will answer to all other comments. The reviewer's comment is marked with a "R:", and our answer is marked with an "A:".

R: - Section 2.1. Given the range of variables in play across the study area (e.g., the range in base level for each of the catchments, set by the Rhone), could the authors make more of directly comparing drainage basins entering the Rhone from opposite sides at similar points along the river?

A: Thank you for raising this point, which we considered as very valid. We thus tried to apply this suggestion, but found out that patterns from streams entering the Rhône River at the same elevation are indeed different between streams entering from the South and streams entering from the North. On the river profiles plot, in which we now included all profiles, we normalized the elevations.

R: - Section 3.1. The authors make frequent reference to the "annual 90% of total daily precipitation", yet I never felt confident that I understood what this statistic meant (and how it related to the more familiar 90th percentile). Please could the authors explain with greater clarity?

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: - Section 3.2. The section on river longitudinal profile methods is very brief. Yes, numerous authors have used similar methods, but the authors should still give a clear

account of the methods they're applied here (which links to the comment below, where alternative methods have been selected rather than longitudinal profile analysis, and to comments about the results section).

A: We have expanded this methodology part.

R: - Section 3.3. The account of the hypsometric curve could be clearer, and also doesn't tell the whole story of how hypsometry might be influenced by glacial modification. River-bed hypsometry is given the briefest of mentions. Is this a technique that has been used widely elsewhere? To me it seems likely to tell a story similar to stream gradient, yet with significantly less resolution, so why introduce this new hypsometry approach? It seems confusing to be using hypsometry for both drainage basins overall, and just along the thalweg of the stream, and also to eschew established longitudinal profile analysis here

A: We have expanded this methodology part. As the river-bed hypsometry appears not to have convinced the reviewers, we excluded it from the paper, and only used the basin hypsometry.

R: - Section 3.4. I found it unclear what tests the authors were proposing to undertake based on the hillslope gradients.

A: We introduced this morphometric variable because it has been used in other studies as one of many other variables (e.g., hypsometry, relief etc.) to characterize the morphology of a basin (e.g. Korup et al., 2005). It can give information about land siding potential (and thus sediment transfer) and has been linked to rock mass strength, glacial and climatic modification and so on. We do see that our text was short on explanations and references. We have thus expanded our explanations.

R: - Section 3.5. How robust are measurements of Vfw? Is there any subjectivity here?

A: We decided not to use Vfw anymore, since it could not convince both reviewers, and for the new approach it would not add much.

R: - Section 4.1. Rather confusing comment that spatial precipitation gradients are low, yet precipitation varies from <500 mm/yr to >2,500 mm/yr. See comment above on "annual 90%".

A: We apologize for the confusion. With spatial variability we here refer to the entire Rhône basin. The spatial variability within each basin is high because of the orographic effect, but since this effect is occurring everywhere, the difference between different basins is rather small.

R: - Section 4.2. "Oversteepened head scarps" are one of the features omitted from the longitudinal profile methods (see above). Please outline in the methods the basis of this approach. Also, I couldn't find a clear illustration of the three groups of river channels; Figure 7 is not presented using this framework, so doesn't match the text here. If the authors are going to argue that each of their three geologic domains corresponds more or less uniquely to a different longitudinal profile form, this case needs to be much more compelling. If Figure 7 is filled out with more longitudinal profiles, will they really disperse into three distinct groups?

A: We now included all river profiles in the same figure.

R: - Section 4.3. Given the broad overall range of hypsometric integrals, is there really a statistically significant distinction between the three litho-tectonic units?

A: We hope we properly addressed this problem with the boxplots.

R: - Section 4.5. What is meant by "have usually"?

A: We decided not to use Vfw anymore, since it could not convince both reviewers, and for the new approach it would not add much.

R: - Figure 5. Vfw as labelled doesn't appear to be a specific, readily repeatable measure. How do you know exactly where this is? (See comment above)

A: We decided not to use Vfw anymore, since it could not convince both reviewers, and

for the new approach it would not add much.

R: - Figure 7. Information about relief/elevation and gradient has been lost when longitudinal profiles are plotted normalised like this. Is this a problem? Also, no mention of groups 1, 2 and 3 here (see comment above). A: Not really if elevations are normalized to the point of entry into the Rhône valley, which we have now done. We apologize for the confusion.

R: - Figure 8. As discussed above, more detail on the hypsometry along the river bed would be good. Given the impressive resolution of the topographic data, is the river bed always 1 pixel wide? What's the upstream end of the river bed? What differences separate this analysis from longitudinal profile analysis (and to what extent is this an improvement)?

A: We have removed the river bed hypsometry (see above) and used longitudinal stream profiles only. As such, the comparison between these variables becomes obsolete. We hope that this clarifies our intents and our paper.

R: - Figure 9. Raises the question of how good the topographic data are in this challenging terrain...

A: These photos really illustrate extreme conditions that we have encountered in the Rhône basin, and this appears to have confused reviewer Brocklehurst. We decided to remove it. Indeed, our analyses is based on a lidar 2 m-DEM, which offers an unbeatable database, at least for the scale of an entire basin.

R: - Figure 14. Is the mean HI the mean of the HI values from the individual basins, or the HI of all of the topography within a litho-tectonic unit?

A: We consider the HI of each basin now (boxplots). Also the torrential catchments labelled are only considered very briefly in the text – how important are they? A: They are usually basins of comparatively small size. Sediment transport in these basins is accomplished in pulses that transport a lot of material in short time. One could argue that the basins are probably controlled rather by short-term events such as earthquakes or exceptional high rainfalls and not so much by the longer-term processes we are looking at. However, now we just treat them as a "normal" basin and included them into the analysis, without prior interpretation.

R: - Figure 16. More detail and justification on the locations of the cross sections, please. A : We decided not to use Vfw anymore, since it could not convince both reviewers, and for the new approach it would not add much.

Interactive comment on Earth Surf. Dynam. Discuss., 3, 1061, 2015.