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Co-evolution of volcanic catchments in Japan

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Response to Referee #1 Comment

The authors appreciate the review comments from the Referee. Our responses to each comment are shown below.

Comment 1: Are these catchments studied here at (dynamic) steady state?

- Line 21: How is it related to steady state? Is it implied here that for those basins whose drainage density does not change with age, they achieved steady state?

Response:

The study catchments are all located in dynamic landscapes with the tectonic uplift in the Quaternary Period of 250 – 1000 m (National Research Center for Disaster Prevention, Japan, 1973). We assumed that all the catchments have reached steady states in which rates of tectonic uplift and erosion are equated when averaged over thousands of years. The fact that the catchments locate in the dynamic landscape is compatible with the assumption which we predicated on; that is the assumption that driving forces of catchment coevolution, or catchment forming factors, are climate, geology and tectonic (Troch et al., 2015).

Comment 2: It might be useful to compare, for e.g., slope-area relationship of catchments from Jefferson et al 2010 (Oregon) with catchments from this study. This may shed some more light to the discrepancy observed between Jefferson et al 2010 and this study, since except the oldest basin (HAZ) from this study all other catchments approximately fall in the range of ages studied in Jefferson et al. 2010. Also, drainage density alone may not be sufficient to characterize relationship between hydrology and geomorphology (which is the major goal of this study) for these catchments, and I think, slope-area relationship will be able to better characterize physical processes/ mechanisms governing the evolution of these landscapes.

Response:

We analyzed the slope area relationship for every catchment. The results indicated that the channels dominated by fluvial erosion emerged between 2 – 6 Ma since their formation, depending on the tectonic uplift rate, and extended towards the upper portion of the catchments until they become substantially eroded gentle hillslopes. It should also be noted that the contributing areas of the inflection points in mean slope area relationship are generally smaller than those of the channel heads defined by the digitized stream lines. The discrepancy between the inflection points and the channel heads implies that some portion of the fluvial channels is ephemeral rather than perennial. The extension of ephemeral streams in mature catchments supports the argument that the major flow pathways have changed over time from deep groundwater to shallow subsurface flow, even after the drainage density have reached equilibrium.

Comment 3: Do these volcanic landscapes have distinct slope-area relationships than other soil mantled landscapes?

Response:

We have not conducted slope area analysis for other soil mantled landscapes because interpretation of slope area relationship is not straightforward especially when climatic conditions and tectonic uplift rate are incorporated as described in the revised manuscript. We would continue working on this topic to shed more light on the interconnected nature of landscape evolution and hydrological properties by decoupling the effects of internal properties (e.g., soil hydraulics properties) and external forcing (e.g., climate) of catchments.

Comment 4: Line 219/Line 228: Not clear on what basis (R value) it is decided what is strong and what is weak. In Fig 4 R value is 0.5 and is termed as weak whereas in Fig 5 is 0.6 and is termed as strong correlation. Incremental increase of correlation magnitude of 0.1 changes the correlation from weak to strong?

Response:

We avoid ambiguous expression of weak/strong correlations in the revised manuscript. Instead, consistent description of significant based on the threshold, p-value of 0.05, was used to decide if the correlation is significant or not.

Comment 5: Line 241: For what scales these slopes were estimated?

Response:

The scale of the map is 1:25,000. This information was added to the revised manuscript.

Comment 6: Line 265: It might be worth discussing little bit more the implications of negative correlation here in terms of physical processes.

Response:

The interpretation of negative correlation was discussed in the section 5.2 of the revised manuscript as follows. Our results suggest two hypotheses to be tested in future studies on the evolution of drainage densities in mature volcanic catchments. One is that as catchment further evolve, hydrologically active channels retreat as less recharge leads to lower average aquifer levels and less baseflow; the other is that it does not significantly change after catchments reached maturity in terms of surface dissection.

Comment 7: Line 297: Is there a reason for why more intense surface dissection is expected?

Response:

We deleted 'more' in the revised manuscript, because the increase in the drainage density observed in the Oregon Cascades is at similar rate suggested by Jefferson et al. (2014).

Comment 8: Line 299: Please give more details on what additional mechanisms are referred to here.

Response:

We describe "the mechanism other than the one described in Jefferson et al. (2010)" in the following paragraphs. One is that as catchment continues to age, the hydrologically active channels retreat because of less groundwater recharge leads to lower average aquifer levels and less baseflow. The other hypothesis is that the active channels do not undergo much surface dissection after the catchments reach maturity.

Comment 9: Figure 15: Which order of channel does this stream profile belong to? If we take an average of several stream profiles and compare them with average of Oregon catchments' (Jefferson et al 2010 study), the differences, if observed, may reveal observed differences between the two studies.

Response:

The channel illustrated in Figure 15 (in the previous manuscript), which is the longest channel in SNK, belongs to first to third order stream. With this figure, we aimed to illustrate the existence of knick point in SNK that may have influenced the anomalously high drainage density of the catchment. The comparison of geomorphology in the Oregon Cascades and Japanese catchments is beyond the scope of our study, which aimed to investigate the relation between landscape evolution and hydrologic responses.

Comment 10: Line 349-354: Are these river networks more dynamic than the river networks of Oregon cascades?

Response:

We have added the following description on how the tectonic uplift rate differ in both studies. "*Studies addressing the tectonic uplift in the central and southern Oregon Coast Range suggests that the portions of the landscape are uplifted at a rate of 0.05 to 0.2 mm/yr (Heimsath et al., 2001; Reneau and Dietrich, 1989; Brown and Krygier, 1971; Beschta, 1978), which corresponds to 130 – 510 m in the Quaternary Period. The rates of tectonic uplift of our catchments, ranging from 250 – 1000 m, suggests some of the study catchments are more dynamic than the catchments in the Oregon Cascades. However, the significant correlation between the catchment age and baseflow index for the combined dataset implies that the differences in tectonic uplift rate does not exert significant influence on the rate of catchment co-evolution.*"

Minor:

Comment 11: Line 191: Calculated

Response:

This has been corrected.

Dear Dr. Jefferson,

We appreciate your constructive comments. Our responses to each comment are shown below.

General Comments:

The manuscript shows that 14 catchments, with primarily volcanic bedrock, show both landscape and hydrologic changes with bedrock age. Drainage density is used as the single metric of topography, while baseflow index and slope of the flow duration curve were used as the hydrologic metrics. The authors endeavor also to test climate as a correlate with catchment hydrology and landscape characteristics, but the fairly narrow range of climates represented in the study makes this section less compelling. Overall, I think the paper does a nice job of presenting a new dataset on hydrology in volcanic catchments, but that it tries to do much with too little in terms of making broad claims about coevolution. In particular, I think the climate section is weak and the topographic metrics are underdeveloped, but could be significantly strengthened with some additional DEM analysis. I think with revision this paper could be a strong contribution to HESS, and I look forward to seeing it published.

Response:

We have added the slope-area analysis to the revised manuscript to understand the time scale of landscape evolution and to infer the physical processes governing the catchment evolution. We agree with you that the climate section is weak due to the quite narrow range of aridity indices of our dataset, which makes it difficult to argue the broad claims about coevolution. Thus, we would focus on presenting a new dataset that corroborates the finding of previous research that showed age is a strong control on hydrologic response. By adding the slope area analysis, we would also suggest that tectonic uplift plays a role in the rate of catchment coevolution, which corroborates the suggestion by Troch et al. (2015) that the hydrological age is not only a function of time since formation, but also a function of three catchment forming factors. In this paper, separating the effects of climate and base rock geology is therefore difficult if we are restricted to empirical analysis. We will continue working on this topic by using process-based hydrological models in an attempt to derive responses that are not observable with direct measurement to shed more light on the interconnected nature of landscape and hydrological properties.

Specific Comments:

Slope-area analyses (such as used by Jefferson et al. 2010) are a powerful tool for inferring differences in process domains across catchments. Other DEM analyses that could be done include generation of hypsometric curves and more river long profiles and calculation of average slopes and ruggedness. Analyses such as these would a more robust way of showing that the landscapes are evolving over time and should help support or constrain the interpretation of processes that explain the drainage density trends. In reading the discussion, I noted that the authors inferred that the tentatively observed decrease in drainage density in older catchments might be due to lowering of the regional water table due to blocked recharge. Alternatively, perhaps the topography has been sufficiently eroded in these older catchments such that the slope-area space can't support such a high drainage density anymore. More geomorphological analysis might help support one of these hypotheses.

Response:

The slope area relationships indicate that the channels dominated by fluvial erosion emerged between 2 – 6 Ma since their formation, depending on the tectonic uplift rate, and extended towards the upper portion of the catchments until they become substantially eroded gentle hillslopes. It should also be noted that the contributing areas of the inflection points in mean slope area relationship are generally smaller than those of the channel heads defined by the digitized stream lines. The discrepancy between the inflection points and the channel heads implies that some portion of the fluvial channels is ephemeral rather than perennial. The extension of ephemeral streams in mature catchments supports the argument that the major flow pathways have changed over time from deep groundwater to shallow subsurface flow, even after the drainage density have reached equilibrium. The slope area relationship also suggested that the oldest catchment was substantially eroded and perhaps this may explain the drainage density trend.

Section 4.2: In Figures 5 and 6 and related discussion in the text, the statistical fits seem highly dependent on the very youngest catchment to show a time trend. I was interested to see if the relationships are statistically significant if this catchment is removed from the dataset. Using the data provided in Table 3, I found that there is no statistically significant log-linear fit between age and drainage density (p=0.28) or age and slope of the flow duration curve (p=0.30) without the youngest catchment. In the discussion, the authors do describe how the geology of the youngest catchment may result in an anomalously high drainage density (relative to the age), but I think it would be worthwhile to point out the effect of the youngest catchment within the results section.

Response:

We don't think it is a good idea to describe the anomalously high drainage density of one specific catchment in the result section. Instead, we added the following description in the result section of the revised manuscript; '*It should be noted that there is no statistically significant log-linear fit between age and drainage density (p=0.28) or age and slope of the flow duration curve (p=0.30) without the youngest catchment. We will later discuss the uniqueness of the youngest catchments and how it influence the interpretation of our results*.'

Section 4.3:

In this section, the authors introduce another dataset, that of MOPEX catchments as discussed in Wang and Wu (2013). This dataset is described sometimes as MOPEX catchments and sometimes as the Wang and Wu dataset, and unless the reader has also read Wang and Wu, this will be confusingly inconsistent. More significantly, I think the main result highlighted by graphing the MOPEX data along with the Japanese catchments is how climatically similarly the Japanese catchments are. Given that, it is unsurprising that the authors find no significant trends with aridity index. Based on this, the authors conclude that catchment age is "the strongest candidate as a predictor of the variability in baseflow index." This seems like a rather strong conclusion to draw from a sample over a narrow range of variability for one climatic metric.

Response:

The citation of Wang and Wu (2013) was used consistently in the revised manuscript. We agree with you that we drew hasty conclusion from the limited range of climate variability in our dataset. The conclusion should be "*the finding that the age is a strong control on hydrologic response is in agreement with previous research*", rather than "the catchment age is the strongest candidate as a predictor of the variability in baseflow index".

Technical Comments:

p. 9962, line 20: This sentence seems to require citation demonstrating the rigorous validation.

Response:

Three citations were added to the revised manuscript.

p. 9963, line 12: What scale were the maps?

Response:

The scale of the map is 1:25,000. This information was added to the revised manuscript.

p. 9965, line 10: You call this a weak correlation, but with a standard p-value cutoff of 0.05, I would call this no significant correlation. Examination of Figure 4 suggests that any inference of a relationship between catchment age and aridity index seems heavily dependent on the two outlier age catchments.

Response:

We avoid ambiguous expression of weak/strong correlations in the revised manuscript. Instead, consistent description of significant based on the threshold, p-value of 0.05, was used to decide if the correlation is significant or not.

We also added the following to the revised manuscript; "*Thus, it is not possible to thoroughly investigate the controls of climate and time on catchment co-evolution, and the regression of any signatures is substantially affected by the behavior of the youngest and oldest catchments.*"

p. 9965, line 23: I think there is a typo in this sentence and something other than age is meant. This was also picked up by another reviewer.

Response:

This has been corrected to 'catchment area'.

p. 9667, line 5: I'm not convinced that is fair to conclude that catchment age is the dominant descriptor of hydrologic variability, if the authors explicitly framed your paper as testing age versus climate but then couldn't really test climate. That said, the finding that age is a strong control on hydrologic response is in agreement with previous research in volcanic catchments.

Response:

As described above, we agree with the reviewer. The main interpretation of our dataset is that "*the finding that the age is a strong control on hydrologic response is in agreement with previous research*", rather than "the catchment age is the strongest candidate as a predictor of the variability in baseflow index".

p. 9667, line 17: "They" is unclear.

Response:

This has been corrected to 'Jefferson et al., (2010)'.

p. 9668, line 24: Citation needed for the statement about flow paths changing from vertical to shallow subsurface over time.

Response:

Citation of 'Jefferson et al. (2010)' has been added to the revised manuscript.

p. 9668, line 27-28: It's not clear why disconnection of the channel network from aquifers would result in a decrease in drainage density, unless it is specified that it is a decrease in the extent of the perennial stream network.

Response:

This 'drainage density' has specified as 'the extent of the perennial stream network' in the revised manuscript.

p. 9669, line 5-6: Here "western US", previously "Oregon Cascades."

Response:

'Oregon Cascades' is used to maintain consistency.

p. 9669, line 8-9: How much difference in drainage density between Oregon and Japan could be due to differences in mapping standards?

Response:

Mapping standards of the US and Japan are both not available; hence the difference in the mapping standards might be a reason for the anomalous drainage density of SNK. However, it is not convincing given the drainage densities of the catchments older than 2 Ma are similar. We would thus assume that the difference in the mapping standards is negligible for comparison although there still remains uncertainty.

p. 9669, line 25: Awkward construction of the last phrase in the sentence.

Response:

This sentence has been revised into ‘the basalt in the upper catchment is younger than that in the lower catchment’.

p. 9670, line 9: Are there any citations to support the assertion that acidic water dissects the landscape faster anywhere in the world?

Response:

The following was added to the revised manuscript; “*Acid precipitation significantly promotes weathering and soil formation by the input of rainfall and H+ (Huang et al., 2013). Acidic stream water is neutralized by the chemical weathering of primary silicate minerals in humid forested watersheds (Johnson et al., 1981).*“

p. 9670, line 10: Ending the discussion with this rather drawn out and speculative discussion of a single watershed seems weak. I think the last paragraph of the conclusions is an appropriate and much stronger way to end the discussion (especially since it is not really conclusions).

Response:

The last paragraph of the conclusion section was moved to the discussion.

p. 9670, line 14: e.g., is unnecessary since the authors have listed all of the variables examined and not just given examples.

Response:

This has been corrected.

p. 9670, line 16: This is the first mention of intra-annual or seasonal water balance. I suppose the slope of the flow duration curve and the baseflow index tell you something about the overall water balance, but I didn't see anything broken out seasonally.

Response:

This has been corrected as "*The age of volcanic rock was significantly related to the partitioning of deep groundwater recharge and shallow subsurface flow*".

p. 9670, line 20-21. Alternatively drainage density has stayed constant in mature catchments.

Response:

This has been added to the revised manuscript.

p. 9670, line 21-22: DD is controlled by aridity index for a large array of diverse watersheds (Wang and Wu), but that assertion has not been convincingly supported in the Japanese catchments.

Response:

This sentence has been corrected as follows; "*While aridity index does not explain the trend in drainage density given a quite narrow range of aridity index of our dataset, catchment age has a more significant impact on the changes in drainage density.*"

p. 9671, lines 1-3: This sentence doesn't really say anything.

Response:

This has been removed from the revised manuscript.

Table 2: It would be lovely to organize the catchments in this table in the same order they are presented in Tables 1 and 3.

Response:

The catchment order in Table 2 has been corrected.

Figure 7: What was the 0.35 to 0.45 range in aridity index chosen for presentation?

Response:

The following reason has been added to the revised manuscript. "*We used this range because it comprises catchments that have wide range of age (from 0.225 to 11.7 Ma).*"

Figure 8: The caption describes weak relationships, but in fact they are non-significant if the p-value cutoff is 0.05 as usual.

Response:

This has been corrected as “not significant”.

Figure 9: The inset of Figure 9 is the same as Figure 8. I think these two figures out to be combined.

Response:

The inset of Figure 15 (in the revised manuscript) aims to illustrate how the plots of our dataset clustered along the curve suggested by Wang and Wu (2010), but not fully explained by the curve. It is not the same as Figure 8, thus we leave the figures as they are.

Figure 10: Are the MOPEX catchments the same as the Wang and Wu catchments used in the previous figure? Consistent labeling would nice.

Response:

We avoid expression of MOPEX catchments and consistently used Wang and Wu dataset in the revised manuscript.

Figure 11: I don’t think that the BFI in Jefferson et al. (2010) is calculated in quite the same way as in this study. Did the authors evaluate what difference that might make on the reported BFI values? If there is an effect of the methodology, how can the authors justify fitting a line through the combined datasets?

Response:

We added the following to the revised manuscript; “*To compare the baseflow index values of our study with those presented by Jefferson et al. (2010), we separated the baseflow from total streamflow by using the Hysep procedure (e.g., Sloto and Crouse, 1996) presented in Jefferson et al. (2010).*”

Figure 12: Why switch to baseflow coefficient here when baseflow index is used elsewhere? This

doesn't seem to be justified or explained in the text.

Response:

The reason for switching to baseflow coefficient is based on Wang and Wu (2013), who hypothesized that the aridity index is the first order indicator of the partitioning of precipitation into baseflow, and found a strong correlation between perennial stream density and the baseflow coefficient, Qb/P, which follows the complementary Budyko curve (Fig.16).

Figure 12: In the caption, the authors report a correlation coefficient (0.74) but no associated p-value. Also, their reported correlation coefficient is for the Wang and Wu catchments and not the ones in the present study. It seems like it would be more relevant to report the relationship for the present study or the combined datasets.

Response:

The correlation coefficient of the combined dataset was added to the caption.

Figure 14: Unclear what the northing and easting are relative to, so some latitude/ longitude coordinates would be nice. Also, in the lower watershed the blue elevation colors make it difficult to pick out the channel network. Finally, there's no units on the color scale.

Response:

The figure has been corrected.