

Interactive comment on “A process-based diagnosis of catchment coevolution in volcanic landscapes: synthesis of Newtonian and Darwinian approaches” by Takeo Yoshida and Peter A. Troch

Anonymous Referee #2

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This paper is overall a nice model-based addition to the growing literature on volcanic catchment co-evolution. I really like how the authors have examined the effects of catchment internal properties versus climate on hydrologic response in way that can't be done in the real world. The results conform to the emerging conceptual view of how the hydrology of volcanic catchments changes as they age. However, I am concerned about over-interpretation of the results (because they fit so nicely!) relative to what can be said with confidence based on this modeling exercise.

The first referee has provided some really important points about the parameter un-

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certainty and estimation and fairly low model performance as measured by the NSE. While I respect the authors' point that they are able to reasonably replicate the observed slopes of the flow duration curves (SFDC), as shown in the figure, the overall lack of fit of the model to the data raises questions about how much credence we should give to model results for the simulated combinations of climate and catchment characteristics. How much of the SFDC residual is due to model performance versus the processes actually under study? It seems like a more complete uncertainty analysis that attempts to propagate the model uncertainty through the results would be quite useful in interpreting the findings.

Beyond these major points, I have a few secondary points.

The authors have assessed how a large number of catchment parameters change with catchment age and climate. They use a cut of $p < 0.05$ as a test of statistical significance of these regressions, without making a correction for multiple comparisons that can lead to false positives. I suggest that the authors apply the standard Bonferroni correction and adjust the p-value for significance accordingly.

In the paragraph around line 65, the authors make a statement about the changes over time in clay layers, chemical weathering, vertical recharge and shallow subsurface flow in volcanic catchments. They cite Jefferson et al (2010) and their previous paper. It should be noted that neither of these papers actually directly observed those processes. Instead both papers were empirical studies of change in stream hydrographs with catchment age that put forward these ideas as possible explanations for the hydrologic signatures. There is literature on clays and chemical weathering of basalts with respect to soil development (c.f., work by Oliver Chadwick and Peter Vitousek), and the work of Lohse and Dietrich (2005) adds some hydrological context to the soil development story.

Around line 72, the mention of the aridity index doesn't make sense for readers unfamiliar with the previous paper. Perhaps the authors should provide more context.

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The conceptualization in Figure 1 is very nice, but mean delta-signature should be defined within the figure or its caption. Also, is the only way to know what the slope of B looks like through an empirical set of watersheds of different ages? How does this limit the utility of the framework you put forward?

In Figures 4 and 5, the x-axis is labeled with units of ka. I believe it should be Ma, based on the assembled catchment ages.

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