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Comment on hess-2022-315

Anonymous Referee #2

Referee comment on "Increased nonstationarity of stormflow threshold behaviors in a forested watershed due to abrupt earthquake disturbance" by Guotao Zhang et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2022-315-RC2, 2022

Review of "Increased Nonstationarity of Stormflow Threshold Behaviors in a

Forested Watershed Due to Abrupt Earthquake Disturbance"

Author(s): Zhang et al.

MS No.: hess-2021-228

- Band, University of Virginia

General comment:

Zhang et al. present an interesting study of stormflow runoff threshold non-stationarity over a time-line before and following a major earthquake in the eastern periphery of the Tibetan Plateau. The earthquake resulted in a massive disturbance of the dominant forest cover due to extensive landsliding which subsequently expanded with monsoon-initiated landslide growth, then slowly began to recover with revegetation, and presumably, renewed colluvial infilling of scars.

The paper provides a good illustration of specific controls of non-stationary threshold behavior in response to geomorphic disturbance and a chronology of ecosystem recovery. This adds to our knowledge of storm event-based threshold behavior with good evidence of the watershed system dynamics and time scales of adjustment. It may be argued that this is an end-member in terms of magnitude of disturbance, but may be increasingly applicable to other cases of large, sudden land use change and slow recovery due to devastating storms, fires, or other disasters.

Specific comments (scientific questions/issues):

The documentation of the threshold stormflow behavior is interesting, but there are a set of areas in the text that are unclear. Specifically, the methods need to be clarified. Otherwise, some of the interpretation and conclusions may appear to be more qualitative and speculative, and not specifically supported by the data.

Figure 3 is a major result and contributes prominently to the conclusions. However, there do not appear to be sufficient observations to separate out the highest thresholds and trend with statistical significance as it appears this is determined by a single, large event. It is also not clear from the methods whether discharge was separately measured or determined for grass shrub, forest, and landslide areas. The position of the gauges suggests each drainage area is a mixture of all three land covers, and more information is required to see how each land cover contribution is deconvolved. Add more detail to this discussion. If separate measurements were not made it is not clear how these piecewise regressions were made. If this is done by modeling using curve numbers or HEC-HMS this should be clear.

Finally Figure 3 is difficult to interpret as all data points have the same symbol and color, over all land uses. Either color code or use a different symbol so the reader can assess the degree of separation between the trends. Clarifying the statistics provided would also help. There is a composite r^2 provided in table 2 for each of three distinct land uses, including two thresholds and three slopes. While the overallthe correlation is significant What is the confidence in each of these parameters? Is it possible to provide SEE for each? I presume this may not be possible for the highest flow slope values if they were support by a single large storm observation.

The authors cite the scarcity of measurements pre-earthquake, and the logistical difficulty of accessing areas post-earthquake as limiting the information available to assess stormflow threshold behavior through this time. Some information is derived from simulation modeling, developed in a previous paper. More information on the number of actual measurements, the information provided by the HEC-HMS model, and its reliability should be provided. The authors point to a specific "tipping point," after which the stormflow thresholds begin to increase again. Are these based on land cover change derived curve numbers within the model, and are there discharge measurements sufficient to verify these changes? In figure 4b we see peak discharge for a set of events first increase and then decrease as the forest ecosystem begins to recover. How are these peak discharges adjusted for the size of the storm, or are they averaged from a larger number of events?

The analysis of threshold behavior shown in figure 5 is presented in the discussion section. I think this should be in the results section, then discussed/interpreted in the discussion section.

Technical corrections, clarifications:

- Reword so it is clear that it was estimates there were roughly 2×10^5 landslides initiated (which is amazing)
- Line 107, the term indeciduous is not clear. Remove and simply call the canopy conifer.
- Line 168-170, sentence may be better placed either in the introduction or discussion. It is not a specific result of the analysis done here.
- Line 261 – I presume you mean the time to peak *decreased* not *increased* following the earthquake?