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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Modelling of the hydrological connectivity changes in the Minjiang Upstream after the Wenchuan earthquake using satellite remote sensing and DEM data" *by* H. Z. Zhang et al.

I. Fuller (Referee)

I.C.Fuller@massey.ac.nz

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This is an interesting paper that seeks to quantify changes in connectivity in the upper Minjiang catchment following large-scale landsliding triggered by the Wenchuan earthquake in 2008. The authors make good use of an excellent resource in the form of remotely sensed images to detect landslides and vegetation recovery over c.8 years. The results show the landscape disturbance following initial seismic shock, followed by a progressive recovery, from which changes in hydrological connectivity are inferred

/ modelled. The thrust of the paper is the development of a model that describes and predicts such changes. As a process geomorphologist I wonder whether the modelling approach proposed is rather oversimplified (and indeed the authors acknowledge the remaining challenge it is to predict debris-flow triggering rainfall events using this model, P1122, L21). My concern is that the model presented requires simplification of some key boundary conditions. Notably, no accounting for variability in rock mass strength is made – the model assumes all the rock is 'hard', i.e. a high degree of rock competence (cf. p1119, L5), but this is surely a serious oversimplification, especially since the authors note on p.1116 L18 that most of the area's bedrock is deeply weathered and fractured. Furthermore, proximity to the fault thrust zone is also likely to contribute to the degree of rock competence, with a reduction in competence along the thrust belt in the south of the study area, which means that a single descriptor of rock competence is probably not valid. The authors' contention that topography is the key variable controlling vegetation growth (modulating landslide susceptibility and connectivity) (p1120, L8) is thus called into question. The paper is focused specifically on hydrological connectivity, but I wonder whether the authors intend something broader, since debris flows and landslides are the focus of the study, not simply runoff. In other words, sediment transfers, as well as water transfers are critical here. As such, the paper may be better placed in a broader catchment connectivity context and could make wider reference to papers by Fryirs (Earth Surface Processes & Landforms 2013); Fuller & Marden (Geomorphology 2011) and Harvey (Catena 2001) to broaden the scope. One other concept to consider by way of interpretation is that of sediment exhaustion, i.e. on P1123, L13, a further reason for slope stabilisation could be that the least stable material had now been removed by the initial landslides, so that there is less material to fail when subject to further rainstorms. It would be interesting to know from a process and a hazards perspective what proportion of landslides in the region are shallow / deep, and also how that distinction is defined here. Inevitably, as the authors recognise, predicting landscape response to storm events is extremely difficult, given the complexity of natural systems. I believe this difficulty and complexity should be embraced in research within this field. Land managers and hazard analysts must recognise the complexity of these natural systems. I would urge the authors to make more of this. Grammar and phrases are in need of attention throughout to clarify and simplify some of the written English. Unfortunately there are too many instances for this reviewer to be prepared to list, but to start with, the title should replace 'upstream' with upper catchment, and Chinese should be spelled correctly in the author's addresses.

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