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Interactive comment on "Geomorphometric delineation of floodplains and terraces from objectively defined topographic thresholds" *by* Fiona J. Clubb et al.

Anonymous Referee #3

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This paper presents a new technique for mapping floodplains and terraces from digital elevation models. The paper is generally well written and the approach is both novel and useful. My biggest concern is the authors' claim that the tool is fully automated, when it does not really produce reliable maps in fully automated mode and would require users to manually edit maps to make them reliable, just as is the case with any of the other semi-automated techniques out there. I would suggest the authors tone down the somewhat disparaging comments regarding existing semi-automated techniques and at the same time tone down the sales pitch on their method being fully automated (just add a caveat that user interaction is needed to produce reliable maps). Aside from that concern and a few other minor question and suggestions below I be-

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lieve the paper will make a nice contribution to ESD.

Lines 92-99: This explanation is not articulated well. I suggest revising, and perhaps condensing this section on Dodov and Foufoula-Georgiou. It seems to be a disproportionate amount of information compared to other studies discussed and the extent to which this information is utilized in the rest of the paper.

Line 113: Overprediction is a feature, not a bug. These are decidedly semi-automated approaches and it is a benefit if the automated portion of the tool slightly overpredicts because it is easy for the user to manually clip polygons.

Line 179: So in the end you use Optimal Weiner filter, correct? If so, why go into detail about Perona-Malik? I suggest either making a better connection between the two filters and explaining how the Perona-Malik equations relate to the Open Weiner filter, or reduce discussion on P-M and instead provide more detail on the OW filter.

Line 202: terrace should be terraces

Line 203: The authors don't provide any evidence that third order is a reasonable threshold. I have frequently seen terrace features on first and second order streams in places in the northeastern, Midwestern and western US. I suggest removing this arbitrary suggestion and simply explaining how the user should determine what the threshold should be for their particular landscape.

Lines 220-234: The authors spend a lot of time explaining quantile-quantile plots. Such explanations may be best left for textbooks as q-q plots are fairly routine, but I leave it to the authors to decide whether or not it is necessary to include. More importantly, I think it is important that the authors explain why it is reasonable to assume that local gradients would follow a Gaussian distribution and why deviations from Gaussian are likely to be transitions between process domains.

Line 240: In what way to do you mean 'connected to the modern channel'? Certainly terraces can abut the modern channel.

Line 296: How and why did you separate flood zones into 100 year and greater than 100 year flood risk? Just based on comparison with the FEMA maps? If so, are the FEMA maps necessarily reliable? Many would consider floodplains above the 100 year flood flood zone to be terraces. At what point do you make this distinction?

Table 4: The authors were somewhat disparaging about semi-automated approaches that have been developed earlier. Seeing these reliability and sensitivity values, I would suggest that the tool they have developed is no different. In comparisons with mapped terraces the tool is mapping a lot of false positives and false negatives. To map terraces reliably a user would need to manually edit these extensively. . .that's fine. . .it's to be expected, really. . .and that's why previous algorithms have claimed to be semi-automated. But I would urge the authors not to make claims about it being a fully automated process when the automated process fails to produce a reliable map.

Lines 445-450: I don't think the authors have made a strong case that their method produces reliable maps as a fully automated system. I agree that their method is a useful first cut, but this is no different from Stout and Belmont or any of the other semi-automated approaches mentioned in the paper.

Line 469: There are several other key papers that could be cited as examples of using terraces to quantify sediment budgets: Trimble, S. W. (1999). Decreased rates of alluvial sediment storage in the Coon Creek Basin, Wisconsin, 1975-93. Science, 285(5431), 1244-1246. Belmont, P., Gran, K. B., Schottler, S. P., Wilcock, P. R., Day, S. S., Jennings, C., ... & Parker, G. (2011). Large shift in source of fine sediment in the Upper Mississippi River. Environmental science & technology, 45(20), 8804-8810. Brown, A. G., Carey, C., Erkens, G., Fuchs, M., Hoffmann, T., Macaire, J. J., ... & Walling, D. E. (2009). From sedimentary records to sediment budgets: multiple approaches to catchment sediment flux. Geomorphology, 108(1), 35-47.

Line 474: Several key papers needed to substantiate this statement as well. Lots of examples, such as: Pazzaglia, F. J., & Brandon, M. T. (2001). A fluvial record of

long-term steady-state uplift and erosion across the Cascadia forearc high, western Washington State. American Journal of Science, 301(4-5), 385-431. Avouac, J. P., & Peltzer, G. (1993). Active tectonics in southern Xinjiang, China: Analysis of terrace riser and normal fault scarp degradation along the HotanâĂŘQira fault system. Journal of Geophysical Research: Solid Earth, 98(B12), 21773-21807. Viveen, W., Schoorl, J. M., Veldkamp, A., & Van Balen, R. T. (2014). Modelling the impact of regional uplift and local tectonics on fluvial terrace preservation. Geomorphology, 210, 119-135.

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