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

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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 believe the paper will make a nice contribution to ESD.

Thank you very much for your comments on our manuscript. In response to your concerns, along with those from the other two reviewers, we have made a clear distinction in the paper about which part of the method are automated, and which parts still require user-defined parameters. We did not intend to be dismissive of other techniques of identifying floodplains and terraces - we agree that these methods are very useful, and have stated this in our manuscript. We have tried to build on these methods by developing statistical techniques for the selection of the thresholds in our method of elevation compared to the channel and local gradient. We have made clearer in our discussion that we believe that the different methods are valuable depending on the scale of the analysis, as well as the field site from which the floodplains/terraces are being extracted.

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.

We have condensed this section as suggested.

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.

We have added in a sentence here to state that the user can manually clip the over-predicted surfaces and remove areas selected incorrectly:

'These semi-automated methods allow the user to manually clip over-predicted terrace surfaces based on field data and DEM observations, and remove surfaces selected that do not represented terraces, such as roads, alluvial fans, or water bodies (Stout and Belmont, 2014)'

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.
We use the Perona-Malik filter for the method of floodplain/terrace extraction. The Perona-Malik filter is a non-linear filter which enhances the transition between features, such as hillslopes/valleys, while preferentially smoothing low gradient surfaces, such as floodplains or terraces. The Optimal Wiener filter is only used here for the extraction of the channel networks using the method outlined by Grieve et al. (2016, ESURF). We have added a sentence to clarify this in the manuscript.

Line 202: terrace should be terraces
Done

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.
In each of our field sites we found that a third order threshold was appropriate for where the terraces initiated in the landscape (see Figures 7 and 8). We have changed this section to state this, and we have clearly stated that a visual inspection of the DEM compared to the channel network should allow the user to select the appropriate threshold stream order:
'We found that a threshold of third order channels was appropriate for each of our field sites, based on a visual inspection of the DEM. One of the outputs of our software package is a raster of the channel network labelled by the Strahler stream order. The

user can identify an appropriate threshold stream order based on visual inspection of floodplain and terrace surfaces compared to this network. '

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.
We believe that it is important to include the description of the quantile-quantile plots as this is a key part of our methodology for selecting the thresholds of gradient and elevation compared to the channel from the DEMs. We chose a Gaussian distribution as a simple model, which can be applied generally over a range of landscapes, and has been used in previous methods of feature extraction (Lashermes et al., 2007; Passalacqua et al., 2010). We have added in some more discussion about the Gaussian distributions in response to this comment plus comments from Reviewer 2.
'Furthermore, in some cases our method did not select all of the terraces identified by the field mapping, particularly at the highest elevations compared to the modern channel (e.g. Figure 7c and d). This may be the case if the threshold for elevation compared to the channel selected by the quantile-quantile plot is lower than that of the highest terrace elevations. This can be examined for the landscape in question by a visual inspection of the quantile-quantile plots and the location of the threshold compared to the distribution of channel relief (e.g. Figure 2). Our method fits a Gaussian distribution to the quantile-quantile plots, and selects the thresholds as the deviation of the real data from this distribution. However, in some landscapes, the distribution of elevations may not be accurately represented by a Gaussian distribution.'

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

The method identifies patches of floodplain as those which are at a similar elevation to the modern channel (based on the extracted channel network), whereas terraces should be at a higher elevation compared to the channel. This was not clear in our original wording: we have rephrased this and added more discussion about the separation between floodplains and terraces to the manuscript based on comments from Reviewer 1.

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?
The separation of flood zones into 100-year and greater than 100 year was on the FEMA maps which are classified based on the annual percentage chance of flooding. There may be some errors with the FEMA flood maps based on this: this may a cause of some of the discrepancies between the floodplains extracted from our method and with these published maps. We have a section in our manuscript discussing some of the potential problems with the FEMA flood maps:
'Published flood maps are useful in providing an independent estimate of likely floodplains in each field site. However, there are potential limitations to these maps which must be carefully considered, and may result in some of the differences compared to geomorphic floodplain prediction techniques. Hydrodynamic models have a large number of parameters, which require careful calibration with field and hydraulic data, such as channel roughness and discharge data from gauging stations. Furthermore, due to the time-consuming and expensive nature of these studies, flood maps are often not produced for small catchment sizes, and may therefore be incomplete on a landscape-scale (e.g. Figure 4). There may also be differences in the methodology used in producing these maps for each site, depending on the input topographic data and modelling software used.'
The distinction between floodplains and terraces is something that may also cause

some problems in our method, especially when the terraces are close in elevation to the modern channel. We have also added in more discussion about this to our manuscript based on comments from Reviewer 1.

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.
We did not intend at all to be disparaging about semi-automated approaches that have been previously developed: we think these methods are very useful, particularly in areas where there is some field data available to calibrate the selection of thresholds and user-defined parameters. We have tried to build on these methods by developing statistical techniques for the selection of the thresholds in our method of elevation compared to the channel and local gradient. As previously stated, we have now made a clear distinction in our manuscript between the user-defined parameters and these thresholds which are calculated statistically. We have made clearer in our discussion that we believe that the different methods are valuable depending on the scale of the analysis in question and location from which the floodplains/terraces are being extracted:
'Semi-automated methods of terrace identification, where the terrace polygons are manually edited by the user, are particularly useful in areas where independent datasets of terrace locations are available for calibration, and may be more appropriate than our method on site-specific scales (e.g. Stout and Belmont, 2014). However, the selection of thresholds based on a objective statistical approach means that our method can be applied in areas where these data do not exist, on a broader landscape

scale, or as a rapid first-order predictor of terrace locations.'

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.
See reply to comment above.

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.
We have added in the suggested references.

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 HotanâAËŸ RQira fault system. Journal ËĞ 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.

We have added in the suggested references.

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