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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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Thank you very much for your positive review of our manuscript, and the interesting points raised about the automation of the method with respect to future research directions. In response to your comments and those of the first reviewer, we have made a clear distinction in the paper about which parts of the method are automated, and which parts still require user-defined parameters. We have also included a section in the discussion about potential future improvements to the method which could reduce the number of user-defined parameters, and lead to fully automated feature extraction.

This paper is a strong contribution to ESURF and a clear step in the right direction towards a mapping floodplains and terraces. I am particularly pleased about the idea of using the quantile-quantile plot approach, which provides a null hypothesis (in this case, normally-distributed topography) against which the landscape may be tested. The majority of my comments are in the paper itself, an annotated version of which is attached.

Thank you! We have responded to your general comments below, and have addressed the specific comments on the manuscript in our revised version and the full author response to reviewers.

1. First, I will echo the first reviewer in writing that there needs to be a more clearly-defined line between 'fully automated' and 'semi-automated'. In other words: define the realms within which your model is automated or is not. Currently, the lack of a well-defined separation undercuts the advances that you really have made by making it seem as if you overstate the work and making the focus on the 'it isn't that far' rather than 'it is a big step beyond prior work'. I have read that Clubb et al. have responded to the first reviewer already in response to this general concern, so I will go on to a couple more specific points:

In response to this comment, and the comments from Reviewer 1, we have edited our manuscript to highlight the distinction between the parts of our method that are fully automated (the statistical selection of the thresholds from the quantile-quantile plots) and the need for some user-defined parameters. Our method does still have some parameters that are user-defined (threshold stream order for running on a landscape scale, width of the swath, and minimum height above the channel for floodplain/terrace distinction). However, in general these parameters can be estimated easily by the user from visual inspection of the DEM, and don't require the input of any independent datasets. However we agree that future research is needed in order to create a fully autonomous method, which is beyond the scope of our paper at the moment. We have added a section to the discussion on future research directions, highlighting the points raised in the review comments:

'A key goal for the Earth surface research community is to develop fully-automated methods of feature extraction from DEMs in order to avoid expensive and time consuming field-mapping, and to investigate the controls on geomorphic processes at a landscape scale. Our new method of floodplain and terrace delineation attempts to meet some of these research needs, by allowing the statistical determination of the thresholds for feature extraction. However, our method still requires the input of some user-defined parameters. If the method is run across the whole landscape, the user must set a threshold stream order for the calculation of elevation compared to the nearest channel. This is necessary so that each pixel is mapped to the main channel along which floodplains or terraces have formed, rather than narrow tributary valleys. We suggest that a threshold of third order channels is appropriate for most landscapes, but this can be determined easily by the user from a visual inspection of the channel network. If the user runs the method based on the swath mode, the width of the swath profile must be set. This can also be done based on a visual inspection of the DEM to provide a sufficiently wide swath compared to the valleys in the landscape. Furthermore, if the method is run in the swath mode, then a minimum terrace height must be set in order to delineate between floodplains and fluvial terraces.

However, future development of new algorithms, such as extraction of valley widths, would allow these parameters to be set based on the topographic data alone. Our method represents a first step towards this goal of fully-automated geomorphic feature identification, which can be improved upon with future research. The combination of different algorithms for terrain analysis, such as hillslope flow routing, channel network extraction, floodplains, and fluvial terraces, would allow an objective landscape-scale investigation of the controls on geomorphic processes.'

(a) One arbitrary piece is the decision about how wide of a swath should be used to search for terraces. To me, this highlights something that has long been on

my 'to do' list: a tool to automatically compute the widths of river valleys (see Shaw et al., 2008, for an analogous problem in coastlines). So I think that your use of an user-defined parameter is due to the lack of a tool that is outside your current scope, making this a placeholder for a better method!

This is definitely an area that needs further research, and would improve our method along with other algorithms for digital terrain analysis. We have added this to our new section in the discussion (see reply to general comment above).

(b) Your wording hints that there are problems in terrace identification when a river exists below a high plateau surface, and that these require some special parameter choices. This could also be aided by a tool to identify valley widths, but a more satisfying explanation about possible failure modes and ways around them – especially considering the range of upland topographies from steep lands with ridges to flat upland plateaus - would be more satisfying.

Yes, the method does not work in areas as well where there is less distinction between the relief structure of the surrounding topography (for example, the plateau surface in the Le Sueur River site) compared to the floodplains or terraces. We have added in some more discussion of the results for the Le Sueur River to clarify the difficulties of automatic feature extraction in these landscapes:

'The Le Sueur River is currently incising through Pleistocene tills, forming a low-gradient surface or plateau (Fisher, 2003; Gran et al., 2009; Belmont et al., 2011). High-altitude, low-gradient surfaces, such as relict plateaus, may result in error in the method due to the difficulty in distinguishing the distribution of terrace elevations from these low-relief surfaces. The Le Sueur River basin is also heavily influenced by human land use, which makes feature extraction challenging (Passalacqua et al., 2012).'

2. Second, and related: I wonder why you chose a Gaussian distribution as the 'landscape null hypothesis' from which you search for variations. I see the power in its *simplicity, but do wonder whether you could replace the Gaussian distribution with the distribution expected from a stream-power-erosion plus hillslope-diffusion (I'll write it in a linear way here) simple model: $\delta z / \delta t = -k_{SP} A^m S^n + k_{HS} \bigtriangledown^2 z$. By integrating through time (e.g., numerically with a landscape evolution model), one can generate a non-Gaussian 'landscape null hypothesis'. This to me would seem a more powerful approach insofar as it represents what is expected on the landscape in absence of floodplains and terraces, but does have sensitivity to the k values chosen (or calibrated to the given landscape with another automated procedure). Nevertheless, I think that some of the by-hand 'tweaking' with the quantile-quantile plots could be reduced by comparing the measured landscape against a more physically-based elevation distribution. To be clear: I am happy to see this paper published without changing its entire basis, but would feel remiss to not leave a record of this idea as a potential future avenue for improvement.*

We chose the Gaussian distribution to use as the reference distribution for the elevation distribution in the landscape in order to keep the approach general, so that it could be applied across multiple landscapes with varying relief, and to limit the amount of user-defined parameters in the method as much as possible. Furthermore, the Gaussian distribution has also been used in feature extraction algorithms previously (e.g. Lashermes et al., 2007; Passalacqua et al., 2010). The idea of using a simple stream power and hillslope diffusion model to generate a distribution of elevation and slopes as a 'null hypothesis' is a very interesting one, which we could potentially apply to improve our method in the future. However, it may actually generate more user-defined parameters than the Gaussian distribution does at the moment (as you say, this may be sensitive to both erodibility and hillslope diffusivity). Although we feel it is beyond the scope of the paper to add this in at the moment, we have expanded our discussion to include some more of the potential limitations of using the Gaussian distribution to model relief.

'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.'

In both of these cases, I think that your approach is the right set of steps towards a process that is fully automated, and think that the places in which it is not fully automated serve to highlight areas in which advances are needed; such advances can lie outside of the scope of this paper.

This is a good point - again we have tried to address this by adding in our new section to the discussion (see reply to general comment 1).

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