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Solid Earth
Discussions

Interactive comment on "A Semi-Automated Algorithm to Quantify Scarp Morphology (SPARTA): Application to Normal Faults in Southern Malawi" *by* Michael Hodge et al.

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Summary

This paper describes an automated method of fault scarp detection and measurement. The method is first tested on synthetic data, and then applied to faults in southern Malawi on real digital elevation models with a range of resolution (from 30 m to 50 cm). I think that this tool could be used on a range of normal fault scarps, to make quick automatic maps of displacement. I think this is a nice study with a good level of analysis. However there are some changes that could be made to this manuscript to improve the clarity of the methodology and application to the Malawi faults, and

the limitations of an automatic approach. I think that any reader should be cautious in applying automatic methods to DEMs, and the interpretation of scarp heights must take into account (1) the age of offset features, (2) how variable long-term preservation of fault scarps may be, and (3) how much displacement at the surface reflects the structure and linkage of faults at depth (e.g. how does localised surface slip reflect co-seismic slip at depth. I think that the authors of this paper have done a sufficient job of discussing 2-3 in their study area, but (1) must be explained from the start of the paper. I did not realise until the discussion what timescale the scarps were interpreted to represent, but this is integral to the paper.

As this is an open review, I can state that I agree with and overlap with comments made by R1.

General comments

(1) The introduction needs to give the context of the timescales of displacement calculations. Do the scarps represent a single earthquake, multiple events, or geological offsets? This concept is only introduced in the discussion. I think the introduction should also include a brief description of the climatic setting and local potential for erosion of the scarps – how long should we expect them to persist (e.g. years, 100's, 1000's, kyrs?).

In general, after reading the discussion, it is still not clear to me how the measured scarp heights can be related to fault growth processes that occur over multiple earthquake cycles on geological (e.g. >10 kyr) timescales. Comparing the slip to length for a single event is reasonable, but if these scarps represent multiple earthquakes, then the ratio is not very meaningful unless the displacements represent the total geological offset (e.g. Cowie and Scholz, 1992). I think that the authors try to boil down some very complicated concepts by trying to relate the scarps to both a single event and the long-term geological evolution of the faults. Displacements in a single event can be highly variable along the strike of the fault, and may not always reflect the structure at depth. This may even be true over a few earthquake cycles. I think there is an interesting question to be addressed, that is, how do persistent variations in surface displacements (over a few EQ's) relate to the structure of the fault?

(2) I like the detailed discussion of the fault evolution and linkage, which is possible due to the high resolution analysis. How old are these faults, and what stage of growth are they in? The authors hint at this in the discussion by suggesting that the Thyolo and Muona faults are more mature than the Malombe faults, but could it just be that the Thyolo and Muona faults have experienced large earthquakes with more shallow slip, more recently?

(3) It would be helpful to include more introduction to how fault scarps are treated in the literature. In other studies, the crest and base of scarps are not used, but instead linear regressions are fit to offset features away from the scarp and projected to the fault. This method avoids problem of near-field scarp degradation and of proximal off-fault deformation (e.g. Nissen et al., 2014; Cowie et al., 2017). It is not clear why the crest-base method has been used in this paper.

(4) I think your algorithm essentially is a way of smoothing out the non-tectonic signals in the data, but then if the scarps are the same size as any non-tectonic signal, they will also be removed (and therefore no scarp will be found in that profile). It would be helpful to the reader to state this more plainly, if it is true? I also wonder if your algorithm is smoothing any off-fault deformation, which may be within 10's of meters of the scarp (e.g. Nissen et al., 2014; Gold et al., 2015).

(5) Overall – addressing the conclusion to the discussion on page 35, I think it is highly likely that the scarps represent multiple events given the extreme slip to length ratios you calculate, but that large magnitude earthquakes are definitely possible (even if they do not rupture the entire fault). Every time there is a new surface-rupturing earthquake, they seem to be even more complex (e.g. New Zealand and C Italy, 2016), so it is not surprising (though very interesting!) that there is some variability of the measured

scarp heights, which may be even more difficult to unravel if they represent several earthquakes but not the total geological history of the faults.

Specific comments

P2, Lines 20-28 hint at the uncertainties associated with DEMS, but these are not discussed formally in the paper. There could be at least one or two references in this paragraph on general DEM processing here or in the methods.

P 7, L 1: Change Media to Median

Pg 8, lines 7-9 – I'm not sure that I consider the profile to represent 'noise' in the data, as the features mentioned to cause noise are real features, which are being reliably recorded in the DEM. I think throughout the manuscript there needs to be a distinction between analytical noise in data and real features in the landscape that may cause ambiguity in the scarp height.

How is the dashed grey line calculated on figure 2a-c? 'By eye' to me, it looks like the manually picked crest of the scarp in 2a is too far to the right on the profile – the scarp looks like it starts at ∼140 m actually closer to the automatic picks.

Figure 4b is nicely summarised in the text on p12, lines 2-6. It would be helpful to do the same for figure 4a, given the amount of work that went into the synthetic tests.

P12, L24-25: Is this analysis in this paper (e.g. do you mean on the real data), or planned future work?

P 18 - It is impressive that scarps with heights less than 5 m can be identified in SRTM! I think this has good implications for using this tool to identify active fault scarps.

Discussion

P29, L18-23: I don't understand the logic behind the choice of average height for the Bilila-Mtakataka fault rupture (11 +/- 7 m). Why are the algorithm results not used? I understand that the rupture surveyed in this paper is not as complete as that from

Hodge et al., 2018? Why not combine the manual picks from the parts of the rupture that were surveyed in the previous work, with the automatic picks from this paper, to have the most data integrated into the average displacement?

P29, L26-27: The 2 km uncertainty seems arbitrary, how was this chosen?

P34, L3: this magnitude calculation is a maximum magnitude – this must be made clear. Smaller magnitude earthquakes can also occur (and also be devastating). I would use the word ‘estimated’ rather than ‘found’.

Figures

Problem with the labels in figure 2 d-f. The text references to the figure do not line up with what is written on the figure – and I cannot tell whether the labels on the figure actually correspond to what is plotted (e.g. in the text (e) is listed as a moving mean, bin width 20 m but plotted as Lowess 40 m – which is correct?). It looks like figure 2f is actually the 40 m bin (Lowess?) because it is smoother. This makes the whole comparison even more confusing for a reader who is not familiar with the different filtering methods.

It would also be helpful to show an example of the Savitzky-Golay filter in figure 2, as it is discussed in section 3 and more readers will not be familiar with this type of filter.

In figure 4, is it possible to also plot the actual fits of the models to the synthetic profile on the first panel – perhaps use the best fitting model, so the reader can get a more physical sense of how well the algorithm is working in the best case?

Figure 6: It would be nice to see an unannotated version of this in the supplementary material, so we can see the scarp. Or even better, use a dashed line or arrows to indicate the trace of the scarp in fig 6. We all like to see a nice fault scarp in a hi-res DEM.

Figure 10: Top of the figure, bold black line is labelled as Manual TanDEM-X 12-m (‘Chapter 3’) – should it be ‘Section 3’?

References:

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Gold et al., 2015. On and off fault deformation associated with the September 2013 Mw 7.7 Balochistan earthquake: implications for geologic slip rate measurements. Tectonophysics 660, p 65-78, doi: 10.1016/j.tecto.2015.08.019

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