Solid Earth Discuss.,
https://doi.org/10.5194/se-2018-38-RC1, 2018

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

Anonymous Referee #1

Received and published: 25 May 2018

This paper is an interesting one, which aims to produce an automated method of analyzing DEMs in order to extract metrics about fault scarps. The aim is a good one, in that it will allow scarp geometries to be analyzed with minimal interpretive input, and rapidly over large areas, which will be useful in terms of documenting and interpreting any along-strike variations that are present (e.g. due to segmentation). I have some comments that I hope will help to improve the clarity of the manuscript.

1. When reading through the discussion, what struck me is that these scarps probably formed in a small number (or possibly one) rupture, as noted by the authors. However, the along-strike patterns of scarp height are interpreted using terminology more

usually associated with multiple-earthquake displacement profiles (e.g. p22, line 21, "...separate faults that have since hard-linked and matured..."). It seems that in a single, or a few, earthquakes, there's not much chance to create new linkages and mature the system, so it would be helpful to discuss how the variations in scarp height might be interpreted on more of a single-event to few-event timescale. From this perspective, the paper could benefit from more discussion of the literature that deals with along-strike variations in how much earthquake slip is expressed on a single scarp, or distributed over a wide area (and therefore very hard to see, or invisible, in the geomorphology). There have been some nice studies of this (e.g. Milliner et al, GRL, doi 10.1002/2016GL069841, 2016; Wang et al, BSSA, doi 10.1785/0120120364, 2014). It would be good to see a discussion of the degree to which the scarp height variations may represent the geomorphological analogues of the variability of on- versus off-fault deformation seen in these recent earthquakes. (These are both strike-slip events, but the same may well be true for normal-faulting – it's worth discussing.)

2. Another point, which in some ways is very closely related to the one above, is the extent to which the scrap height variations may relate to the robustness/weathering of the scarp. Looking at modern normal faulting earthquakes, when we see them in the field the scarps degrade in a very laterally-variable manner over years and decades. In part this relates to whether they are in bedrock or alluvium, but there is also lots of variety within each lithology, possibly related to the degree of fault damage or consolidation of the rocks. A good example are the scarps from the 1981 Corinth earthquake sequence, some of which are still big and dramatic, and some of which have pretty much disappeared. It's hard to say how this might feed into the results of the authors, as there's nothing very quantitative known about the along-strike variations in scarp degradation from the recent events, but it's probably worth some discussion.

3. I liked the synthetic testing, but it would be good to make a couple of small alterations. One is that the examples shown in Fig 4 don't have any profiles that look like the real scarps in Fig 2 (I think because the wavelength and possibly amplitude of

non-scarp topography in the foot-wall isn't big enough). It would be good to see some synthetics that look more like fig 2b. The other thing I thought would be helpful would be to have a graph of the scrap characteristics of the cases where the algorithm failed. The pattern seems to be that the methods and parameters that give the most accurate results are also the ones where there are lots of failures by the algorithm to find scarps. It would therefore be good to know whether there is a systematic bias introduced into the results, based on which type of scarps do and don't get recognized by the algorithm. This discussion of whether any bias is introduced would then be useful for later in the paper, when it comes to interpreting the results from Africa.

4. In terms of the slip-length ratios, it's worth noting that the 2008 Yutian and 2006 Mozambique normal-faulting earthquakes both had a ratios at seismogenic depths of 1-2 x 10ˆ-4. These were mostly blind, but the principle of the biggest continental normal-faulting events we've seen having these ratios suggests that it's not out of the question that the scarps studied in this paper could be due to single events (if they ruptured to the surface). In general, I think this section (and the one about magnitudes) relies too much on the very sparse record of big modern normal-faulting events, either in Africa or elsewhere. They are rare enough (because of the long repeat times) that our modern record is extremely small, and it's an open question how representative it is.

More minor comments:

- There is a general feeling in the paper (mostly in the introduction) that soft-linkage is likely to halt earthquake ruptures. A famous example at Platea-Kaparelli in 1981 involved two fault segments rupturing in the same earthquake, with the deformation between them occurring by spatially-distributed minor normal-faulting, which in the topography would look like soft linkage. This is only one example, and there are others where soft-linkages have halted ruptures, but it would be good to mention somewhere just to keep things balanced.

- p3, line 5 – I think most people actually do this by fitting lines to slopes that are a safe

distance to either side of the scarp, and looking for an offset between them, rather than actually picking the top and bottom of the scarp itself.

- A small inset showing the location within Africa would be helpful for those not familiar with the area.

- P6 L5 – 'the' Pleiades DEM is introduced, but a few notes about its properties/construction would be handy. Also, the resolution is here given as 50 cm, but elsewhere (e.g. Fig 7) as 5 m.

- P6 L15 – a small sketch on one of the figures explaining the geometry described here would be useful.

- P6 L27 (and elsewhere) – I see what is being meant by 'signal-to-noise ratio', but I would describe it as something different (e.g. 'non-tectonic features in the DEM'), to avoid possible confusion with the noise level in the DEM relating to the data and analysis methods used (i.e. the measurement noise).

- P7 L14 – Is it that it 'better represents the average', or that it's using a different definition of 'average' (i.e. median rather than mean)?

- P8, top of page. Most readers probably won't know what G-S and Lowess filters are (including me until I looked it up). It would therefore be good to put some equations and explanation here, to help people see what the filters are actually doing.

- P11 L 17 – there is a mis-match between the labeling of 'moderate' and 'high' between this text and fig 4.

- Fig 4 and 5 – it would be nice to put a big bold line on for 0 misfit, to make it clear which parts of the plots represent the ideal result.

- Fig 10 – it would be good to show the actual values, as well as the moving average.

- Fig 12 – I struggled to see what the splitting of S3 into the three lettered sections was based on, so a clearer explanation would help.

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