Review for esurf

Overall I feel that many of the issues raised in the original reviews were addressed, and the work has improved since the first round. Several aspects of the analysis and approach were explained much more clearly and completely, making the work both easier to follow and more convincing. In addition, there is a more extensive discussion on possible sources of error that I think is quite important.

However, I think that there still needs to be a discussion on how craters erode and how they are identified. The conclusions of the authors critically depend on the ideas that

1) The lifetime of a crater is directly related to its initial depth and the regional erosion rate.

2) The crater record is complete above a certain size

3) They have been able to correctly characterize the incompleteness of the record below that critical size

4) There is no regional bias in crater discovery.

As most terrestrial geomorphologists (the main audience of this work) are not well versed in how craters are identified on earth and how they erode on earth, it is not feasible for the average reader to critically assess how reasonable the above assumption are. A discussion of how craters erode and how this will influence their discoverability based on methods that are used to find craters would be a critical element to support the author's conclusions.

More specific points:

Section 8.1: It has been pointed out to me that there is a large body of literature on how craters erode (Craddock et al, 1997, Craddock & Howard, 2002, Forsberg–Taylor et al., 2004, Howard 2007) that is completely ignored here. Given the incredible relevance of this process to the results laid out in this paper, I think this is a critical oversight, and while these references do not necessarily apply directly because they were developed for mars, they show both that a much more critical assessment of how craters erode is possible, and that people have already laid the groundwork for such an assessment.

Craddock, R. A., T. A. Maxwell, and A. D. Howard (1997), Crater morphometry and modification in the Sinus Sabaeus and Margaritifer Sinus regions of Mars, J. Geophys. Res., 102(E6), 13321–13340, doi:10.1029/97JE01084.

Craddock, R. A., and A. D. Howard, The case for rainfall on a warm, wet early Mars, J. Geophys. Res., 107(E11), 5111, doi:10.1029/2001JE001505, 2002.

Forsberg−Taylor, N. K., A. D. Howard, and R. A. Craddock (2004), Crater degradation in the Martian highlands: Morphometric analysis of the Sinus Sabaeus region and simulation modeling suggest fluvial processes, J. Geophys. Res., 109, E05002, doi: 10.1029/2004JE002242.

Alan D. Howard, Simulating the development of Martian highland landscapes through the interaction of impact cratering, fluvial erosion, and variable hydrologic forcing, Geomorphology, Volume 91, Issues 3–4, 2007, Pages 332-363, ISSN 0169-555X, https://doi.org/10.1016/j.geomorph.2007.04.017.

Lines 29-33: This argument only makes sense if craters are found by identification of altered rocks in the environment. I believe that this would be the case for very large craters, but how are smaller craters actually found? Are they identified initially by the rocks of the crater floor? Or are they initially found based on morphological clues that might appear to a nearby observer or in a DEM? In any case, this story also allows for a period of time where craters may be hidden by sediments deriving from their own erosion.

Section 8.2: Lines 27-29: This is cool! I very strongly think you should show the results from this analysis alongside the original results, at least for the overall erosion rate. Seeing agreement between the two size distributions will help the overall argument because it is easier to imagine that >6km craters are tied to regional erosion rates. Also, if the error from this method is extremely large, that will motivate the use of the less dependable but larger record of smaller craters. But why have you used a different I for the larger craters? For the original analysis, you use I=5e-5 for the entire record, including these large craters. Should you not keep the same value of I then for this secondary analysis?

8.3: "However, the uncertainty arising from this relationship should be clearly smaller than the statistical uncertainty." - I'm not sure this sort of statement is useful. It sounds a bit like an opinion, rather than a quantitatively supported statement. Without any numbers for statistical uncertainty or uncertainty in I we cannot evaluate such a statement. You should either attach numbers here, or drop this statement.

Section 8.4: End of the section: "However, this is unrealistic, and we expect the potential bias to be much smaller." - This statement again sounds more like an opinion. I think you could make a stronger argument with a statement like: "while relief has surely changed, orogens are long lived, lasting for tens of millions of years (citation - Whipple?) with response times to changing boundary conditions on the scale of 1e5-1e6 years (Whipple). Therefore we expect there to be a correlation between modern and past relief on the time scale of 10 mya.

Section 8.5: This is a really great example to describe the approach taken here!

Section 8.7: "Going a step beyond the occurrence of hiatuses in the erosional history discussed in the previous section, intermittent phases of sedimentation should also be taken into account as a potential source of errors" - phases of sedimentation are generally considered to be one of the main mechanisms for a hiatus in erosion.

Line 28-29: True, it faithfully records the longterm average rate of erosion, but it breaks the relationship between erosion rate and boundary conditions, so it would mess up any correlation between say climate or relief and longterm erosion rates.

Section 8.7: I really appreciate the attempt throughout this entire section 8 to account for error in the approach. However, I don't agree with the last paragraph at all. It seems to me that you strongly underplay the effect of missing craters in the record. This is also essentially a one way bias that only leads to an overestimation of erosion rates. Unless it is possible to identify something else as a crater by accident, craters can only be missed. Even the argument in section 8.7 allows for the idea that craters can be buried until a given date, which may not be today. It seems prudent to allow for the possibility that the incompleteness has been mischaracterized (given the few craters that available to characterize it) similarly it seems prudent to allow for a subset of craters that were eroded or erased much faster than the background average erosion rates would imply. I think to try to sum the sources of error and imply that you could have only underestimated erosion rates is coy. Error is error. In such an approach with so many unknowns over such a long period of time, I would not end with such a statement as you do here.