

Interactive comment on “Has erosion globally increased? Long-term erosion rates as a function of climate derived from the impact crater inventory” by Stefan Hergarten and Thomas Kenkmann

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Dear Liran Goren,

thank you very much for your thorough and constructive comments. I am quite sure that we will be able to submit an improved version of the manuscript soon.

... Reading the abstract, I expected the analysis to be neat and simple, reading the rest of the text, I found it to be neat and very far from simple.

Both reviews have indeed convinced me that there are several points that are not as simple as I thought. The problem is that the approach differs much from all other approaches and thus requires a quite specific mathematical / statistical treatment. In particular, the terrestrial crater record is so sparse that we have to take the big gun in order not to be killed by the statistical variation in the numbers of craters. So we will give our best to explain the methodological aspects more clearly in order not to lose the majority of the readers too soon.

I identify five major methodological hurdles (the first two are probably the most important). Even if they can be dismissed, clarifications in the text are essential.

1. *Could it be that craters are inherently more erodible than their surrounding due to the higher relief of the crater rim and the higher erodibility of the impact-induced breccia in and around the crater? If this is the case, then the time that it takes to erode a crater significantly underestimates the time that it takes to erode the surrounding material. This may introduce a strong bias toward the high erosion rates. The authors acknowledge (p. 6 lines 26-27) the effect of the local crater topography, but it is not further developed into an estimation of this potentially large bias.*

I think this will indeed be the case for most of the craters, but it will not introduce a major bias. In the first phase, the elevated crater rim will perhaps be eroded more rapidly, and the crater could be filled by a lake. As erosion in the surrounding region proceeds, the outlet of the river will incise, and the lake sediments will be eroded. Finally the lower bound of the altered rocks at the crater floor will be reached, and at this point the erosion of the crater floor should be tied by the rivers in the domain, so that the point where the crater cannot be detected or proven any more should indeed be defined by the large-scale erosion of the region.

2. *Browsing through the supplementary material, it appears that in some cases, the*

statistics involve very small numbers, even in the erosive terrains. For example: 4 craters in cold orogens, 0 in cold igneous provinces, 4 in temperate shields, 2 in temperate orogens, 0 in tropical orogens, and so on. This raises the questions of: how do the authors estimate erosion rates in climatic-geologic terrains with 0 craters? Also, what is the validity of the estimation when the number of craters is so small? For the latter question, even a single unidentified/hidden crater (or a recently eroded crater) can have a significant impact on the statistics and the estimated erosion rates.

At this point both reviewers got stuck, so it is probably the point with the highest need for a better explanation. In the first step, relief was assumed (and roughly verified) as the primary control on erosion, and a linear relationship was established (for the predominantly erosive provinces). Then a subdivision into the climatic zones was performed in order to take into account climate as the secondary control, but keeping the linear relationship between relief and erosion rate in each zone. As a consequence, only 5 independent parameters are fitted from the crater record (the erosion rate per relief = erosional efficacy s of each climatic zone). These parameters follow Poissonian statistics per climatic zone (not per province). So statistics indeed relies on only 4 craters for the polar tundra class (reflected in very high error bars in Fig. 3), but this class does not contribute very much to worldwide erosion, while the numbers are higher in the other classes.

- 3. The authors discuss the possibility of terrains moving in between climatic zones during the relevant timescale. This discussion, however, is not sufficiently developed. For example the half-life is estimated for the different climate zones, but when a continent or a climate zone shifts, then this affects not only the erosion rate but also the half-life. For example, if a continent has shifted from cold to temperate to tropic zones (i.e., India or Africa), then the half-life of the last climate zone should be even shorter.*

This is in principle true and applies to both the erosional efficacy (and thus the

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erosion rates) and the half-lives. Both estimates refer to the part of the crust that corresponds to the respective climatic zone today. For your example this means that our estimates for the tropical zone do not completely reflect tropical conditions, but are a mixture of tropical with some contribution of cold climate during history. And as you mention, the contribution from the cold zone is even an average over a longer time span than the main contribution (tropical zone). But in principle the only consequence is that the statistical distribution of the half-lives within each climatic zone shown in Fig. 8 (exponential distribution) may not be completely random, but may have a systematic spatial variation.

I would say the more important aspect in this context is the effect on the erosion rates themselves. Here we will add an explanation (probably a section in the appendix) what happens if the subdivision into climatic zones does not reflect the climatic conditions over the geological history properly. In this case, the estimates for the chosen zones are closer to each other than they would be if the choice of the zones was perfect, and the worldwide mean erosion rate will be underestimated (closer to the harmonic mean), but never be systematically overestimated.

4. *On the same note, how can the effects of changing relief during the relevant timescale and the effect of quaternary glaciation be quantified?*

As far as I can see, this could be the only source of significant systematic errors (in relation to the statistical uncertainty that is already quite high as shown in Fig. 3) that could be realistically expected. If the ratios of the average relief have changed significantly over the history, the erosional efficacies will indeed be biased. However, the effect finally cancels when moving from the efficacies to erosion rates. Nevertheless, the erosional efficacy would indeed be overestimated if the relief in a climatic zone was significantly reduced recently, e.g., by glaciation. Trying to avoid such effects was indeed the main reason for taking the relief over quite large spatial windows, so that, e.g., the shape of individual

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valleys has no effect.

5. *The manuscript presents several biases for the estimation of the erosion rates, but their magnitudes are, in most cases, not evaluated. Even if currently it is not possible to evaluate the magnitudes, maybe the authors can explain what are the missing data and understanding that will allow their estimation in the future.*

Yes, it is indeed difficult to quantify the biases or uncertainties, but nevertheless we will discuss them in more detail in a revised version.

- (a) Uncertainties arising from the depth-diameter relation and from the crater production function are probably quite low and negligible in relation to the statistical uncertainty.
- (b) The completeness of the available crater record may be a more critical point. Any systematic incompleteness of the record linearly transforms to an over-estimation in the erosion rates. In our EPSL paper we have only shown that, if there is a significant incompleteness, it must extend uniformly over the entire diameter range above 6 km and concluded that this is unlikely.
- (c) The linear relationship between relief and erosion rate might even be the most critical point. According to the relationship between lifetime and erosion rate, most of the information is drawn from regions with low to moderate erosion rates, while regions with high erosion rates also contribute much to worldwide erosion. If the erosion rate increases more than linearly with relief in reality, we will underestimate the worldwide erosion rate and vice versa. We could indeed add some estimate on the magnitude of this potential bias.

Some arguments, particularly those that are used for describing biases are quite hard to follow. For example:

1. *Page 3. Lines 12-14. The point is clear, but readers might appreciate a simple artificial example.*

Indeed, we will combine this with the discussion of the subdivision into subdomains.

2. *Page 5. Lines 18-22. The text is too complicated.*

Hm ... ok

3. *Page 6. Lines 24-33 and 34-35. Hard to follow.*

Hm ... ok

4. *It is hard to interpret fig. 6. Consider adding an inset, where the y-axis is in percentage. (This might help the 75%-25% discussion).*

Good idea, we will do this unless we find an even better solution.

Editing issues:

1. *Sources for biases are presented throughout the manuscript in different sections. Organizing them in dedicated subsections might be helpful.*

I think it would indeed be a good idea to do this or even to make one dedicated section on this topic.

2. *Missing commas after opening clauses.*
3. *Refer to appendices using the word 'appendix.'*
4. *Explain the vertical dashed black line in fig 6 in the captions.*

Should be no problem to fix these points, thanks!

Despite these substantial comments, and even if the methodology and the conclusions remain controversial, I believe that as long as all the uncertainties and biases are presented and discussed in the text (including the abstract and the summary), the

manuscript could be an important addition to the global erosion rates discussion. I would have certainly liked to read it for its original methodology.

Thanks! I hope that the readers of the final papers will also like to read it.

All the best,
Stefan

Interactive comment on Earth Surf. Dynam. Discuss., <https://doi.org/10.5194/esurf-2018-59>, 2018.

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