

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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The manuscript uses the world distribution of impact craters to estimate erosion rates. This is achieved by the assumption and the leading equation that describe the preservation potential of craters as a function of the erosion rate. The technique is used for evaluating erosion rates as a function of relief, erosion efficiency as a function of climatic zones, and global mean erosion rates. One of the main declared goals of the manuscript is to address the timely question of the effect of climate cooling on the mean erosion rate. Different studies came up with opposite conclusions concerning

the effect of cooling, and therefore, the motivation behind the study is well-justified.

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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.

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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.

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.

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.

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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.

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

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.

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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.
2. Page 5. Lines 18-22. The text is too complicated.
3. Page 6. Lines 24-33 and 34-35. Hard to follow.
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).

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Editing issues:

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

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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.

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

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