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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

Anonymous Referee #1

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I was invited to review this manuscript by the editors of ESurf. My expertise lays mostly in erosion processes themselves, though I have quite a bit of familiarity with the debate on apparent vs. real increases in erosion rates in the past, and I am also familiar with the key techniques used to determine erosion rates in the present and past (sedimentary fluxes, terrace preservation, cosmogenic nuclides -detrital or otherwise, thermochron, and sedimentary sequences) and their potential timescale biases. Finally I have only passing knowledge on crater formation and evolution as well as impact frequency. Though the results laid out here don't seem to depend critically on a detailed

understanding of these process, I am the first to admit a lack of knowledge in this area. Overall I feel well equipped to offer a well-rounded and critical review of this paper.

The authors propose to use the crater record on Earth as an independent measure of longterm erosion rates. This is a technique that has been applied on other bodies in the solar system to infer rates of surface activity. The major hurdles to applying this technique on Earth are the rapid rate of surface activity leaving only a fraction of the craters found on other bodies, and the influence that the atmosphere has on incoming objects. Given the previous work of these authors, it is clear that they are well positioned to overcome these challenges.

It is surprisingly challenging to determine erosion rates in the past, even with course resolution over large areas and time periods, and there is a major lack of consensus in the geomorphology community on this topic. A new technique, independent from existing ones is clearly welcome, and there is no question that this work is relevant. Overall, I find their approach to be original and exciting, and I support the effort wholeheartedly. My impression is that the work of the authors is thourough, careful and sophisticated when it concerns the frequency-magnitude distribution of impact craters and the completeness of the record. However, I feel that there are some shortcuts made to establish a connection between the crater record and the longterm erosion rate on the surface of the Earth.

Given the complexity, longevity and significance of the debate on past erosion rates, it is my opinion that there is an extra burden on the authors, and as it stands I do not recommend this paper for publication. However, I don't think the issues are insurmountable. The authors must do one of two things to convince me that this work should be published. They can either recast their results as very preliminary, and do more work to cast doubt on their own conclusions, describe all potential sources of error more thouroughly, and most importantly, discuss their results more conservatively. Or they can work to establish more convincingly some of the key assumptions that they make, and expand on how the processes that they depend on might actually function, hopefully through a more complete literature search and some simply modelling.

I have 4 major issues with this work:

1. First, there should be more work to discuss how the crater record reflects erosion rates over long periods of time:

– In particular, is this approach really invulnerable to time-scale biases? Just stating that the record is spatially integrated doesn't convince me that there is no time-scale bias.

– What happens when erosion rates are spatially variable? This is dealt with later I know, but could be discussed more directly and clearly. The discussion of harmonic versus arithmetic mean is unclear and should be reworked for clarity.

– What happens when erosion rates are temporally variable?

– What happens if there are hiatuses that reflect a heavy tailed distribution as discussed in Ganti et al. 2016 - what if the hiatuses are spatially coherent? This probably isn't relevant for the global estimation, but what about when the authors divide the earth into more regions than there are craters in the record they use in the final analysis?

– What if erosion rates themselves follow a heavy tailed distribution, as discussed in Schumer et al., 2009?

2. There should be much more discussion about how craters actually erode away:

– Are the key processes the same for craters of all sizes? The largest craters modify the crust, leaving a mark in the rock over large areas, and it is clear that we will probably find them unless the crust is eroded to nearly the depth of the crater, or unless they are completely buried. Is this true of smaller craters? I would imagine that craters on the order of hundreds of meters to a few kilometers might be hidden more easily. Perhaps hillslope diffusion rates or soil production rates are the critical rates.

– Similarly, do small craters need to be completely eroded to disappear, or is it sufficient

to just erode them partly? This could lead to an overestimation of the longterm erosion rates. Either modeling or field results, potentially taken from the literature could be a major help here.

– Is there a regional bias that could effect the record of smaller craters? For example, could the North American ice sheets repeated advance and retreat have been sufficient to erase visible traces of craters below a certain size? Could something like this be responsible for the observed effect of climate on erosion rates through time? A better discussion of how craters of different sizes evolve and erode could guide the thinking here.

– Although I appreciate the urge to restrict the analysis to erosion only regions, over the timescales involved it seems to me that there may be no erosion only regions. There should be at the very least a larger concession to the error that sedimentation could introduce (see discussion for example in Willenbring et al., 2010).

3. The results of the climatic regions is interesting, but I am quite skeptical of this approach overall:

– Eastern Canada, Scandanavia and Australia seem to account for a majority of the craters used in this analysis (47 out of 77 or so). Can the authors bring in other lines of evidence to support the idea that these regions have been eroding more slowly than the rest of the Earth's surface for the last 10-100 Ma?

– Have the authors checked that there is no correlation between vegetation cover and crater frequency. Many of the places with many craters (northern canada, scandanavia and australia) are also regions that tend to have short or sparse vegetation.

– Though it is my impression that the authors have a good grasp of the appropriate statistics for this problem, I was plagued with questions about the role of chance while reading this paper. According to the authors, there are only 188 craters that have been found on earth, and of those only 112 are used in the analysis. Further, only 77 craters (as far as I can tell) fall in the erosion-dominated regions, though the authors then divide this into 89 sub-regions. My understanding then is that many of these subregions would have either 0,1 or at most 2 craters, and often the erosion rates will be optimized for the observation of finding no craters in the relevant region. How much error is introduced simply by the extraordinary rarity of having a significant event in a given region. According to Bland & Artemieva 2006, the expected time between craters > 500m is 20,000 years (I know the authors use 250m as the lower limit, but Bland and Artemieva give only the value for 500m craters). Assuming that impacts are truly randomly distributed on Earth, and that the surface area is 500,000,000 kmˆ2, then it seems to me that the mean expected wait time between impacts >500m in a region of 1,000,000 kmˆ2 would be on the order of 10 Ma. The expected time between craters > 500m for the smallest region they use would be greater than the age of the Earth (approx. 6 Ga). This temporal variability becomes significant when small regions are considered, and seems to me could lead to very large error bars on estimated erosion rates. Further the global erosion rates for the Polar Tundra, Temperate and Tropical regions are based on what appears to be only 4, 7 and 8 craters respectively. How does the estimated erosion rate change if there are one or two more (or fewer) craters in each climatic region?

– I think that a simple toy forward model could be extremely convincing here. It would be simple to build a model that randomly places craters down with the expected size and frequency on a large area with heterogeneous erosion rates that are known. Using the techniques applied here, the authors should show that the right answer can be recovered reasonably well when the crater record is a sparse as it is on Earth. They could further use the model to investigate the effect of temporally variable erosion rates on the inverted erosion rates.

– If the timescales of averaging are really approaching 100 Ma, what does it mean to divide the world into climatic zones? Over such timescales, not only did climate change significantly, but the crust itself was rearranged, moving craters from one climatic region

to another. The authors mention this, but these are described as effects that can blur the climate boundaries. I feel they don't acknowledge that plates can move 1000s of km and climate can change radically in such a timeframe.

– I think that the authors should consider removing this analysis overall, and focusing on the global rates, which are more convincing and also more relevant to the debate that they are addressing. However, a forward model would still be valuable!

4. My final issue concerns figure 9. I think that this figure is not an equal comparison of the two techniques. The marine sediment derived erosion rates are divided into different time periods while the crater-derived erosion rate is integrated over the history of the Earth. I think the authors miss what would be the single most significant test of the time-scale-bias-invulnerability of the crater-derived erosion rates that they claim. Because they have a record of craters with a wide range in sizes and because larger craters reach further back into time, it should be possible to subdivide their record in time instead of in space as they do for the climatic regions. Showing that the record reflects similar erosion rates for different size-groups of craters, and therefore over different time periods, would be a powerful piece of evidence in favour of their argument as well as a more accurate comparison of the crater record with the marine sedimentary record.

Details:

– Page 2, Lines 15-20: I think this is a bit of an unfair interpretation of previous work. High relief and high topography are both often the result of high uplift rates, and it is not surprising that they are correlated. Additionally, if relief is indeed the first order control on erosion rate, as you reasonably argue, then any comparisons of the influence of climate and lithology will have to take that into account. It would be necessary for example to show that the deviation from the expected linear trend is controlled by one of these two effects, or that for a given relief or slope the erosion rate is secondarily controlled by one of these factors. Studies such as Portenga and Bierman do not take this into account. Some other studies that do find a clearer influence of climate (Ferrier et al. Nature 2013, Moon et al. Nature Geoscience 2011). I think it would be fair to use this reference to point out that climate is not a first order control on erosion rates, but not to imply that climate does not have the influence that we expect, as currently seems to be the implication.

– Page 2 line 30 to page 3 line 1: I think it would be important to express what I is and where it came from. I am guessing that $I = \int_{D_{ea}}^{D} \dot{N}'(D')H(D')dD' + H_{max}\dot{N}(D_{ea})$. I felt that I had to go back and read your previous paper before I understood equation 1, but it isn't referenced here. Even more critical would be an in depth discussion of the sources and magnitudes of error on I. What are the reasonable ranges of error. How much could it vary by? Perhaps with the least squares optimization it's a bit more complex, but my impression is that if I were 20% lower, the overall erosion rate would also be 20% lower. That seems like it would be a big deal.

– Page 3 line 2-3: This one line is a crux point in the paper, and I think is passed over a bit rapidly here. It is true that spatially averaged measurements will be less susceptible to the effects of temporal hiatuses and incomplete records that plague point measurements. However, there are other measures of erosion rate that are spatially integrated. The work of Herman et al, 2013 for example is based on thermochronological data which is integrated across tens of kilometers. More relevantly, Willenbring et al., 2010 mention 4 causes of the time-scale bias for sedimentary records some of which might matter in the case of craters, and they further show 4 data sets, several or all of which are spatially averaged, yet exhibit time-scale bias. More care should be given to demonstrate that the crater record is immune to time-scale biases.

– page 3, line 11-14: I don't think this point is made very well here. I guess you are trying to explain the difference between the old estimate of 59 m/Ma based on spatially homogenous erosion rates, and the new estimate of 78 m/Ma based on heterogenous rates? I think you should try to be a bit more clear on why exactly you are bringing in the harmonic and arithmetic means. Also, are you completely sure this is the correct

argument? What about in places where the erosion rate is based on the observation of no craters. Since you have no crater, you have no timescale, so it is not necessarily 'how long it takes to erode a given amount of material'.

– page 4, line 14 and other places: I think calling s the 'erosion rate per mean relief' is pretty awkward, I would jump straight to erosion efficiency as you eventually call it later in the manuscript

– Page 5, line 27: 'This result already suggests that erosion rates in the past might be much higher than those obtained from preserved sediments.' I feel that this point is way too strongly emphasized given the lack of discussion about potential sources of error in your estimate. I would remove it.

– Page 7, lines 15-19: I think this argument makes good sense for the timescale associated with the global erosion rates. However, for climate zone erosion rates, it seems to me that the timescales of the slower regions, e.g. the cold climate zone will be longer. This makes it harder to accept the idea that the climatic regions have any meaning over the integration timescales.

– Page 8, lines 5-7: Can you add some references for the widely accepted trend.

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