

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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Received and published: 23 October 2018

Dear Reviewer,

thank you very much for your thorough and constructive comments and for obviously spending a lot of time with our manuscript. It is always worrying if a reviewer even with an obviously solid theoretical background missed some of the main points of the approach. So we have to accept that we should explain the theory in more detail in order not to lose the majority of the readers too soon.

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 following fundamental points more clearly in a revised version, namely

- why a subdivision into distinct zones (here the climatic zones) is necessary in order to overcome the shortcomings arising from the harmonic mean,
- why the details of the subdivision (here the relationship between the present-day climatic zones and the paleoclimate) are not very important, and why even a completely wrong, subdivision of Earth's surface does not make any damage except that we got stuck at the harmonic mean and thus still underestimated the global erosion rate,
- how the parametric approach with the relief serves as a backbone to avoid the problem with the very small numbers in each province,
- and that all potential sources of error in sum indicate that the global erosion rate is still rather underestimated than overestimated, providing further support for our result that long-term global rates have been higher than previously assumed.

Following the suggestion of the second reviewer we will also introduce a distinct section addressing the potential sources of errors.

In detail:

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.*

Our reasoning about a potential sampling bias did not refer to time-scale biases, but only to the potential bias by an uneven spatial location of sampling points, e.g, due to correlations between the number of outcrops and the topography. However, I think that taking values that are integrated over large spatial scales indeed reduce the potential time-scale bias (see the following points).

- *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.*

Seems that understanding the bias by taking the harmonic mean instead of the arithmetic mean if the erosion rates are spatially variable is indeed more difficult than I thought. I think we can explain it using an example in combination with the discussion of the subdivision into climatic zones.

- *What happens when erosion rates are temporally variable?*
 - Nothing if the distribution of the erosion rates has a finite mean value and if there are no intermittent phases of deposition.
 - Erosion rates are underestimated (but never overestimated!) if there are intermittent phases of deposition.
- *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, . . .*

In the model proposed by Ganti et al. 2016, the observed time-scale basis does not really arise from the heavy-tailed distribution of the lengths of the hiatuses at least qualitatively. I tested this model with exponential and

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uniform distributions instead of the truncated Pareto distribution (which is also not heavy-tailed) and also discussed the results with Vamsi Ganti. The effect itself occurs in principle for all distributions as soon as you assume that all measurements where the erosion rate is zero are excluded. If you assume that zero erosion rates are also measurable and include them in the measurement, the effect completely vanishes for all hiatus distributions. Obviously impact craters do not mind if there was no erosion during the last years before present, so that our approach is definitely robust against the type of time-scale bias addressed by Ganti et al. 2016.

... 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?

This specific situation has no meaning at all; the problem that you are probably referring to already occurs if any region has zero crater count. Then the estimated erosion rate is infinite and thus destroying the whole estimate. Even if there are just a few craters in any region the error increases extremely due to the Poissonian statistics. This is the reason why only a small number of domains can be considered as completely independent (here the climatic zones). Relief as the primary control is included by a parametric approach in the form that the erosion rate is proportional to the relief, which means that all provinces belonging to the same climatic zone have the same ratio of erosion rate to relief. As shown in Appendix A, each climatic zone (not each province) is characterized by Poissonian statistics with 4 to 33 usable craters.

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

Schumer et al. (2009) consider both heavy-tailed distributions of the hiatus lengths (in contrast to Ganti et al. 2016) and of erosional peaks. But in my opinion they do not consider a bias (due to measurement) but a real dependence of the mean erosion rate on the considered scale. If the hiatus

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lengths follow a heavy-tailed distribution, the erosion rate will tend towards zero in the limit of infinite time interval. If the erosional peaks follow a heavy-tailed distribution, the erosion rate will tend towards infinity in the limit of infinite time interval. In both cases, erosion rate is no longer a well-defined term.

Going back to the results of our EPSL paper, the existence of such a scale dependence could even be refuted if the erosion rate was not spatially variable. A nonlinear scaling relation between time interval length and erosion rate would destroy the linear relationship between crater depth and lifetime, and this would be visible in Fig. 1 of the EPSL paper. However, as soon as the erosion rate is spatially variable, the effect may be blurred, so that we cannot refute the existence of a dependence of the erosion rate on the time scale. However, the effect should be the same (if present at all) for all methods or be weaker for methods averaging over large spatial scales such as our approach. I would therefore guess that a nonlinear scaling with time scale does not exist at large spatial scales, and that our approach is well-suited to avoid any time-scale bias.

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.*

This aspect was indeed briefly discussed in our EPSL paper on the completeness of the crater inventory. Following our key assumption that a crater remains visible until the regional erosion depth reaches the depth of the

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deepest altered rocks we found a really good fit above 6 km diameter, but the real record rapidly drops below the prediction at smaller diameters. Potential reasons are:

- (a) The record below 6 km diameter could still be incomplete (what was unfortunately considered as the key point in several press releases).
- (b) The protection of Earth from small impacts by the atmosphere is still underestimated in the model of Bland and Artemieva.
- (c) Small craters erode (or become invisible) faster than predicted by the regional erosion rate (the point you mention).

In our EPSL paper we even found an approximation for this apparent incompleteness, however, without being able to explain it physically or to decide which of the three potential reasons applies here. The value I we used for the craters above 0.25 km in diameter includes this correction. This means that the apparent incompleteness of small craters does not introduce a systematic overestimation of the erosion rates. We only need to assume that the lifetime of small craters is still inversely proportional to the regional erosion rate, which is admittedly not completely clear, but does not really have a serious influence on the result. In order to test how much the small craters affect the results we applied the same method to the craters larger than 6 km some time ago, and we found no significant effect on the results except for a larger formal statistical uncertainty due to the smaller number of craters. For this reason we decided to include the small craters in the analysis (with the empirical correction).

- *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.*

This point should be covered by our discussion of your previous point.

- *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.*

We expected such variations during our work on this topic as there was some hope to be able to predict where undiscovered small craters could be found. However, we did not find any large regions where the number of small craters is either exceptionally high or exceptionally low in relation to the number of large craters. So we would tentatively claim that there is no such effect.

- *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).*

Yes, there are indeed two potential effects.

- (a) If a region assumed to be erosional is a region of deposition over long times, craters are lost, so that the erosion rate is overestimated.
- (b) Phases of intermittent sediment deposition increase the lifetime of craters and thus result in an underestimation of the erosion rate.

In sum of both I would expect the second effect to be stronger, so that the erosion rate will be rather underestimated.

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

- *Eastern Canada, Scandinavia 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?

At least for Australia we considered this in our first study on this topic (Lunar and Planetary Science Conference, 2014). Kohn et al. (2012, doi:10.1046/j.1440-0952.2002.00942.x) obtained a very low mean erosion rate of about 10 m/Ma over the entire continent at the 300 Ma scale from thermochronometry. As Fig. 4 shows, our estimate is quite close to this value for large parts of Australia. Taking the average over the entire continent we obtain about 26 m/Ma due to some regions with high relief, but taking into account the spatial variation and the different time scales I think that our estimate for Australia and also for other regions with a not too low number of craters should be quite ok.

- *Have the authors checked that there is no correlation between vegetation cover and crater frequency. Many of the places with many craters (northern Canada, Scandinavia and Australia) are also regions that tend to have short or sparse vegetation.*

How should this be checked formally? There is definitely a correlation between climate and vegetation, and nobody will seriously question a correlation between climate and erosion and thus between climate and crater record. A potential bias could only be detected in the inventory of small craters in relation to large craters. However, as mentioned above we did not find any evidence for such a bias so far.

- *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-*

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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. . . .

This is obviously the problem of not getting the key point of the method correctly. As mentioned above, relief being the primary control is included by a parametric approach in the form that the erosion rate is proportional to the relief, which means that all provinces belonging to the same climatic zone have the same ratio of erosion rate to relief. 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). As shown in Appendix A, each climatic zone (not each province) is characterized by Poissonian statistics with 4 to 33 usable craters.

. . . 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 km², then it seems to me that the mean expected wait time between impacts >500m in a region of 1,000,000 km² 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. . . .

This looks reasonable to me, but what is the consequence? Using our estimated erosion rates we find that highest expected number of craters among all regions is 16.1 (with 13 craters in reality), while the lowest expected num-

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ber of craters among all regions is 0.0005 (with 0 craters in reality). We can, of course, include these numbers in the supplementary data sheet in order to make the numbers more convincing. However, as the statistics rely on the numbers per climatic zone, the numbers have no immediate meaning.

... 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?

Yes, the crater counts follow Poissonian statistics, and the errors (70 % and 95 % confidence intervals) arising from this are given as error bars in Fig. 3. Not a big surprise that these error bars are quite large for the three climatic zones mentioned above, and also not a big surprise that these Poissonian statistics are the main source of uncertainty in the entire analysis.

- *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. ...*

Not really. If you refer to different climatic zones, they are independent of each other. If you refer to different provinces within a climatic zone, they are constrained by the parametric relationship between relief and erosion rate. This means we already know the ratios of the erosion rates from the relief and only estimate one parameter. As mentioned above, this estimate is controlled by Poissonian statistics, and I do not think that it is very convincing to simulate Poissonian statistics with a numerical model.

... They could further use the model to investigate the effect of temporally variable erosion rates on the inverted erosion rates.

Yes, but it is already clear that the obtained mean erosion rates are an average with a sensitivity decreasing exponentially through time into the past (Fig. 8). So the result would be that a high recent erosion rate has a stronger effect on the estimated mean rate than a high erosion rate in the past. But this specific model would not yield much more information.

- *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.*

Admittedly, this point was discussed in our manuscript only very briefly (page 5, lines 1-10). Starting from the point that the method applied to a single domains always yields a harmonic mean instead of the arithmetic mean value we always obtain a systematic underestimation as soon as the rates are spatially variable. A subdivision into a reasonable number of sub-domains (so that the number of craters per domain is not too low) is the most convenient way to ship around this problem. As relief being the primary control is already covered by a parametric relationship (erosion rate proportional to relief), climatic zones are a somewhat natural choice.

From theory: If the erosional efficacy (erosion rate per relief) was constant within each climatic zone and also constant through time we would arrive at the correct result with regard to both the relationship between the climatic zones and the worldwide average (except for the statistical variation). This is probably not true. The other extreme would be completely random sub-domains without any systematic differences in erosional efficacy. Then we would arrive at the same erosional efficacy on average within each domain (the harmonic mean over the respective domain). In total we would simply

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get stuck at the underestimation by the harmonic mean; the “wrong” subdivision would bring not progress at all, but also make no damage. Your argument is referring to the situation where the climatic zone make some sense, but they are probably not the perfect subdivision. The consequences are that

- the estimated erosion rates refer to the spatial domains corresponding to the actual climate zones (so not, e.g., to what is today arid climate over Earth’s history),
- compared to the real erosional efficacies of the considered types of climate, the variation between our zones is smaller, and
- there is still some underestimation of the worldwide mean erosion rate.

I think these aspects could be explained in detail with a simplified consideration of two subdomains in the appendix, which would probably make much more sense than the toy model mimicking Poissonian statistics suggested above.

- *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!*

Clearly not! We need any kind of subdivision of Earth’s surface in order to avoid (or at least reduce) the underestimation of the mean erosion rate due to the harmonic mean. So the option would only be hiding the results referring to the climatic zones, and this makes no sense in my opinion.

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*

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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.

This is basically true, but unfortunately it is practically impossible. I tried this some time ago. If the erosion rate was spatially homogeneous, then a characteristic time scale could be assigned to each crater depth. Then the problem would still be that all craters are sensitive to a time interval from the present, so that the inversion of the crater-size distribution is already somewhat unstable. But as the erosion rate varies by orders of magnitude due to the variation in relief alone, there is no realistic chance to invert the crater-size distribution with regard to time. And I agree that Fig. 9 is not a perfect representation of the two different methods, but I have no better idea and think the bar with a fading background color indicating the decreasing sensitivity is not too bad.

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.

It was not our intention to say that climate has no effect on erosion; the message should have been that other controls (mainly relief) may shadow the effect of climate at large scales, so that it is quite difficult to quantify the effect of climate. When writing the paper we originally decided not to include the two papers as they refer more to the regional scale than to the worldwide scale. We will include them and write the discussion about shadowing the climatic effect by topography a bit more precisely.

- *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. ...*

Your guess is correct; we will explain this a bit more in detail and reference the equation.

... 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.

Your guess with the effect of a 20% variation is correct. However, the value of I originates from the crater production rate and the depth-diameter relation of craters (both being quite well constrained and described in our EPSL paper). I

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am quite sure than the uncertainty in I is much smaller than the statistical errors due to the small Poissonian crater counts already included in Fig. 3.

- *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.*

This is partly true, but in principle this aspect has been discussed above.

- *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? ...*

Definitely!

... 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'.

Also discussed above.

- *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

Good idea! The only thing I am not completely sure is whether we should use efficiency instead of efficacy. We could indeed see relief as a resource and interpret *s* in the sense how efficiently the climatic zone generates erosion from using relief. However, I still feel that efficacy could be more appropriate than efficiency.

- *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.*

The phrase “already suggests that” was chosen taking into account that the timescale has only been roughly estimated at this point and that there was no thorough discussion of the potential errors. I think it should stay there as a preliminary conclusion at the end of the section.

- *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 is true, and the time scale for the slowest zone is even given in line 14 on the same page, while the time scales for the other zones are given in Fig. 8.

... This makes it harder to accept the idea that the climatic regions have any meaning over the integration timescales.

Although there was a bit more shift over the longer time scale, this does not affect the meaning of the climatic zones as discussed above.

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

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Some references were given in the introduction, but I do not mind rewriting this sentence.

All the best,

Stefan

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

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