Notes

- Referee comments are marked with red color.
- Responses are identical with those in my original responses and are displayed in black.
- References to changes in the manuscript are marked in blue color; line numbers refer to the version with marked changes (latexdiff).

Reviewer 1

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.

Extended and hopefully improved explanations at several places in Sect. 2, 3 and 4; removed Appendix A1 as it is no longer necessary with the new explanations.

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

New Sect. 8.

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).

  Page 3, lines 19–22 and Sect. 8.6.

- 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.

  Page 3, lines 23–31.

- 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.

  Sections 8.6 and 8.7.

- 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 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.

  Section 8.6.

  ... 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.

Page 4, lines 7-11, page 5, lines 16–19 and page 5, lines 26–19.

- 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 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.

  Section 8.6.

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 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 in include the small craters in the analysis (with the empirical correction).

Sections 8.1 and 8.2.

- 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.

  Section 8.1.

- 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.

  Section 8.2.

- 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.

  Section 8.7.

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 Earths surface for the last 10-100 Ma?

At least for Australia we considered this in out 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.

No change to the manuscript as I think that it makes no sense to pick individual regions where our estimate matches the data from other studies particularly well.

- 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.

  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.

  Section 8.2.

- 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. ...

  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.

  Page 4, lines 7-11, page 5, lines 16–19 and page 5, lines 26–29.

  ... 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 number 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.

No change to manuscript.

… 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.

Page 4, lines 7-11, page 5, lines 16–19 and page 5, lines 26–29.

- 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.

  No change to manuscript.

  … 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.

  No change to manuscript.

- 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 dont 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 subdomains (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 subdomains 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 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.

Section 8.5.

- 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.

  No change to manuscript.

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.

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.

Page 9, lines 6–17; Fig. 9 (now Fig. 8) unchanged.

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 an write the discussion about shadowing the climatic effect by topography a bit more precisely.

  Page 2, lines 14–16 and 22–25.

- 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 isnt referenced here. …

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

  Page 3, lines 9–11.

  … 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 its 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 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.

Section 8.3.

- 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.

  Section 8.6.

- page 3, line 11-14: I dont 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!

  Page 3, lines 23–31.

  … 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, lines 7-11, page 5, lines 16–19 and page 5, lines 26–29.

- 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.

  Adjusted throughout the manuscript starting from page 6, line 5.

- 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.

  No change to manuscript.

- 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.

  No change to manuscript.

  ... 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.

  Section 8.5.

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

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

  Page 10, lines 26–27.

Reviewer 2

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 tho lose the majority of the readers too soon.

Extended and hopefully improved explanations at several places in Sect. 2, 3 and 4; removed Appendix A1 as it is no longer necessary with the new explanations.

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.

Section 8.1.

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 accoont 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.

   Page 4, lines 7-11, page 5, lines 16–19 and page 5, lines 26–29.

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 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.

Section 8.5.

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 valleys has no effect.

   Section 8.4.

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 overestimation 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.
   Section 8 and subsections.

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.

   Page 3, lines 23–31.

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

   Hm . . . ok

   Page 7, lines 19–25.

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

   Hm ... ok

   Removed (page 8, line 29 to page 9, line 9.) and replaced by a hopefully better discussion in Sect. 8.4.

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.

   Done.

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.

   Section 8 and subsections.

2. Missing commas after opening clauses.

   I checked the text and indeed found some.

3. Refer to appendices using the word appendix.

   Fixed (page 7, line 29); the other appendix has been removed.

4. Explain the vertical dashed black line in fig 6 in the captions.

   Added to the legend instead of to the caption.

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

Has erosion globally increased? Long-term erosion rates as a function of climate derived from the impact crater inventory

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Abstract. Worldwide erosion rates seem to have increased strongly since the beginning of the Quaternary, but there is still discussion about the role of glaciation as a potential driver and even whether the increase is real at all or an artefact due to losses in the long-term sedimentary record. In this study we derive estimates of average erosion rates on the time scale of some tens of million years from the terrestrial impact crater inventory. This approach is completely independent from all other methods to infer erosion rates such as river loads, preserved sediments, cosmogenic nuclides and thermochronometry. Our approach yields average erosion rates as a function of present-day topography and climate. The results confirm that topography accounts for the main part of the huge variation of erosion on Earth, but also identifies a significant systematic dependence on climate in contrast to several previous studies. We found a fivefold increase in erosional efficacy from the cold regimes to the tropical zone and that temperate and arid climates are very similar in this context. Combining our results to a worldwide mean erosion rate, we found that erosion rates on the time scale of some tens of million years are at least as high as present-day rates and suggest that glaciation has a rather regional effect with a limited impact at the continental scale.

1 Introduction

The origin of the apparently huge increase of worldwide erosion in the late Cenozoic era is one of the major puzzles in the younger geologic history of our planet (Molnar and England, 1990; Zhang et al., 2001; Molnar, 2004; Willenbring and von Blanckenburg, 2010; Herman et al., 2013; Wang et al., 2014; Marshall et al., 2015; Willenbring and Jerolmack, 2015). As high temperatures facilitate weathering of rocks, the cooling climate during the Cenozoic era should rather result in decreasing erosion rates, bringing Pleistocene glaciation as a major driver of erosion into discussion (Yanites and Ehlers, 2012; Brocklehurst, 2013; Egholm, 2013; Pedersen and Egholm, 2013; Koppes et al., 2015; Herman and Champagnac, 2016).

However, most of the knowledge about the apparent worldwide increase relies on estimates of long-term erosion rates from preserved sediments in the oceans (e.g., Wilkinson and McElroy, 2007). Based on Sadler's theory (Sadler, 1981) addressing the scale dependence of sedimentary records, the existence of a worldwide increase has already been questioned by Willenbring and von Blanckenburg (2010). In their study the theoretical arguments were supported by Beryllium isotope ratios revealing no systematic variation in the overall sediment delivery rates during the last 12 Ma. On the other hand, a recent study on thermochronometric data not depending on the long-term sedimentary record has revealed a strong increase at least in some mountainous regions with high erosion rates during the last 10 Ma (Herman et al., 2013). However, potential systematic errors in thermochronometry have been discussed in the previous years (Valla et al., 2010; Willenbring and Jerolmack, 2015), and the ~~a~~ worldwide increase found by Herman et al. (2013) has recently been questioned by Schildgen et al. (2018).

Worldwide present-day erosion rates have also been addressed in several studies. However, all approaches suffer from the need to upscale point data, leading to a large variation in the estimates of the worldwide mean rate (see, e.g., the compilation by Willenbring et al., 2013). As an additional source of uncertainty, an increasing portion of the eroded sediments is trapped in artificial reservoirs today (Syvitski et al., 2005).

As topography, climate, and lithology are the main controls on erosion, there have been several approaches to quantify the contribution of these components. Concerning the variation over Earth's surface, topography has the strongest influence. The seminal study of Ahnert (1970) suggested a linear dependency of the erosion rate on mean relief (difference between maximum and minimum elevation) even without any correlation to precipitation. Later studies used either relief, slope or modal elevation and also obtained a linear or almost linear increase of the erosion rate with the respective geomorphic property (for a comparison see Summerfield and Hulton, 1994).

~~The relationship to climate was found to be less systematic~~Although the effect of climate on erosion has been addressed in several publications at least indirectly (see references above), the number of studies finally arriving at a clear relationship between long-term erosion and climate seems to be limited. In a study on organic carbon fluxes, Ludwig and Probst (1996) also estimated sediment fluxes into the oceans and found a strong correlation with climate. According to their results, the wet tropic climate zone contributes about 44 % to the worldwide sediment supply, while the tundra and taiga zone contributes only 5 %, although both cover the same area on Earth in total. In contrast, the presumably most comprehensive compilation of millennial-scale erosion rates (Portenga and Bierman, 2011) involving cosmogenic nuclide data from almost 1600 drainage basins and outcrops even yielded an unsystematic dependence on climate~~in contradiction to the widely accepted increase of erosion with temperature and~~, presumably because the dominant effect of topography shadows all other influences. A weak effect of climate was also found by Riebe et al. (2001a) even at smaller scales. In turn, recent studies (Moon et al., 2011; Ferrier et al., 2013) have at least confirmed the correlation between precipitation and erosion rates that is implicitly assumed in all models of fluvial erosion within regions with high contrasts in precipitation.

2 Deriving erosion rates from the impact crater inventory

In planetary geology, the inventory of impact craters provides the most valuable data for unraveling the geological history (e.g., Neukum et al., 2001; Stöffler and Ryder, 2001) The terrestrial inventory, however, has not been exploited systematically beyond the research on impact processes themselves, probably due to its small extent compared to other planets and to its uncertain completeness. However, recent studies suggest that the terrestrial crater record is by far not as incomplete as it was presumably assumed in the past. Taking into account the age distribution of the Earth's crust, it was ~~recently~~ found that the inventory of the craters at least 85 km wide may already be complete (Johnson and Bowling, 2014). A subsequent study (Hergarten and Kenkmann, 2015) also considering the consumption of craters by erosion even revealed no evidence for any incompleteness in the crater record above 6 km diameter exposed at the ice-free part of Earth's land surface~~and~~. This study also quantified the potential incompleteness in the diameter range from 0.25 km to 6 km.

Using the presumably best estimate of the terrestrial crater production rate available (Bland and Artemieva, 2006), it was found that the expected ~~number n~~ density N (number per area) of craters with a diameter of at least 0.25 km ~~(taking into account the incompleteness) in a given region of area A~~ at an erosion rate r is

$$\cancel{n}N = \cancel{\frac{AI}{r}}\,\frac{I}{r} \tag{1}$$

with a constant $I = 4.94 \times 10^{-5}\ \frac{\mathrm{m}}{\mathrm{Ma\,km^2}}$ ~~. Equation 2 can be used to estimate the long-term mean erosion ratefrom the number of impact craters . As this method directly yields some spatially~~ (Hergarten and Kenkmann, 2015, Eq. 9) . The value of I takes into account the crater production rate, the depth-diameter relation of craters and an estimate of the potential incompleteness of the inventory in the diameter range from 0.25 km to 6 km.

If erosion is spatially homogeneous in the considered domain, Eq. (1) immediately predicts the expected number n of craters as

$$n = AN = \frac{AI}{r} \tag{2}$$

where A is the size of the domain. For heterogeneous erosion, Eq. (1) yields

$$n = \int N dA = I \int \frac{1}{r} dA. \tag{3}$$

Inverting this relationship allows for an estimation of some spatially and temporally averaged erosion rate ~~, it avoids any sampling bias occurring in other methods based on point-like measurements.~~ from the number of impact craters in a given region.

At this point it is noteworthy that the spatial average is not an average over the locations of the existing craters, but over the entire area. In other words, the approach does not only derive information on erosion rates from regions where craters are, but also from crater-free regions. It therefore avoids the potential sampling bias due to an uneven distribution of locations that might occur in all methods where erosion rates measured at points or over small areas must be transferred to large areas.

In turn, ~~an inevitable statistical uncertainty arises from the low number of~~ the occurrence of r in the denominator in Eq. (3) reveals that the number of craters in a given region does not yield the arithmetic mean erosion rate (as it is relevant, e.g., for the sediment yield), but the harmonic mean rate. The latter is always lower than the arithmetic mean, and the discrepancy increases with increasing spatial heterogeneity. Let us illustrate the difference by a simple example (which will be revisited in Sect. 8.5). If the entire surface of Earth consisted of two parts of equal sizes where one part has a high erosion rate of $r_h = 120$ m/Ma and the other a low erosion rate $r_l = 30$ m/Ma, the arithmetic mean rate would be 75 m/Ma. The harmonic mean erosion rate would, however, be only $\left(\frac{1}{2}\left(r_h^{-1} + r_l^{-1}\right)\right)^{-1} = 48$ m/Ma and thus be more than one third lower than the arithmetic mean rate. Taking this discrepancy into account, it can be expected that the harmonic mean rate for the entire ice-free land surface of $r = 59$ m/Ma obtained by Hergarten and Kenkmann (2015) significantly underestimates the arithmetic worldwide mean.

Overcoming this limitation is one of the main goals of this paper. Subdividing the total surface into a sufficient number of domains and then averaging over these domains seems to be a straightforward idea, but is limited by the low number of impact craters exposed at Earth's surface. At the time of the original study, the Earth Impact Database (http://www.passc.net/EarthImpactDatabase/) comprised 188 terrestrial craters in total with only 112 of them exposed at the surface and wider than 0.25 km. While two more craters have been added to the database until now, the number of relevant craters is still ~~112, so that the value of I given above is still valid. Due to this low total number , the approach is most suitable for large regions.~~ 112. ~~Application to the entire ice-free surface of Earth yields a worldwide mean erosion rate of $r = 59$ m/Ma (Hergarten and Kenkmann, 2015) .~~ While this number in total provides a moderate statistical error of about 10% (standard deviation of Poisson distribution), the statistical errors rapidly increase if the number of craters per domain decreases. In particular, crater-free domains would cause serious problems as the estimated erosion rate would be infinite (with an infinite error, too). Therefore, a more sophisticated approach is required; it will be explained in the following sections.

~~Although this method is not susceptible to sampling errors, spatial heterogeneity introduces a bias since the approach is based on lifetimes of craters instead of erosion rates, reflected in the occurrence of r in the denominator of Eq. 2. In other words, it is measured how long it takes to erode a given amount of material and not how much material is eroded in a given time span. As a consequence, applying Eq. 2 to a region with a non-uniform erosion rate yields the harmonic mean rate being always lower than the arithmetic mean, resulting in an underestimation of the mean rate. In turn, craters~~ The original estimate of $r = 59$ m/Ma contains a second source of potentially large systematic errors. Craters are not only consumed by erosion, but may also be buried by sediments. As local sediment accumulation rates in a crater may be much higher than regional erosion rates, the total rate of crater consumption may be significantly larger than the mere erosion rate. Thus, sediment deposition in parts of the considered domain leads to an overestimation of the erosion rate. So the ~~estimate $r = 59$ m/Ma~~ original estimate contains two sources of systematic errors in opposite directions.

3 The influence of topography on erosion

Topography contributes the largest part to the spatial variation in erosion. Ahnert (1970) suggested a linear dependency of the erosion rate on the mean relief. In contrast to the local slope often used in the context of erosion at small scales (Summerfield and Hulton, 1994; Montgomery and Brandon, 2002; Whipple et al., 2013; Willenbring et al., 2013), the relief is more robust against the resolution of the considered digital elevation model (DEM). In this study we measure relief over squares of 10 km edge length using the worldwide ETOPO1 DEM with a mesh width of one arc minute and also verified that our results basically persist for squares of 5 km and 20 km edge length as originally used by Ahnert (1970). In contrast to the widely used definition of relief based on circles, the relief taken over squares can be computed efficiently for all points of a large domain.

In order to verify the relationship between relief and erosion rate on large scales, we first subdivide the ice-free land surface into the six basic types of continental crust (shield, platform, orogen, basin, igneous province, extended crust) defined in the world map of the main geological provinces provided by the USGS (http://earthquake.usgs.gov/data/crust/type.html). Figure 1 relates the mean apparent erosion rates (rates of crater consumption) estimated from Eq. ~~2~~ (2) for each of the types of crust to their average 10 km relief. The three crustal types shield, orogen, and igneous province expected to be predominantly erosive regimes differ strongly in their mean relief, but show a strikingly linear relationship between the rate of crater consumption r and the mean relief Δ,

$$r = s\Delta. \tag{4}$$

The three other types, platform, basin, and extended crust, are characterized by much higher rates in relation to their mean relief, suggesting that deposition of sediments significantly contributes to the consumption of craters here. We therefore consider only the three predominantly erosive crustal types in our analysis and assume a linear relationship between relief and long-term erosion rate.

If we forget for the moment that Eq. (4) is applicable to large scales only, inserting it into Eq. (3) allows for estimating s from the total number of craters n according to

$$s = \frac{I}{n} \int \frac{1}{\Delta} dA. \tag{5}$$

With regard to the applicability of Eq. (4) at large scales only, the integral should be replaced by a discrete sum over a finite set of (sufficiently large) subdomains,

$$s = \frac{I}{n} \sum_{i=1}^{k} \frac{A_i}{\Delta_i}, \tag{6}$$

where k is the number of subdomains, and A_i and Δ_i are the size and the mean relief of each subdomain, respectively.

As the key point of this consideration, the estimate of s is still obtained from the total number of craters following a Poissonian distribution and does not rely directly on the number of craters in each subdomain. Thus, the parametric relationship between relief and erosion rate allows to take the heterogeneity arising from the strong worldwide variation in relief into account without increasing the statistical errors from the limited number of craters on Earth.

4 The influence of climate on erosion

~~Beside topography climate is the second major influence on erosion . In the following~~ While the strong effect of relief on erosion rates can be taken into account using the parametric approach discussed in the previous section, it is not possible to proceed in this direction for the further factors controlling erosion. This would require a quantitative relationship between erosion rate and any property where data are avaliable at the entire surface (e.g., precipitation), whereas pure correlations do not help. Thus, a further reduction of the underestimation arising from the harmonic mean can only be achieved by a subdivision of Earth's surface into independent domains where the value of s differs among the domains. As the number of craters per domain is lower than the total number, we reduce the systematic error for the price of increasing statistical uncertainty then. Thus, the domains should be chosen in such a way that they capture a major part of the variation of erosion going beyond the effect of topography, but the number should not be too high.

As a tradeoff between the expected effect on erosion and the number of domains we consider the primary classes tropical (A), arid (B), temperate (C), cold (D), and polar tundra (ET) of the Köppen-Geiger classification of the recent climate (Peel et al., 2007) shown in Fig. 2. The class polar frost (EF) was omitted as it primarily consists of ice-covered areas. ~~Each of these classes is then subdivided into the~~ A value of s is then assigned to each of the climate zones by applying Eq. (6). The parameter s is a lumped parameter summarizing all influences on erosion going beyond the topography. As variations in lithology should not be significant at the large scales considered here, s can be seen as a measure for the erosional efficacy of the respective climatic regime, so that we well use this term throughout the paper.

For applying Eq. (6) to each of the climate zones, the respective domain must be further subdivided into subdomains capturing the variation in relief reasonably well. For this we use the six main types of crust mentioned above where only the three predominantly erosive types are used for estimating erosion rates. ~~This~~ Considering unconnected parts of the same type of crust as separate subdomains, this yields a subdivision of the predominantly erosive provinces into 89 subdomains (13–22 per climate zone) with sizes from about 1600 km^2 to about 11 million km^2 (for details see supplementary material).

~~As shown in **??**, the erosion rates per mean relief s can be estimated for each of the climatic classes using a maximum-likelihood approach according to~~

$$s = \frac{I}{n}\sum_{i=1}^{k}\frac{A_i}{\Delta_i}.$$

~~Here, k is the number of subdomains within each climate zone, A_i and Δ_i are the size and the mean relief of each subdomain, and n is the total number of craters in the climate zone.As variations in lithology should not be significant at the large scales considered here, s can be seen as ameasure for the erosional efficacy of the respective climatic regime.~~

The resulting erosional efficacies of the climate zones are shown in Fig. 3a. In contrast to ~~previous studies (Ahnert, 1970; Riebe et al., 200 our erosion rates per relief s (Fig. 3a) show a~~ some of the previous studies (Ahnert, 1970; Riebe et al., 2001a; von Blanckenburg, 2006; Port we found a clear systematic dependence on climate, at least for those classes primarily defined by temperature (A, C, D, ET). While the two cold Köppen-Geiger classes D and ET are very similar ($s = 0.13\,\mathrm{Ma}^{-1}$), the mean erosion rate per relief in the tropical zone ($s = 0.62\,\mathrm{Ma}^{-1}$) is almost 5 times higher. With $s = 0.30\,\mathrm{Ma}^{-1}$, the temperate class is close to the (geometric) mean of the two extremes. This clear trend goes along with the increase in both temperature and precipitation from polar to tropical regions.

The result for the arid zone, $s = 0.30\,\mathrm{Ma}^{-1}$, suggests that that the erosional efficacy of the arid climate is as high as that of temperate climate. This may be surprising as the arid zone is defined by low precipitation in relation to the temperature and covers a wide range of temperatures. However, the major part of the worldwide arid range is characterized by high temperatures (Köppen-Geiger classes BWh and BSh), so that the mean rate of chemical weathering should indeed be high here. But as water is the main agent for mechanical erosion and sediment transport, the result that the high temperatures are able to compensate the low precipitation compared to the temperate climate is still surprising.

In this context, the time scale of the considered mean values must be taken into account, too. Based on the estimated lifetimes of the considered impact craters, a time scale of 10–100 Ma was estimated (Hergarten and Kenkmann, 2015). Mean temperatures have varied over this time span, accompanied by changes in overall precipitation, so that the climate classes primarily defined by temperature have shifted with the coldest and warmest classes extending or shrinking. Furthermore, continents have ~~also~~ moved on this time scale. ~~Thus, each of our estimates isa mean value over a range in climate which is wider than the respective Köppen-Geiger class, so that the systematic increase in erosional efficacy from polar to tropical climate may be even larger than the fivefold increased revealed in this study. However, this simple argument does not necessarily hold for the arid class that might temporarily have been much smaller than today. In this sense, our~~ So it should be mentioned that our estimate s is, strictly speaking, not the actual erosional efficacy of the present-day climate, but the erosional efficacy of the part of Earth's surface belonging to the considered climate zone measured over a long time span into the past. As discussed in Sect. 8.5, the real differences in erosional efficacies of the climatic zones may be higher than suggested by our study. This may also apply to the surprisingly high erosional efficacy of the arid zone. Our results do not refute the importance of water for erosion, but may tentatively suggest that the present-day arid zone may have been wetter than today in the past.

The clear relationship between ~~mean erosion rate per relief~~ erosional efficacy and climate (Fig. 3a) is slightly blurred after computing absolute erosion rates using the mean relief (Fig. 3b). The mean relief of the predominantly erosive provinces is highest in the temperate zone, $\Delta > 500$ m, while it is lower than 300 m in both the tropical and the arid zone and on an intermediate level ($\Delta \approx 400$ m) in the two cold regimes. As a consequence, the variation in the absolute erosion rates shown in Fig. 3b is smaller than the variation in s, and the temperate zone is characterized by a high mean erosion rate almost catching up with the tropical zone.

Figure 3c shows the extrapolation of the results for the entire ice-free surface including the types of crust excluded not taken into account so far (platform, basin, and extended crust). For the extrapolation we assumed that ~~the relationship between relief and erosion found for the predominantly erosive provinces holds there, too. This procedure is appropriate if the not predominantly erosive provinces consist of erosive parts and parts~~ parts of these provinces are erosive with the erosional efficacy of the respective climate zone, while the rest is dominated by sediment deposition~~with~~. Assuming that regions of sedimentation have a very small (strictly speaking, zero) relief~~. Otherwise, the erosion rates given in Fig. 3c may be slightly biased towards high values.~~, the erosional efficacy is also valid for these mixed zones and thus for the entire climate zone (including the regions of sedimentation). Depending on the climate class, the mean erosion rates decrease by 13 % to 32 % due to the lower mean relief of the extrapolated provinces. However, the results are qualitatively similar to those obtained for the predominantly erosive provinces.

The area-weighted mean over the five climatic zones (Fig. 3c) yields a worldwide mean erosion rate of $r = 78$ m/Ma (107 m/Ma for the predominantly erosive provinces) with 95 % confidence limits of 52 m/Ma and 116 m/Ma (see ~~??~~Appendix A). Our result is almost 40 % higher than the mean Pleistocene (2.58–0.01 Ma b.p.) erosion rates of $r = 56$ m/Ma obtained from preserved sediments (Wilkinson and McElroy, 2007). The latter value is even close to our lower 95 % confidence limit, and all known values for earlier periods of Earth's history are even lower. This result already suggests that erosion rates in the past might be much higher than those obtained from preserved sediments. We will return to this point after considering the time scale addressed by our approach more thoroughly (Sect. 6).

5 The spatial distribution of erosion on Earth

Figure 4 shows a world map of the estimated erosion rates using the 10 km relief on a $0.1° \times 0.1°$ lattice and the values s of the respective climate zones. The dominance of topography over climate is immediately visible. While the mean relief amounts to 260 m, the maximum relief is 5887 m, which is more than 20 times larger than the mean relief. In contrast, the erosional efficacy s differs only by about a factor of 5 between the warmest and the coldest climate classes. However, very high erosion rates above 1000 m/Ma occur over considerable areas only in combination of tropical climate and high relief. The largest domain with estimated erosion rates above 1000 m/Ma is found in New Guinea.

Figure 5 compares the estimated erosion rates with the present-day erosion erosion rates published by ~~(Wilkinson and McElroy, 2007)~~ Wi on the study of Ludwig and Probst (1996). As this study focused on organic carbon, specific bioclimatic zones were defined instead of the Köppen-Geiger climate classes used in our study. Therefore a direct comparison based on climate zones is not possible, so that a comparison by latitude remains as the most convenient approach.

In general our estimates show a much more homogeneous distribution on Earth than the estimates of the recent erosion rates. The quite inhomogeneous distribution of the latter is reflected in a strong asymmetry between the two hemispheres, a strong decrease towards the polar regions and a pronounced peak at 20°N. However, the smaller variation of our results is not surprising since ~~the climatic zones may have movedin the past as discussed in Sect. 4.~~ our results are an average over a long time span where climate has changed and even continents have moved.

As shown in Fig. 6, the contribution of the area with an erosion rate greater than r can be approximated well by an exponential distribution $C(r) = 0.25\exp(-\frac{r}{200\,\mathrm{m/Ma}})$ at high erosion rates above 250 m/Ma. This means that the area on Earth with an erosion rate greater than r decreases by about 40 % if r increases by 100 m/Ma. Qualitatively the same behavior was found for soil losses at the plot scale, but with a decay constant about 5 times smaller (Wilkinson and McElroy, 2007). Even more striking, there is a significant deviation from the exponential decay at erosion rates below 250 m/Ma. The exponential part covers only 8 % of the total ice-free land surface. This steeper decrease in the cumulative distribution at smaller erosion rates indicates that smaller areas with small erosion rates contribute much more to the total area than the exponential tail. However, when considering the contribution to the worldwide erosion, a different behavior is observed. Here, the contribution of the large area with small erosion rates is not so high. Using our estimate of the worldwide mean erosion rate of 78 m/Ma, the data reveal that only about 25 % of the total land surface have an erosion rate above the mean, but these 25 % contribute about 75 % to total erosion. This 75 to 25 relation describes a more uneven distribution than Willenbring et al. (2014) obtained (about 70 to 30), but it is less inhomogeneous than the 80 to 20 relation often referred to as Pareto's principle in many contexts.

~~At this point the question may arise whether the spatial distribution of the impact craters on Earth might cause a systematic error.~~

6 The time scale of the terrestrial crater inventory

As the lifetime of a crater ~~is inversely proportional to the erosion rates, the majority of craters is found in regions with rather low erosion rates, which is confirmed by the erosion rates at the 77 craters used in the analysis shown in Fig. 6. In order to avoid~~

~~a bias by the local topography of the craters, we used the~~ at a given erosion rate depends on its size, the number of craters of different sizes should reflect the mean erosion rate ~~of the respective province instead of the estimate at the location of the crater itself. For the temperate zone, the median erosion ratesof the existing craters is 61 m/Ma. Repeating the analysis of Fig. 5 for this climate zone we found that 60of the area have a higher erosion rate , which means that 50of the craters are in these 40of the area with the lower erosion rate, and 50in these 60of the area with a higher erosion~~ over different time intervals. We might therefore think about an inversion approach using the crater inventory as a function of the crater size for deriving time-resolved erosion rates. ~~This distribution seems to be not very asymmetric, but the 60of the area with a higher erosion contribute more than 92to the total erosion. As a consequence, half of the craters in the temperate zone are located in a part of the climate zone contributing less than 8to the total erosion.~~

~~In view of this result, the estimate of~~ Alternatively, we could use the very good fit of the ~~worldwide erosion rate strongly relies on the assumed and to some extent verified linear relationship between relief and erosion rate. However, to our knowledge all studies in this context either found linear or slightly convex relations between morphometric parameters and erosion rates . It we assumed a convex dependency of the erosion rate on the relief,~~ inventory assuming a constant erosion rate obtained by Hergarten and Kenkmann (2015) as evidence for a constant erosion rates over millions of years. However, the ~~estimated erosion rates at large relief and thus the worldwide mean erosion rate would even increase. This result would even strengthen our finding that the worldwide erosion rates on the million year scale were higher than suggested by the sedimentary record in the oceans~~spatial variation of erosion rates immediately tears down these such ideas. Mainly due to the variation of relief, erosion rates vary by orders of magnitude. This variation blurs the relationship between crater size and lifetime, so that no serious information about the temporal variation in erosion rates can be gathered. The obtained erosion rates remain temporal mean values, and we can only try to specify the time interval of averaging or, more precisely, the sensitivity of the mean value as a function of the time before present.

7 ~~The time scale of the terrestrial crater inventory~~

~~According to Fig. 7, the estimated lifetimes of the considered cratersare in the range from 1 Ma to 1000 Ma~~The sensitivity of our estimated erosion rate with regard to time can be assessed using the ages of craters. As available information about the age of individual craters is often vague or only provides an upper or a lower limit, we compute the lifetime of each crater from the ratio of its depth and erosion rate. Crater depths are inferred from diameters (for details, see Hergarten and Kenkmann, 2015) . In order to avoid a bias by the local topography of the craters, we used the mean erosion rate of the respective province instead of the estimate at the location of the crater itself. We then use half of the estimated lifetime as an estimate of the age. Figure ~~8~~ 7 gives the cumulative distribution of these ages. This distribution can also be interpreted as a sensitivity with regard to the time before present as it states how many of the existing craters would be affected by a change in erosion rate at a given time. It is immediately recognized that these sensitivity functions roughly decrease exponentially with time for all considered climatic zones as well as worldwide.

In order to obtain a robust estimate of the decay constant τ, we use the time where the area below the curve from 0 to τ amounts to a fraction $1-\exp(-1) \approx 63\,\%$ of the total area. This results in a minimum value of $\tau = 13$ Ma for the temperate zone and a maximum value of $\tau = 70$ Ma for the cold climate zone. So it is not possible to define a distinct time window of sensitivity for our method, but find that the sensitivity exponentially decreases with time before present. As the worldwide mean erosion is dominated by the temperate zone and the tropical zone showing the smallest decay constant, we suggest $\tau = 13$ Ma as a conservative estimate. So our approach covers a time span characterized by a cooling climate, but without any ~~fundmental~~ fundamental changes in the location of the continents on Earth and in the spatial distribution of the orogens.

7 Has erosion globally increased?

Taking into account an exponentially decreasing sensitivity with $\tau = 13$ Ma, Fig. ~~9~~ 8 compares our result on the worldwide long-term mean erosion rate with previous results. The green area represents our result of $r = 78$ m/Ma with the 70 % confidence intervals. The decreasing opacity visualizes the exponentially decreasing sensitivity with $\tau = 13$ Ma.

Except for the average Pleistocene (2.58–0.01 Ma b.p.) erosion rate, our result is significantly higher than the estimates derived from preserved sediments for all epochs. All these estimates are even much below our lower 95 % confidence limit of 52 m/Ma. This result supports the hypothesis of Willenbring and von Blanckenburg (2010) that the erosion rates obtained from preserved sediments are much too low.

As a reference value for the worldwide present-day erosion rate we use the values compiled by Willenbring et al. (2013). The studies starting from 1950 show a high variablity from 35 m/Ma to 218 m/Ma. The mean value of these 31 studies is 76 m/Ma, and the standard deviation is 37 m/Ma, i.e., about 50 % of the mean value. The standard deviation reduces if we consider only those 16 studies not older than 1975. We then obtain a mean value of 63 m/Ma with a standard deviation of 15 m/Ma. As these values do not change much if we reduce the data set further, we take $r = 63 \pm 15$ m/Ma as a reference value for the present-day erosion rate. As it is recognized in Fig. ~~9~~8, the uncertainties in our long-term estimate and in the present-day erosion rate are similar, and the recent erosion rate is slightly below the lower bound of our 70 % confidence interval. This means that we could reject the hypothesis of equal erosion rates at about 15 % error level, but clearly not at 5 % error level following the widely used practice in statistics. So our long-term estimate is even higher than the present-day erosion rates, but the uncertainty in the data does not allow the conclusion that the long-term erosion rates were indeed higher than the present-day rates, although this would be consistent with the retention of sediments in artificial reservoirs and with the ~~widely accepted trend of~~ tendency towards decreasing erosion in a cooling climate due to lower rates of weathering.

8 Potential systematic errors

The statistical variation arising from the sparse impact crater inventory on Earth already included in Fig. 3 is the most obvious source of uncertainty in our approach. However, there are also several potential sources of systematic errors that will be discussed in the following.

8.1 Impact craters as passive erosion markers

Our approach hinges on the idea that impact craters can be used as passive markers of large-scale erosion, although they may have a strong influence on local landform evolution in particular in an environment dominated by fluvial erosion (Wulf et al., 2019) . In the first phase, the elevated crater rim will be eroded more rapidly than the surrounding region, and the crater could be filled by a lake depending on the climate. But when erosion in the surrounding region proceeds, the outflow of the river will incise into the rim, and the lake sediments will be eroded. When finally the lower bound of the altered rocks that are used for proving the impact origin of the crater will be reached below at the crater floor, the erosion of these rocks should be tied by the rivers in the domain. Thus, the point where the structure cannot be proven as an impact crater any more should indeed be defined by the large-scale erosion of the region rather than the local processes in and around the crater.

However, the crater may indeed be invisible in the meantime due to local landform evolution, so that it may either not be listed as a crater exposed to the surface or might remain completely undetected. This loss of craters in the record may be relevant in particular for small craters and is addressed in the next section.

8.2 The completeness of the crater inventory

Estimated erosion rates and erosional efficacies are inversely proportional to the expected number of craters in our approach. Therefore, any incompleteness in the crater inventory directly leads to an overestimation of the erosion rate. Our paper on the crater inventory (Hergarten and Kenkmann, 2015) concluded that there is no evidence for a systematic incompleteness in the inventory above 6 km diameter. Comparing the real crater inventory with the prediction of a simple model based on erosion and age of the crust, it was shown that any significant incompleteness must cover the entire range of crater sizes above 6 km diameter. As small craters should remain undiscovered more easily than large craters, this was considered to be unlikely. Although the lack of newly detected craters listed in the Earth Impact Database supports this result further, there are probably still some undiscovered craters in the relevant range, leading to an overestimation of the erosion rates. However, the systematic error arising from an incompleteness in the range above 6 km in diameter should be much smaller than the statistical uncertainty.

The diameter range between 0.25 km and 6 km is more critical. Here the real record rapidly drops below the prediction at decreasing diameters. The discrepancy may be either due to an incompleteness in the record, but in principle it could also be possible that the protection of Earth from small impacts by the atmosphere is still underestimated in the model of Bland and Artemieva (2006) . The value $I = 4.94 \times 10^{-5}\ \frac{\mathrm{m}}{\mathrm{Ma\,km^2}}$ used in this study already includes an empirical correction for this apparent incompleteness in the diameter range between 0.25 km and 6 km, so that it does not lead to a systematic error in itself. However, if it arises from an incomplete record, the incompleteness must be random and should not systematically differ among the climatic zones. Comparing the numbers of small craters to the number of large craters, we did not find any systematic variation. If the incompleteness is related to the potential invisibility discussed in Sect. 8.1, the lifetime of the craters must still be inversely proportional to the regional erosion rate. This seems to be reasonable, but cannot be proven as long as there is no model for this process.

As these considerations cannot exclude any bias arising from taking into account craters smaller than 6 km, we performed the same analysis for the craters larger than 6 km (with $I = 2.99 \times 10^{-5} \frac{\mathrm{m}}{\mathrm{Ma\,km^2}}$), but did not encounter any significant effect on the results except for a larger formal statistical uncertainty due to the smaller number of craters.

8.3 The value of the parameter I

Similarly to the potential incompleteness of the crater inventory, the parameter I occurring in all our calculations has an immediate effect on all estimated erosion rates. According to Eq. (9) in Hergarten and Kenkmann (2015) , it relies on the rate of crater production as a function of the diameter (Bland and Artemieva, 2006) and on a relationship between the average depth down to which the impact origin of a crater can be proven by altered rocks as a function of the diameter. The crater production rate should be well constrained except for the potential uncertainty at small diameters discussed in Sect. 8.2. The relationship for the depth used by Hergarten and Kenkmann (2015) was based on a limited set of data, so that it is probably more uncertain. However, the uncertainty arising from this relationship should be clearly smaller than the statistical uncertainty.

8.4 The role of the relief

As the lifetime of a crater is inversely proportional to the erosion rate, the majority of craters is found in regions with rather low erosion rates and thus with low to moderate relief. In turn, erosion is in sum dominated by a rather small part of the surface with high relief as illustrated in Fig. 6. Therefore, the relationship between erosion rate and relief (Eq. 4) plays a central part in transferring information from the crater inventory to high-relief regions where the record is sparse. Although the linear relationship defined in Eq. (4) is consistent with early work of Ahnert (1970) and with the data presented in Fig. 1, this relationship may be a major source of systematic errors.

In order to assess the influence of the linearity of the relationship, we assume a more general power-law relationship of the form

$$r = s\Delta^b \tag{7}$$

and repeat the analysis for scaling exponents b in the range $0 \leq b \leq 2$. The result is given in Fig. 9.

The erosion rates in general increase with increasing scaling exponent b. This is the expected behavior as the mean erosion rate is somewhat tied to regions with low relief due to the higher data density, while the estimate at high relief relies more on the relationship between relief and erosion rate. The potential bias is highly asymmetric; the worldwide mean erosion rates would be more than three times as high as our estimate for $b = 2$, while the minimum erosion rate occurring at $b = 0.31$ (57 m/Ma) would be only 27 % lower than our estimate. However, such a strong deviation from the linear relationship is not very realistic. To our knowledge all studies in this context either found linear or slightly convex (b slightly above 1) relations (see, e.g., Summerfield and Hulton, 1994) . This means that our approach perhaps underestimates the worldwide erosion rate slightly.

However, the relief also bears a potential of an overestimation going beyond the validity of the linear relationship for two reasons.

1. For deriving the worldwide mean erosion rates (Fig. 3c) from those of the predominantly erosive provinces (Fig. 3b), we assumed that the relationship between relief and erosion rate also holds for the other provinces. This procedure is based on the idea that these regions consist of erosive parts with the same value s as the purely erosive provinces and parts dominated by sediment deposition. Assuming that the value of s is valid for the entire region requires that the depositional parts have zero relief. However, even completely depositional areas have a (rather small) nonzero relief in reality, and this relief also contributes to the mean relief. Thus, the contribution of the not predominantly erosive provinces to worldwide erosion will be slightly overestimated.

2. Even more important, relief has changed through time at the million-year scale. If this change was spatially uniform, it would only affect the values of the erosional efficacies s, while the erosion rates would still be valid. Effects of glaciation should also not be very strong as the relief is measured at quite large scales (10 km). However, the formation or the decay of entire orogens would disturb the assumed relationship between present-day relief and long-term erosion. Then the relationship between these two properties would be weaker than we assumed, and the effect would be similar to a concave relationship ($b < 1$). So the real erosion rate could then be lower than our value obtained from the linear relationship. In the worst-case scenario, there would be no correlation between the present-day relief and the long-term erosion rate everywhere, and the real erosion rate could drop to the value of 57 m/Ma mentioned above. However, this is unrealistic, and we expect the potential bias to be much smaller.

8.5 The subdivision into climatic zones

The subdivision of Earth's surface into the primary Köppen-Geiger classes of the present-day climate is probably the most obvious source of potential systematic errors. First, the erosional efficacy of each climatic class is still a harmonic mean value, any spatial variation within a climatic class will result in an underestimation of the erosional efficacy. Beyond this, the climate has changed during the considered time intervals, and even significant parts of Earth's surface have moved, so that the question for the consequences of a potentially inappropriate subdivision of Earth's surface arises.

In this section we will use a simple model for illustrating that an inappropriate subdivision of the surface will result in a systematic underestimation of the worldwide mean erosion rate, but never in a systematic overestimation. In the worst case, the improvement coming from the subdivision will be entirely lost, and we would end up at the harmonic mean value.

We start from the example used for illustrating the underestimation by the harmonic mean in Sect. 2. We assume that Earth's surface consisted of two domains of equal sizes with a high erosion rate $r_h = 120$ m/Ma in one domain and a low erosion rate $r_l = 30$ m/Ma in the other domain. Let us now assume that we subdivide the surface in two also equally sized domains, but we were not able to delineate them correctly, so that both regions contain a mixture of r_h and r_l. Let λ be the contribution of the wrong erosion rate, so that domain 1 consists of $(1-\lambda)$ of r_h and λ of r_l and vice versa for domain 2. Then the estimated erosion rates of both domains are given by the harmonic mean values

$$r_1 = \frac{1}{\frac{1-\lambda}{r_h} + \frac{\lambda}{r_l}} \quad \text{and} \quad r_2 = \frac{1}{\frac{\lambda}{r_h} + \frac{1-\lambda}{r_l}}. \tag{8}$$

The estimated worldwide mean erosion rate is the arithmetic mean of r_1 and r_2.

The results of this simple model shown in Fig. 10 reveal that any imperfection in the subdivision causes an underestimation of the mean erosion rate. In the worst case, the mean erosion rate drops to the harmonic mean erosion rate of 48 m/Ma, so that the improvement achieved by the subdivision is entirely lost. As expected, the difference between the erosion rates of the two regimes is shadowed if the subdivision is not perfect. As the harmonic mean is dominated by the lower value, the high erosion rate r_h is strongly underestimated from domain 1 by the imperfect subdivision, while the lower rate r_l is only slightly overestimated from domain 2. In this example, even 10 % wrong contribution in each domain cost almost half of the improvement coming from the subdivision in total.

However, this example is somehow extreme as two strongly different regimes are mixed here, while the variations on Earth should be more gradual. Nevertheless, the subdivision of the surface into a limited number of domains will always retain a part of the underestimation coming from the harmonic mean being an inherent property of the approach. This underestimation will mainly effect the climatic zones with a high erosional efficacy.

8.6 Scale dependence of erosion rates

The discussion about potential systematic errors in erosion and sedimentation rates has been initiated by the fundamental paper of Sadler (1981) addressing the dependence of sedimentary records on the considered time scale. In this context we must distinguish whether a dependence of the rate on the covered time interval really exists or arises from the measurement. The latter would refer to a situation where the spatial distribution of the measurements is biased towards high erosion rates in the recent past. This effect has been, e.g., considered in the model of Ganti et al. (2016) where it was assumed that erosion takes place in distinct events, and measurements are only performed if there was an erosional pulse within a short time interval before present. Our approach is obviously invulnerable by this type of bias.

The situation considered by Schumer and Jerolmack (2009) is, however, more complex. In this study it was shown that a heavy-tailed distribution of hiatus lengths leads to a systematic decrease of erosion rates with time scale, while a heavy-tailed distribution of the sizes of erosional pulses leads to a systematic increase. However, it can be expected that the effect decreases when averaging over large spatial domains, so that our method should be less susceptible to such a bias than approaches based on individual points.

As long as there is no generally accepted model for the time-scale dependence of erosion rates often found, we cannot refute any susceptibility of our approach for such a bias completely, but there is at least no reason why it should be larger than in other methods.

8.7 Intermittent periods of sedimentation

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. As our approach addresses time scales of several million years, we cannot assume that all provinces considered as predominantly erosive (shields, orogens, igneous provinces) have been purely erosive over the entire time span.

Let us for the moment consider craters of a given depth H only, and let us assume that we are actually in a phase of erosion. Figure 11 illustrates the three types of behavior that could arise from intermittent periods of sedimentation. For the green curve we would find those craters produced in the continuous time interval since the depth of burial of the actual surface has dropped below H (horizontal dashed line). The period of recording is extended compared to a purely erosive situation, so that our estimated erosion rate will be lower than the average over the erosive phases. So it should be emphasized that our mean erosion rates are net rates where periods of deposition contribute negatively to the average, but this should not be considered as a bias.

Potential systematic errors are illustrated by the blue and red curves. The blue curve describes a scenario where sediment deposition took place long ago – a situation that has occurred quite frequently in the history of Earth. Then the period of recording is extended. As long as the old craters are also detected, the erosion rate will be underestimated. In turn, the red curve describes a situation where a depth of burial corresponding to the considered crater depth H has never been reached. Then the period of recording is shortened, so that the mean erosion rate will be overestimated. However, since the depositional phase also contributes to the crater inventory, the erosional period must be quite short in order to generate a significant overestimation.

Recapitulating the sources of systematic errors considered in the previous sections, there are two sources with unique direction. The residual incompleteness of the inventory above 6 km in diameter (Sect. 8.2) leads to an overestimation, while the imperfection in the subdivision into climatic zones (Sect. 8.5) results in an underestimation. The assumed linear relationship between relief and erosion rate (Sect. 8.4) and intermittent periods of sedimentation (Sect. 8.7) may introduce a bias in both directions, but at least for the latter, underestimation appears to be more likely than overestimation. The other potential systematic errors should be small. So there should in sum be some tendency towards underestimation rather than towards overestimation.

9 Conclusions

Our study yields long-term mean erosion rates as a function of topography expressed in terms of the 10 km relief and climate represented by the primary Köppen-Geiger classes. While the huge variation of topography on Earth makes the biggest contribution to the worldwide variablity of erosion rates, our results reveal a significant systematic dependence on climate in contrast to the results of several previous studies. We found an fivefold increase in erosional efficacy ~~defined by the erosion rate per relief~~ from the cold regimes to the tropical zone. Furthermore we found the temperate and arid climates to be very similar concerning their erosional efficacy. In this context it has to be taken into account that our study relates long-term erosion rates on the time scale of some tens of million years to present-day topography and climate. ~~As the climatic zones have shifted on this time scale,~~ Our approach yields long-term erosion rates of these parts of the crust actually belonging to a certain climatic zone, so that the difference in recent erosional efficacy ~~may be~~ can be expected to be even higher than predicted by our method. Furthermore, the erosional efficacy of the arid climate being similar to the temperate climate does not refute the importance of water for erosion, but may be related to less dry conditions in the arid zone in the geological history.

~~Concerning~~ With regard to the worldwide erosion rates we obtained a mean value on the time scale of some tens of million years of 78 m/Ma which is much higher than previous estimates derived from preserved sediments. As discussed in Sect. 8, this estimate should even be rather too low than too high, although the systematical errors should in sum be smaller than the statistical uncertainty. This result supports the hypothesis of Willenbring and von Blanckenburg (2010) that the apparent increase in worldwide erosion may be an artefact of the sedimentary record and that the observed increase in some mountainous regions (Herman et al., 2013) probably related to the Pleistocene glaciation could be a regional effect with a limited worldwide relevance. Our estimate is even about 25 % higher than the mean value of the ~~results~~ worldwide present-day erosion rates published since 1975. This result is qualitatively consistent with ~~the widely accepted~~ a decrease of erosion with decreasing temperature due to lower rates of weathering and could also be related to the retention of sediments in artificial reservoirs. However, both our long-term erosion rates and the present-day rates have uncertainties in the order of magnitude of the difference. Therefore we can conclude that the erosion rates have clearly been higher than they seem from preserved sediments and that there is no evidence for any change in worldwide erosion rates on the scale of some tens of million years.

Data availability. Data for reproducing the results and generating additional figures are available at http://hergarten.at/supplement.zip (preliminary location during the review process).

Appendix A: ~~Maximum-likelihood estimate of the erosion rate per relief~~

~~We consider a domain consisting of k subdomains (here, k is between 13 and 22) of areas A_i and mean relief Δ_i. According to Eqs. 2 and 4, the expected number of craters in each subdomain is~~

$$\lambda_i = \frac{A_i I}{s \Delta_i}$$

~~where we used the symbol λ_i instead of n_i in order to distinguish it from the actual number. The probability $p_i(n_i)$ that the actual number n_i of craters occurs, is given by the Poisson distribution,~~

$$p_i(n_i) = \frac{\lambda_i^{n_i} e^{-\lambda_i}}{n_i!}.$$

~~Then the joint probability to find the actual combination $n_1, , n_k$ is~~

$$p(n_1, \ldots, n_k) = \prod_{i=1}^{k} p_i(n_i).$$

~~This probability depends on the parameter s via Eqs. **??** and **??**. The maximum likelihood-method determines the most likely value of s in such a way that the probability to obtain the actual combination $n_1, , n_k$ becomes maximal. For convenience, the~~

~~function~~

$$
\begin{aligned}
L(s) &\equiv \log p(n_1,\ldots,n_k) \\
&\equiv \sum_{i=1}^{k} \log p_i(n_i) \\
&\equiv \sum_{i=1}^{k} \left(n_i \log \lambda_i - \lambda_i - \log(n_i!)\right).
\end{aligned}
$$

~~is maximized instead of p itself, so that~~

$$
\begin{aligned}
L'(s) &\equiv \sum_{i=1}^{k} \left(\frac{n_i}{\lambda_i} - 1\right) \frac{d\lambda_i}{ds} \\
&\equiv -\frac{1}{s^2} \sum_{i=1}^{k} \left(n_i s - \frac{A_i I}{\Delta_i}\right).
\end{aligned}
$$

~~The condition $L'(s) = 0$ immediately leads to Eq. 6.~~

Appendix A: Confidence intervals for the estimated erosion rates

~~Equation 6~~ Equation (6) used for determining the ~~erosion rates per relief~~ erosional efficacies s of the climatic zones only involves the total number of craters n in the considered zone as a random variable. As this variable follows a Poissonian distribution, confidence limits are readily obtained from the respective cumulative distribution. This also holds for the erosion mean absolute rates within each climatic zone according to Eq. ~~4~~(4). Only the worldwide mean erosion being the area-weighted mean of the individual rates,

$$
r = \frac{\sum_i A_i r_i}{\sum_i A_i}, \tag{A1}
$$

involves multiple random variables, so that confidence interval cannot be directly computed from a single statistical distribution. However, as shown in Fig. 3, the 70 % confidence intervals (corresponding to the standard deviation for a Gaussian distribution) are almost symmetric on a logarithmic scale. We therefore use half of the widths of these intervals as estimates of the individual errors $\delta \log_{10} r_i$ and compute $\delta \log_{10} r$ by Gaussian error propagation:

$$
\begin{aligned}
(\delta \log_{10} r)^2 &= \sum_i \left(\frac{\partial \log_{10} r}{\partial \log_{10} r_i} \delta \log_{10} r_i\right)^2 && \text{(A2)} \\
&= \sum_i \left(\frac{r_i}{r} \frac{\partial r}{\partial r_i} \delta \log_{10} r_i\right)^2 && \text{(A3)} \\
&= \frac{\sum_i (A_i r_i \delta \log_{10} r_i)^2}{\left(\sum_i A_i r_i\right)^2}. && \text{(A4)}
\end{aligned}
$$

Following the analogy of the 95 % confidence interval to twice the standard deviation for a Gaussian distribution, we define the 95 % confidence interval for the worldwide mean erosion rate by $2\delta\log_{10} r$. As the individual 95 % confidence intervals are more asymmetric and smaller than two times the 70 % confidence intervals on the logarithmic scale, this is a rather conservative estimate in the sense that the error towards lower erosion rate is overestimated.

Author contributions. S.H. designed the study and developed the theoretical framework and wrote the paper. T.K. provided the original idea and the knowledge on impact processes.

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References

Ahnert, F.: Functional relationships between denudation, relief, and uplift in large, mid-latitude drainage basins, Am. J. Sci., 38, 243–263, https://doi.org/10.2475/ajs.268.3.243, 1970.

Bland, P. A. and Artemieva, N. A.: The rate of small impacts on Earth, Meteorit. Planet. Sci., 41, 607–631, 2006.

Brocklehurst, S. H.: How glaciers grow, Nature, 493, 173–174, https://doi.org/10.1038/493173a, 2013.

Egholm, D. L.: Erosion by cooling, Nature, 504, 380–381, https://doi.org/10.1038/504380a, 2013.

Ferrier, K. L., J. T. Perron and, S. M., Rosener, M., Stock, J. D., Huppert, K. L., and Slosberg, M.: Covariation of climate and long-term erosion rates across a steep rainfall gradient on the Hawaiian island of Kaua´i, GSA Bull., 125, 1146–1163, https://doi.org/10.1130/B30726.1, 2013.

Ganti, V., von Hagke, C., Scherler, D., Lamb, M. P., Fischer, W. W., and Avouac, J.-P.: Time scale bias in erosion rates of glaciated landscapes, Sci. Adv., 2, e1600 204, https://doi.org/10.1126/sciadv.1600204, 2016.

Hergarten, S. and Kenkmann, T.: The number of impact craters on Earth: any room for further discoveries?, Earth. Planet. Sci. Lett., 425, 187–192, https://doi.org/10.1016/j.epsl.2015.06.009, 2015.

Herman, F. and Champagnac, J.-D.: Plio-Pleistocene increase of erosion rates in mountain belts in response to climate change, Terra Nova, 28, 2–10, https://doi.org/10.1111/ter.12186, 2016.

Herman, F., Seward, D., Valla, P. G., Carter, A., Kohn, B., Willett, S. D., and Ehlers, T. A.: Worldwide acceleration of mountain erosion under a cooling climate, Nature, 504, 423–426, https://doi.org/10.1038/nature12877, 2013.

Johnson, B. C. and Bowling, T. J.: Where have all the craters gone? Earth's bombardment history and the expected terrestrial cratering record, Geology, 42, 587–590, https://doi.org/10.1130/G35754.1, 2014.

Koppes, M., Hallet, B., Rignot, E., Mouginot, J., Wellner, J. S., and Boldtand, K.: Observed latitudinal variations in erosion as a function of glacier dynamics, Nature, 526, 100–103, https://doi.org/10.1038/nature15385, 2015.

Ludwig, W. and Probst, J.-L.: Predicting the oceanic input of organic carbon by continental erosion, Global Biogeochem. Cycles, 10, 23–41, https://doi.org/10.1029/95GB02925, 1996.

Marshall, J. A., Roering, J. J., Bartlein, P. J., Gavin, D. G., Granger, D. E., Rempel, A. W., Praskievicz, S. J., and Hales, T. C.: Frost for the trees: Did climate increase erosion in unglaciated landscapes during the late Pleistocene?, Sci. Adv., 1, e1500 715, https://doi.org/10.1126/sciadv.1500715, 2015.

Molnar, P.: Late Cenozoic increase in accumulation rates of terrestrial sediment: How might climate change have affected erosion rates?, Annu. Rev. Earth Planet. Sci., 32, 67–89, 2004.

Molnar, P. and England, P.: Late Cenozoic uplift of mountain ranges and global climate change: chicken or egg?, Nature, 346, 29–34, 1990.

Montgomery, D. R. and Brandon, M. T.: Topographic controls on erosion rates in tectonically active mountain ranges, Earth. Planet. Sci. Lett., 201, 481–489, https://doi.org/10.1016/S0012-821X(02)00725-2, 2002.

Moon, S., Chamberlain, C. P., Blisniuk, K., Levine1, N., Rood, D. H., and Hilley, G. E.: Climatic control of denudation in the deglaciated landscape of the Washington Cascades, Nature Geosci., 4, 469–473, https://doi.org/10.1038/ngeo1159, 2011.

Neukum, G., Ivanov, B. A., and Hartmann, W. K.: Cratering records in the inner solar system in relation to the lunar reference system, Space Sci. Rev., 96, 55–86, https://doi.org/10.1023/A:1011989004263, 2001.

Pedersen, V. K. and Egholm, D. L.: Glaciations in response to climate variations preconditioned by evolving topography, Nature, 493, 206–210, https://doi.org/10.1038/nature11786, 2013.

Peel, M. C., Finlayson, B. L., and McMahon, T.: Updated world map of the Köppen-Geiger climate classification, Hydrol. Earth Syst. Sci., 11, 1633–1644, https://doi.org/10.5194/hess-11-1633-2007, 2007.

Portenga, E. W. and Bierman, P. R.: Understanding Earth's eroding surface with ^{10}Be, GSA Today, 21, 4–10, https://doi.org/10.1130/G111A.1, 2011.

Riebe, C. S., Kirchner, J. W., Granger, D. E., and Finkel, R. C.: Minimal climatic control on erosion rates in the Sierra Nevada, California, Geology, 29, 447–450, https://doi.org/10.1130/0091-7613, 2001a.

Riebe, C. S., Kirchner, J. W., Granger, D. E., and Finkel, R. C.: Strong tectonic and weak climatic control of long-term chemical weathering rates, Geology, 29, 511–514, https://doi.org/10.1130/0091-7613(2001)029<0511:STAWCC>2.0.CO;2, 2001b.

Sadler, P. M.: Sediment accumulation rates and the completeness of stratigraphic sections, J. Geol., 89, 569–584, 1981.

Schildgen, T. F., van der Beek, P. A., Sinclair, H. D., and Thiede, R. C.: Spatial correlation bias in late-Cenozoic erosion histories derived from thermochronology, Nature, 559, 89–93, https://doi.org/10.1038/s41586-018-0260-6, 2018.

Schumer, R. and Jerolmack, D. J.: Real and apparent changes in sediment deposition rates through time, J. Geophys. Res., 114, F00A06, https://doi.org/10.1029/2009JF001266, 2009.

Stöffler, D. and Ryder, G. G.: Stratigraphy and isotope ages of Lunar geologic units: chronological standard for the inner solar system, Space Sci. Rev., 96, 9–54, https://doi.org/10.1023/A:1011937020193, 2001.

Summerfield, M. A. and Hulton, N. J.: Natural controls of fluvial denudation rates in major world drainage basins, J. Geophys. Res., 99, 13 871–13 883, https://doi.org/10.1029/94JB00715, 1994.

Syvitski, J. P. M., Vorörösmarty, C. J., Kettner, A. J., and Green, P.: Impact of humans on the flux of terrestrial sediment to the global coastal ocean, Science, 308, 376–380, https://doi.org/10.1126/science.1109454, 2005.

Valla, P. G., Herman, F., van der Beek, P. A., and Braun, J.: Inversion of thermochronological age-elevation profiles to extract independent estimates of denudation and relief history – I: Theory and conceptual model, Earth. Planet. Sci. Lett., 295, 511–522, https://doi.org/10.1016/j.epsl.2010.04.033, 2010.

von Blanckenburg, F.: The control mechanisms of erosion and weathering at basin scale from cosmogenic nuclides in river sediment, Earth Planet. Sci. Lett., 242, 224–239, https://doi.org/10.1016/j.epsl.2005.11.017, 2006.

Wang, P., Scherler, D., Liu-Zeng, J., Mey, J., Zhang, J.-P. A. Y., and Shi, D.: Tectonic control of Yarlung Tsangpo Gorge revealed by a buried canyon in Southern Tibet, Science, 346, 978–981, https://doi.org/10.1126/science.1259041, 2014.

Whipple, K. X., DiBiase, R. A., and Crosby, B. T.: Bedrock rivers, in: Fluvial Geomorphology, edited by Shroder, J. and Wohl, E., vol. 9 of *Treatise on Geomorphology*, pp. 550–573, Academic Press, San Diego, CA, 2013.

Wilkinson, B. H. and McElroy, B. J.: The impact of humans on continental erosion and sedimentation, Geol. Soc. Am. Bull., 119, 140–156, https://doi.org/10.1130/B25899.1, 2007.

Willenbring, J. K. and Jerolmack, D. J.: The null hypothesis: globally steady rates of erosion, weathering fluxes and shelf sediment accumulation during Late Cenozoic mountain uplift and glaciation, Terra Nova, 28, 11–18, https://doi.org/10.1111/ter.12185, 2015.

Willenbring, J. K. and von Blanckenburg, F.: Long-term stability of global erosion rates and weathering during late-Cenozoic cooling, Nature, 465, 211–214, https://doi.org/10.1038/nature09044, 2010.

Willenbring, J. K., Codilean, A. T., and McElroy, B.: Earth is (mostly) flat; apportionment of the flux of continental sediment over millennial time scales, Geology, 41, 343–346, https://doi.org/10.1130/G33918.1, 2013.

Willenbring, J. K., Codilean, A. T., Ferrier, K. L., McElroy, B., and Kirchner, J. W.: Short Communication: Earth is (mostly) flat, but mountains dominate global denudation: apportionment of the continental mass flux over millennial time scales, revisited, Earth Surf. Dynam. Discuss., 2, 1–7, https://doi.org/10.5194/esurfd-2-1-2014, 2014.

Wulf, G., Hergarten, S., and Kenkmann, T.: Combined remote sensing analyses and landform evolution modeling reveal the terrestrial Bosumtwi impact structure as a Mars-like rampart crater, Earth. Planet. Sci. Lett., 506, 209–220, https://doi.org/10.1016/j.epsl.2018.11.009, 2019.

Yanites, B. J. and Ehlers, T. A.: Global climate and tectonic controls on the denudation of glaciated mountains, Earth. Planet. Sci. Lett., 325–326, 63–75, https://doi.org/10.1016/j.epsl.2012.01.030, 2012.

Zhang, P.-Z., Molnar, P., and Downs, W. R.: Increased sedimentation rates and grain sizes 2–4 Myr ago due to the influence of climate change on erosion rates, Nature, 410, 891–897, https://doi.org/10.1038/35073504, 2001.

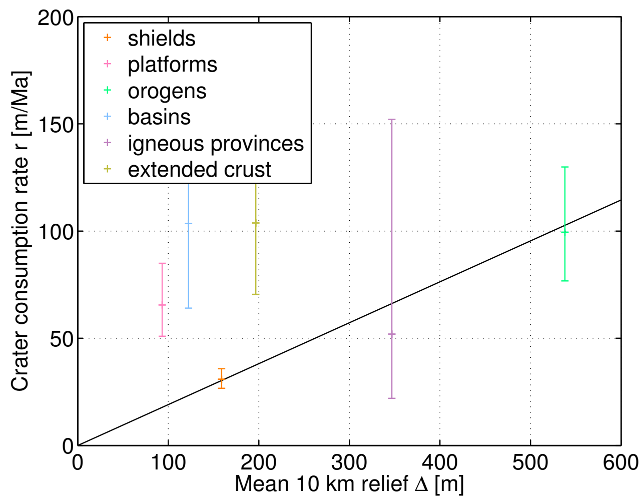

Figure 1. Rates of crater consumption derived from Eq. (2) vs. mean relief for the basic types of continental crust (http://earthquake.usgs.gov/data/crust/type.html). The error bars represent 70 % confidence intervals corresponding to the standard deviation for a Gaussian distribution.

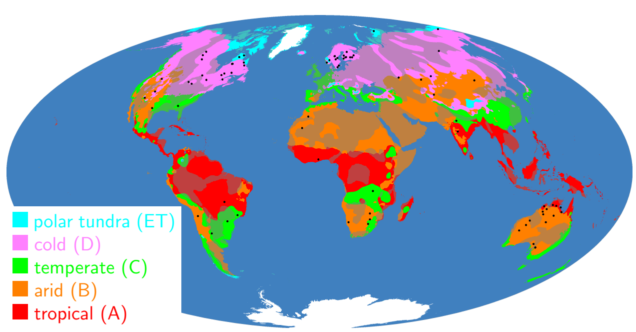

Figure 2. The primary Köppen-Geiger climate classes (Peel et al., 2007) considered in this study. Solid colors correspond to the predominantly erosive provinces (shield, orogen, igneous), while the respective pale colors mark those regions not considered in order to avoid a bias by sediment deposition. The black dots show the 77 craters with diameters $D \geq 0.25\,\text{km}$ located in the predominantly erosive provinces being the basis of our analysis.

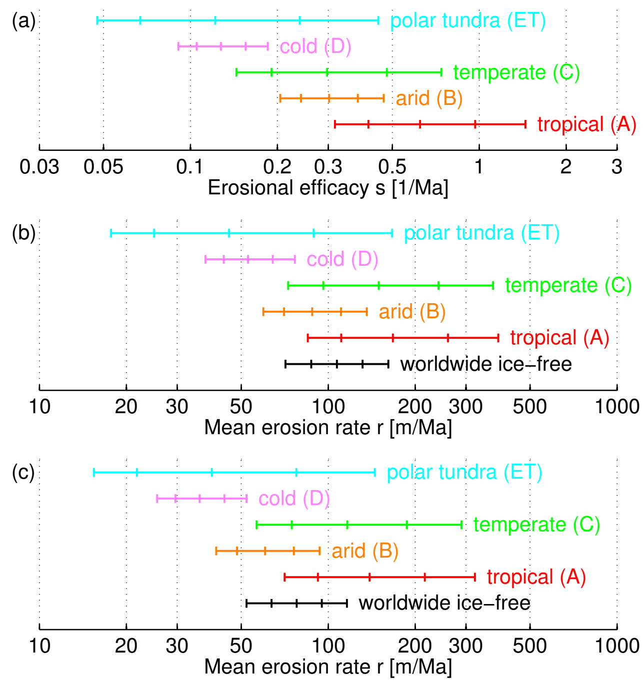

Figure 3. Erosion rates by climatic zones. (a) Mean ~~erosion rates per relief for the primary classes~~ erosional efficacies of the primary Köppen-Geiger ~~scheme~~classes. (b) Respective absolute mean erosion rates for the predominantly erosive provinces. (c) Absolute mean erosion rates extrapolated to the entire ice-free surface including the classes platform, basin, and extended crust. Error bars represent ~~the~~ 70 % confidence intervals (corresponding to the standard deviation for a Gaussian distribution) and ~~the~~ 95 % confidence intervals (see ~~??~~Appendix A).

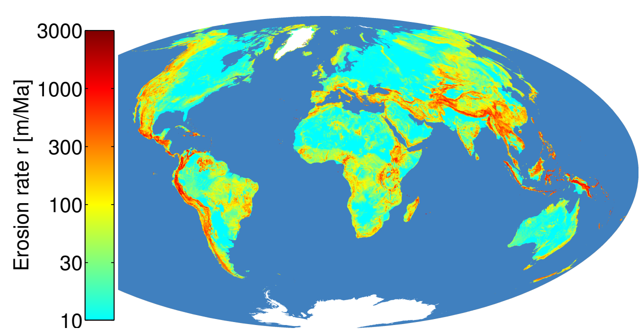

Figure 4. World map of the erosion rates obtained in this study.

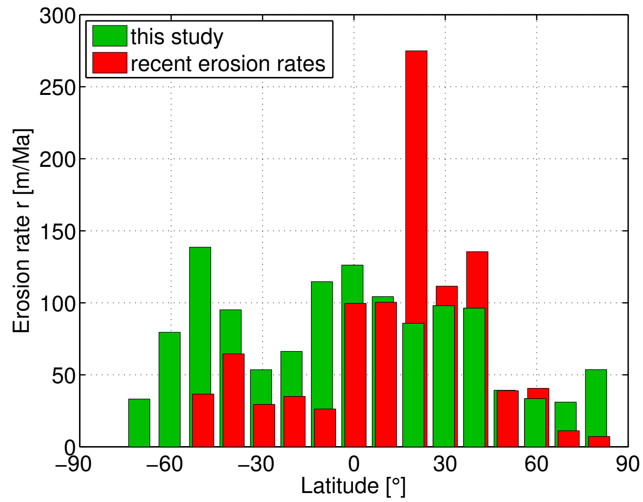

Figure 5. Mean erosion rates as a function of latitude in 10° intervals. Present-day erosion rates are taken from Wilkinson and McElroy (2007).

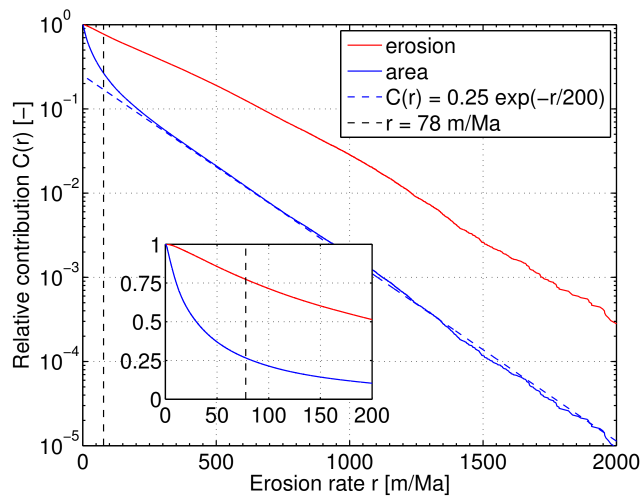

Figure 6. Cumulative distribution of the erosion rates and their contribution to the total erosion. The blue curve shows the contribution of the part of the land surface with an erosion rate greater than r to the total area, and the red curve its contribution to the total erosion.

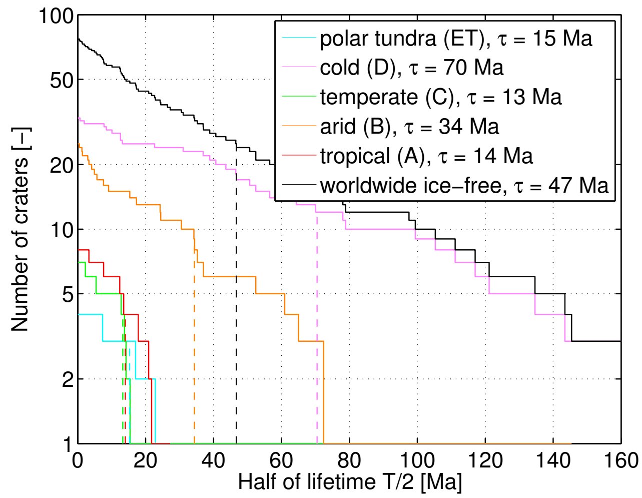

Figure 7. Cumulative number of the considered craters as a function of half of their estimated lifetime, equivalent to the sensitivity of the number of craters to changes in the erosion rate at a given time. The values of τ given in the legend are the decay constants of the exponential decrease.

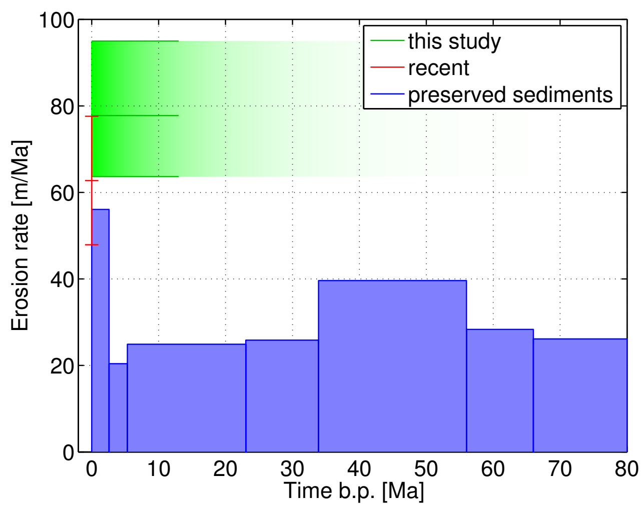

Figure 8. Comparison of our worldwide long-term mean erosion rate with estimates obtained from preserved sediments (Wilkinson and McElroy, 2007) and recent erosion rates compiled by Willenbring et al. (2013) . The green area represents our result for the mean erosion rate of $r = 78$ m/Ma with 70 % confidence intervals. The decreasing opacity visualizes the exponentially decreasing sensitivity with $\tau = 13$ Ma.

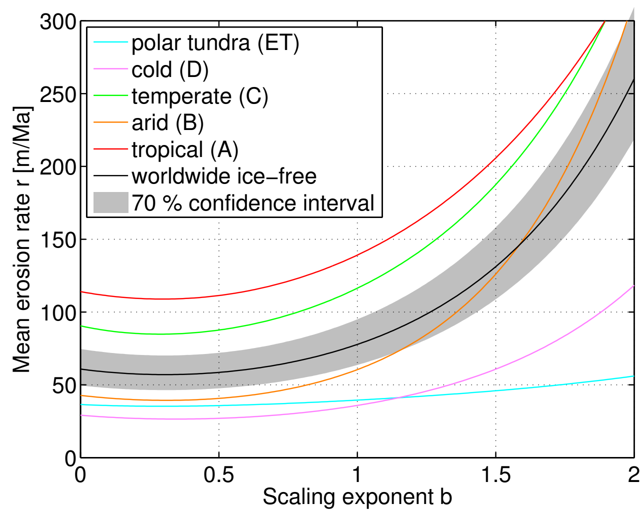

Figure 9. Results of our approach assuming a nonlinear relationship between relief and erosion rate (Eq. 7).

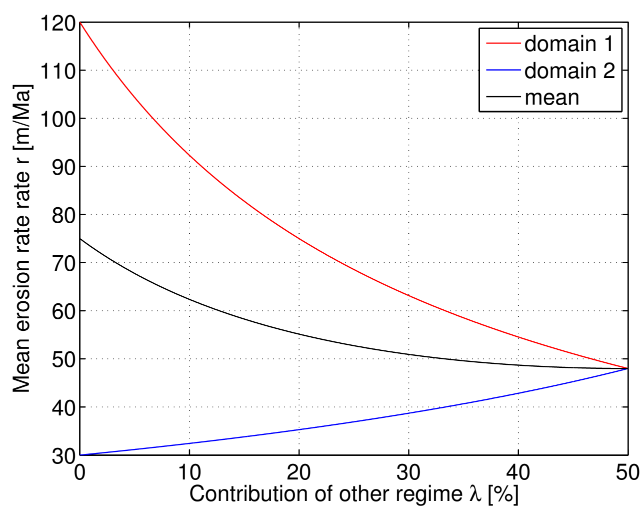

Figure 10. Estimated erosion rates if Earth consisted of two equally-sized parts with erosion rates $r_h = 120$ m/Ma and $r_l = 30$ m/Ma. It is assumed that two also equally-sized domains are analyzed separately, where the major part of domain 1 has the erosion rate r_h and the major part of domain 2 has the erosion rate r_l, but each of the domains contains a given contribution λ of the other erosion rate (Eq. 8).

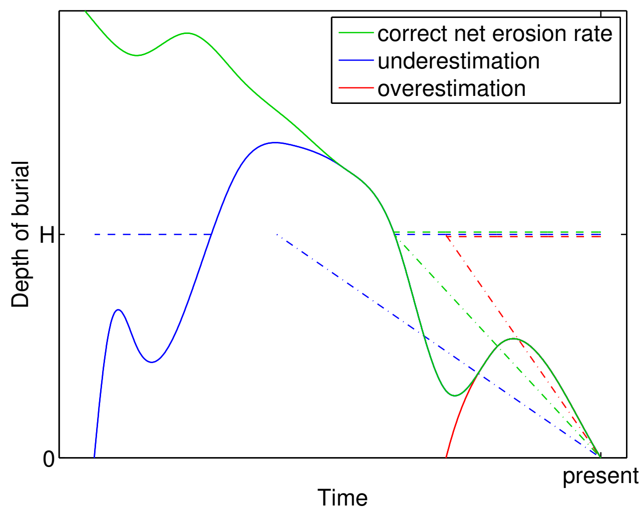

Figure 11. Three scenarios of intermittent phases of sedimentation. Solid curves: depth of burial of the present-day surface. Dashed lines: time intervals of crater production where a crater of a given depth H would be detectable at the present surface. Dotted lines: equivalent erosional histories (same expected number of craters) with constant erosion rates.