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Interactive comment on "Bias and error in modelling thermochronometric data: resolving a potential increase in Plio-Pleistocene erosion rate" *by* Sean D. Willett et al.

Sean D. Willett et al.

swillett@erdw.ethz.ch

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Response to Anonymous Referee #2

REVIEWER: 1. Overview: what is the conclusion of Herman et al. (2013)? Is their conclusion that some mountainous, glaciated regions were exhuming faster between 0-2 Ma compared with 4-6 Ma? Is their conclusion that most glaciated mountains were exhuming faster between these times? Most mountains? Or that there was a net increase in erosion rates globally?

RESPONSE:

We find this a rather odd question to ask. It is with reference to a paper published 7 years ago, not the one currently under review, and it is a question that we believe was answered implicitly and explicitly many times in both papers. Herman et al. (2013) and Willett et al. (2020) make no estimate of the mean global erosion rate or its change in time. There is no claim to this effect in either manuscript. There would not be such a focus on resolution and its estimation, if we were not trying to determine which regions have the resolution to make an estimation of changes in erosion rate. The maps in Herman et al. (2013) and Willett et al. (2020) (Fig. 25) show precisely where the analysis establishes sufficient resolution and therefore where it applies. It does not apply elsewhere and the conclusions are limited to the specific sites on those figures. These are all sites where erosion rates are high enough that thermochronometric ages are young and resolution is established (Willett et al. (2020), Figure 1). If it is grey on the map, there is no estimate made. We do not argue that the well-resolved regions are representative of the Earth as a whole. We can add this statement to the current paper if desired, but it is not clear as to why we should have to do this since we never claimed to provide a global estimate. In terms of a testable null hypothesis: regions that have erosion rates high enough that thermochronometric data can provide cooling rate constraints over the last 6 Ma have had no change in erosion rate over this time.

REVIEWER:

2 Schildgen et al., 2018 critique

RSEPONSE:

This is a fair assessment of our paper.

REVIEWER:

3 Willenbring and Jerolmack, 2016 critique

WJ16 argued that the methodology of H+13 is biased because only sites with rapid exhumation will precisely resolve a recent change in exhumation. Even if GLIDE perfectly identifies regions where thermochronology ages resolve a recent increase in erosion rate (a point contested by S+18), there will be many regions which have experienced constant or lower erosion rates that are cannot be resolved and are therefore under-represented.

RESPONSE:

This is true, but only relevant if we are trying to estimate the average global erosion rate. As stated above, we are not trying to obtain a global average.

REVIEWER:

“The essence of the argument is that slower rates of exhumation are progressively clipped from the measured age distribution of rocks as one approaches the modern because of the precision of the technique. The reason is that thermochronometers measure the time since achieving an associated closure depth; rate is simply the measured time divided by this depth. If erosion rates have not been high enough to bring rocks to the surface, the rates associated with those buried rocks cannot be measured.”

And here is the response to this critique in W+PS:

“. . .Willenbring and Jerolmack (2016) misinterpreted the meaning of the limit to resolution, or what they called “precision”. They took this concept from measurements of sediment layer thickness [the ‘idea of Anders et al., (1987)’ referenced in WJ16 argument above], which can be biased by the inability to measure layer thicknesses below a specific precision (Anders et al., 1987). “Thermochronometric data are fundamentally different in that an age does not represent an estimate of rate at a point or across a closed interval of time such as represented by a stratigraphic layer, but rather, represents the integral of the exhumation rate. . .from the time of closure to the present day. Thus, rather than being unsampled, the region [from the youngest closure age until present] is actually the most heavily sampled part of parameter space. The better definition of the “limit to resolution” is that no change of exhumation rate can be resolved

between this limit and time zero, because there is no sampling internal to this part of the parameter space, but the average rate across this interval is sampled and can be determined.”

This response misconstrues the legitimate criticism raised by WJ16.

RESPONSE:

We don’t agree that we are misconstruing WJ16. We understand what they said and our paper is directly responding to the passage quoted by the reviewer. The problem is that what WJ16 say in the first paragraph: "If erosion rates have not been high enough to bring rocks to the surface, the rates associated with those buried rocks cannot be measured.” is, in our view, not correct. The premise and analysis of WJ16 is based on this misunderstanding of the nature of thermochronometric ages. If the premise is wrong, there is no point in engaging with the predictions. This is what the above paragraph from our paper is addressing. An age integrates the full exhumation path and therefore it is NOT necessary to bring up a rock from the closure depth to the surface in order to measure a change. This is more or less the same point brought up by the reviewer in section 5 on buried rock bias, so we will address this further in response to that point.

REVIEWER:

4. Sensitivity analysis

What remains to be demonstrated is whether the methodology of H+13 / W+PS could resolve constant or decreasing erosion rates within a synthetic test. The analyses in Section 4 of W+PS indicate that constant but spatially distinct long-term erosion rates can be resolved, but similar tests are not provided for a recent change in exhumation rate.

RESPONSE:

We agree that we have not investigated in detail the case where there is a true change in erosion rate. The paper is already long and we feel it is more important to establish the principles of resolution and bias to provide context, and to address the problem of the “false positives”, that is to show that the increases that H+13 find are not artifacts of the methodology. If necessary, future work could show this, but there is not a strong argument that it is harder to resolve an increase than to resolve a constant rate, provided the resolution criteria are selected to resolve either.

REVIEWER:

5 The problem of still buried rocks ll. 185-189. “[I]f one regresses age against depth (Figure 2d) with a moving depth window, one obtains the correct, unbiased regional mean erosion rate (Figure 2e, Average 1).” This statement and the corresponding figure, W+PS Fig. 2d (reproduced here in Fig. 1), neglect the fact that a significant portion of rocks which passed through the closure depth after a recent change in exhumation rate will not yet have reached Earth’s surface. W+PS Fig. 2d illustrates only a special case, where erosion is spatially varied but constant through time. This is unfortunate and misleading, because the question at hand is whether transient erosional signals (i.e., the onset of Northern Hemisphere glaciations) can be measured (in the midst of spatial variability).

If an instantaneous change in exhumation rate occurred recently, it will only be observable if rocks: a) began beneath the closure depth and b) were exhumed from that depth to the surface to be collected. This exhumation distance imposes a limit on which recent exhumation rates can be measured (Fig. 1).

The following thought experiment crudely estimates the magnitude of this ‘still buried rock’ bias effect (Fig. 2). H+13 evaluated the difference in global erosion rates between the time periods of 4 – 6 Ma and 0 – 2 Ma. So, it is useful to consider what would happen if global erosion rates changed significantly and instantaneously at 2 Ma. Apatite (U – Th)/He is the lowest temperature system considered in H+13 and would close at a depth of approximately 2 km, depending on the local exhumation rate and geothermal

gradient. At 2 Ma, all rocks below this closure depth would begin their ascent according to the new surficial erosion rate. Therefore, a geologist collecting surficial samples today could only possibly collect samples which rose at a rate of (2 km) / (2 Ma) or greater. All other rocks would still be buried. This effect is illustrated in Fig. 2 for a range of times at which erosion rates could have instantaneously changed.

RESPONSE:

We are happy to discuss the thought experiment offered by the reviewer. However, we think this framing is not correctly describing the situation. Yes, rocks that closed at 2 Ma, but experienced a slow erosion rate, are still buried. But there is still a rock at the surface. That rock still has an age. An apatite (U-Th)/He age of this rock has only two possibilities; it is less than 2 Ma or greater than 2 Ma. If it is less than 2 Ma, it accurately measures the current erosion rate; higher closure-temperature ages would then correctly resolve the change in rate, provided there is at least one age over 2 Ma. If the surface apatite (U-Th)/He age is older than 2 Ma, it closed prior to the acceleration, but its age reflects the integrated rate of exhumation since this time, including the 2 Ma at a higher rate (Fig.1a, red curve). Relative to a rock that did not experience an increase in erosion rate, but continued to exhume at the original rate, all ages at the accelerating site would be younger by a constant amount, and this is true for any erosion rate, not just high rates. The same is true for a rock that experienced a decrease in erosion rate (Fig.1a, blue curve); the age integrates the travel time from the closure depth, and so reflects any change in exhumation rate post-closure. Reviewer 2 argues that no changes in rate can be detected for rates less than the black dashed line (unexposed rock limit) in Fig.1, but this is clearly not the case. Rates inferred from these ages using the two mineral method are shown in Figure 1b. The ages do not perfectly resolve the chang, but both increases and decreases are detectable. The fact that part of the zone from the closure depth to the surface is still buried is not the limitation. Stating that a rock from the closure depth must come to the surface since a change in rate occurred, as is shown in Reviewer 2’s Fig. 1b in order to detect it is overly restrictive. The example in our Figure 1 shows that a 4 Ma apatite He age would still detect an increase or a decrease in erosion rate, but with half (ignoring the non-linear relationship between age and erosion rate) the magnitude and spread over time. The earlier rate is still properly resolved.

What an older age cannot do is resolve precisely the timing and magnitude of the change in erosion rate. It is detected, but poorly resolved and thus averaged with the earlier rate. The older the age, the worse the resolution, as discussed in our paper. If the ages are too old, resolution will be so poor that data with measurement error will not resolve it and the H+13 analysis would reject the site; but this limit is much lower than the 1 mm/yr estimated by the reviewer. Given old ages, most analyses will underestimate the magnitude and overestimate the onset time of an increase in erosion rate. Note that this bias is towards a constant rate of erosion, not a false increase. This was the basis for our statement on lines 196-198. This critique is also true for the reviewer's Figure 2. We don't see that we have misrepresented the problem or what W+P stated; nor have we failed to engage with the criticism. We are simply stating in the current manuscript that they premised the problem incorrectly, and we stand by our original discussion based on the logic presented above.

REVIEWER:

From this exercise, two observations stand out. First, only terrains experiencing very rapid exhumation will inform the thermochronology record of erosion rate changes at 2 Ma. Second, it is much more likely to observe accelerations in exhumation than to observe constant or decelerating exhumation. To avoid confusion, note the following distinction. The approach of H+13 / W+PS will register low erosion rates during the period of 2 Ma to present owing to old AHe and AFT ages (how 'resolved' low rates are depends on the GLIDE algorithm). What the H+13 / W+PS approach will not register, however, are any decelerations down to rates below about 1km/Ma.We must take as a null hypothesis the idea that global net erosion rate change was zero from 6 Ma until present. For the reasons outlined above, it is my view that the H+13 / W+PS

methodology is inherently incapable of resolving unchanging net erosion rates globally. The authors have not taken seriously this critique, initially outlined in WJ16, and have not demonstrated their ability to resolve a constant erosion rate or a decreasing erosion rate.

RESPONSE:

Again, we have to disagree with most of this conclusion. It is mostly directed at the issue addressed in the first paragraph – as if we are trying to estimate a global erosion rate. As we are not estimating global erosion rates, most of this and the 'null hypothesis' as stated above are not relevant. We have effectively pre-selected the sites we studied as having high erosion rates (not 1 mm/yr – that is much too high), and we are testing the null hypothesis that these sites individually and collectively have no change in erosion rate.

There is a hint of a true bias implicit to this final criticism, even though the quantification in Figs 1 and 2 is not correct. Sites with high erosion rates will be more likely to have experienced a recent acceleration than deceleration; sites with low modern rates are more likely to have experienced a deceleration in the recent past, and because low rate sites are removed, and because erosion rates cannot be negative, this does create a bias in the analysis suite of Herman et al. (2018). This bias does not depend on the rate or the burial argument of Reviewers Figures 1 and 2, but rather on the frequency and magnitude of erosion rate changes in orogenic belts. For a given modern erosion rate, there is some probability as to what the erosion rate would be at any point in the past, i.e. 2 Ma, and this probability will be skewed because of non-negativity and truncation of low rates. We believe this skewness will be small because tectonic-driven erosion rate changes are heavily damped by geomorphic processes and isostasy (Whipple and Meade, 2004) and rapid climate change is mostly cyclic, but we acknowledge there are few data and no systematic treatment of this problem. This is a bias on the median change, i.e. the mean or median of the distribution of Figure 25 in Willett et al. (2020), and if this distribution were closer to zero-median, we would have investigated it in

Herman et al. (2013). However, this is not a bias on the result at any individual site, so is not the main topic of the current manuscript or part of the critique by Schildgen et al. (2018).

In summary, we think this review is mostly arguing a point that is not really the subject of this paper: whether the high erosion rate mountain belts of the world are representative of the Earth as a whole. We have not made any statement that this is true and are happy to clarify that once more in the introduction of this paper. If one interprets the hypothesis of Willenbring and Jerolmack (2016) as being applicable on a local site bias, we contend it is still not correct because it does not recognize the integral nature of a cooling age since closure, and the preselection implicitly done by Herman et al. (2013). We do accept that there could be a bias in the median value of the global analysis of Herman et al. (2013) due to the loss of some sites where deceleration has led to the sites dropping below the resolution cutoff, which would bias the median of the distribution in Fig. 24, but this bias does not affect the result at any individual site. We can add a paragraph to section 6.4 acknowledging this effect.

Ref

Whipple, K., and Meade, B., 2006, Orogen response to changes in climatic and tectonic forcing: Earth and Planetary Science Letters, v. 243, no. 1-2, p. 218–228, doi: 10.1016/j.epsl.2005.12.022.

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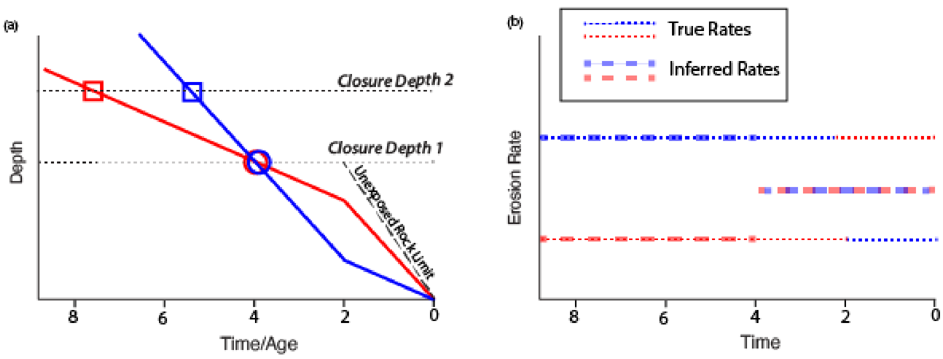

Fig. 1. Resolution of an increase or decrease in erosion rate at 2 Ma, where all ages close prior to the change. (a) Depth time path and ages. (b) Inferred erosion rates from the mineral pair method.