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Earth Surface Dynamics Discussions

Interactive comment on "Tectonic controls of Holocene erosion in a glaciated orogen" *by* Byron A. Adams and Todd A. Ehlers

G. Hilley (Referee)

hilley@stanford.edu

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Summary:

This contribution presents 14 new cosmogenically derived erosion rate measurements from the Olympic Mountains, Washington State, USA. These rates are compared to various climate measures, morphometrics, and exhumation / incision proxies to provide insight into the following questions: 1) Is there a discernible imprint of climate gradient on erosion rates measured by these cosmogenic inventories? 2) Is there a signal of disequilibrium conditions recorded by a discrepancy between erosion and exhumation measures over various time-scales?, 3) Do landscape morphometrics scale with erosion rates?, and 4) What is the relationship between measured erosion rates

and inferred long-term rock uplift rates? The authors generally find that variations in modern climate measures do not scale with measured erosion rates, but, at least at low erosion rates, measures such as local relief and mean channel steepness scale in some way with erosion rates. The authors find that there is a general correspondence between river incision rates, exhumation gauged by low-temperature thermochronology, and modern-day cosmogenically derived erosion rates. As such, even modern (millennial time-scale) erosion rates appear to track long-term exhumation (and perhaps rock-uplift) rates in the Olympics, and that glacial processes do not appear to disrupt landscape equilibrium to an extent that would produce a divergence between modern and long-term measures of erosion of the range.

Recommendation:

This paper presents interesting new data and analyses of an active, glaciated mountain belt, where large precipitation gradients and temporal changes in surface processes may be expected to leave some imprint on the erosion of the range. The authors finding that, despite these spatial and temporal variations, erosion rates measured over various time-scales are approximately constant, should be of interest to the readership of Earth Surface Dynamics. The study appears thoughtfully conceived and executed, and is written in a clear and concise manner that requires few grammatical changes. Thus, with consideration of the comments below, I would feel comfortable recommending acceptance pending MINOR to MODERATE REVISIONS. Below, I make some general suggestions, as well as some specific comments geared to individual lines in the text.

General Comments:

1) The authors have carried out a detrital 10-Be study that supposes that erosion rates in the catchments are everywhere equal. This is somewhat addressed in the text under the 5.1 section, last paragraph, where the authors discuss the impact of the introduction of dosed and shielded material into their samples. Yet, this does not address the

fact that the authors' approach assumes that each point in the basin is equally represented in the sample, as well as the fact that the calculated mean production rate could be biased by increased contributions from different elevation ranges because of the non-linear increase of production rate with elevation. I am not uncomfortable with the authors' assumptions (in addition to the fact that quartz is uniformly distributed in the sourced lithologies). But, given that some of these catchments have a good amount of local relief and lithologic variability leading to heterogeneous quartz "fertility", some discussion of this effect, and its potential impact might be appropriate to include in section 5.1.

2) I found the correspondence between river incision, exhumation, model-derived erosion rates, and 10-Be denudation rates compelling. One way in which these relationships could be made more effective and illustrative would be to actually plot the quantities versus one another, rather than distance (Figure 7). I think I understand why the authors plotted these rates in the particular space they did, in that some of the primary studies were carried out within areas that do not overlap with the cosmogenic samples directly, but lie within similar tectonic positions when these data are projected onto a cross section. My reading of the primary literature is that 1) the Clearwater (which I think are the black dots) is located outside of the sampled area shown in Figure 2E, and so must be projected into the sampled basins to be used in this study. 2) The AFT ages are from throughout the range, and so there is probably a good path forward for interpolating these across the sampled basins to calculate point-by-point estimates of exhumation rate, and to use these to quantify basin-averaged exhumation rates within each sampled watershed. 3) Drew Stolar's modeling study is a profile model, which is fine. But, it is tuned to a specific mean erosion rate that I think was chosen with the AFT exhumation rates in mind. Thus, it is not particularly surprising that the magnitudes match up with what is observed, since the AFT exhumation rates roughly align with the 10-Be rates.

This is all to say that the most robust and comparable of these datasets appear to be the

AFT- and 10-Be-derived rates. To use the other measures, it seems like some sort of projection has to be made, which introduces its own set of assumptions. Please correct me if I am wrong about this. This being the case, it might be interesting to carry out a direct comparison of the most comparable of the datasets, meaning the (interpolated) AFT-derived exhumation rates for each watershed that was sampled for 10-Be. One would then have a direct comparison that could be used to carry out a formal analysis to reveal the strength of correspondence (through regression analysis), the presence / absence of systematic discrepancies between datasets that could reveal more insight into where correspondences are best versus where they might break down to some degree. I feel that a map of the difference between exhumation and erosion rates in the spirit of Figure 2E might be revealing.

Specific Comments:

Lines 115-120: For documentation, it would probably be good to have some description of how you calculated ks. There are a few different solution methods available, and a few different software packages to do this. Quick mention of these would be good for completeness of documentation.

Lines 290-300: The relationship with channel steepness looks good between $0 \leq e \leq 600$ m/Myr. At higher erosion rates, it does not do so well, even for the samples with reasonable 26Al/10Be. Also, you might consider adjusting your concavity to 0.4, so that you can use the multiplier of the power law to calculate K and compare this to the range for your lithologies via Stock and Montgomery (1999). This will help provide some additional validation.

Lines 310-315: As per general comment 1 above, I don't think that spatial variations in erosion rates are covered here, meaning that one could have a 6.75 ratio (i.e., no complex exposure history) but your sample might not be representative of all sediment in the basin. Also, I assume that the "deep" landslide mechanism would be one in which dosed material was buried for a long time (a fraction of the 1/2 life of 26-Al) and then reworked into transported sediment. But, I think a "deep" bedrock landslide should still have the 6.75 ratio, since it did not get dosed and re-buried.

Thanks for the opportunity to review this work. I really enjoyed the paper and look forward to its formal "release" online in Earth Surface Dynamics.

George Hilley.

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