

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

Anonymous Referee #1

Received and published: 11 April 2018

General Comments: The authors present results of topographic analysis and catchment-scale denudation rates determined using cosmogenic nuclides in the Olympic Mountains of Washington, USA. The goal of the analyses is to assess controls on the spatial patterns of denudation. The authors find that denudation rates scale with multiple metrics of topographic steepness and previously recognized spatial patterns of exhumation. Present day precipitation patterns and the extent of prior glaciation do not explain patterns denudation measured by cosmogenic nuclides.

The finding that denudation rates are not spatially correlated with precipitation, but instead are correlated with tectonic forcing is consistent with findings from a number of recent studies in other mountain ranges. Hence the work contributes to an emerging view on the role of climate in influencing erosion rates in tectonically-active landscapes

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and is hence appropriate for publication in Earth Surface Dynamics. However, I have a number of comments that should be addressed in a revised manuscript below.

More generally, the manuscript begins (in the abstract) by indicating the role of topographic adjustment by glaciers in setting post-glacial erosion rates is unknown and that there are intense spatial variations in the glacial modification of topography. These statements are both correct, and in the Olympic Mountains there is evidence for spatial variation in topographic modification (e.g., Montgomery, 2002; Prasicek et al., 2014). However, the manuscript does not exploit the spatial variability in glacial modification to ask whether the degree of topographic modification and Holocene erosion rates scale with glacier size or whether Holocene erosion rates scale with the degree of glacial topographic modification. The manuscript stands on its own without addressing these questions, however, addressing these questions would increase the impact of the manuscript and help determine whether rock uplift alone drives the observed patterns in erosion or whether there is also an additional signature caused by glacier-induced increases in relief.

Comments: Line 21-24: It could easily be argued that buildup of topography, high relief, high erosion rates, etc., has also occurred after the onset of Cenozoic cooling and glaciation. Willett (1999) presents results of a modeling exercise, which does not directly support the claims regarding controls on topography and erosion prior to the Cenozoic. Hence it is not clear that these introductory sentences properly motivate the story that follows.

Line 24: 'between these characteristics' – please be explicit and write out what is meant by 'these'

Line 27: It would be useful to include a citation to a study or studies that document glacier fluctuations on response to climate change.

Line 47: It is not clear what is meant by 'efficiency' and also not clear what data/prior study support that statement.

Line 158: The description of why effective latitude and altitude values calculated for each catchment do not incorporate temporal variation in production rates needs to be re-visited or further explained. The time-variation in production is caused by temporal variation in earth's magnetic field. Given the size and elevation range of the catchments sampled, it is not clear (without a calculation to demonstrate it) that using effective altitude and latitude inputs would substantially distort predictions from a time-varying production model. Alternatively, simply state the reported values are based on a constant production rate model.

Line 194-196: The text explaining why statistics were not performed on subsets of the data is cumbersome, primarily because there first is not a justification for why the data would or could be divided into subsets.

Line 197: If the regressions account for uncertainty in both variables, then the regression technique should be reported; York or RMA or ?

Line 199: Given the MSWD statistic is little used in the geosciences outside of isochron geochronology, it would be useful to also report the correlation coefficient (R-squared) values.

Equations 3 and 4: It should be noted what values were used for K, Sc, and Rc.

Line 245: Here (and elsewhere, e.g., line 376) reference is made to the size of glaciers, but the manuscript does not report quantitative measures of glacier size, but instead refers to contours of ELA. Although they may be related, ELA is not the same as 'size'.

Line 271: A citation reporting the expected $^{26}\text{Al}/^{10}\text{Be}$ ratio is needed.

Line 343: An alternative explanation is that normalized channel steepness does not linearly track erosion.

Line 414: 'this study'; it is not clear if the phrase relates to the study cited in the previous sentence or to the present manuscript.

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Line 406: The sentence is asking a question ('whether' appears twice), but then needs to end with a phrase that starts: 'depends on...'

Line 419-423: It is not clear that landscapes where glaciers are efficient agents of erosion are necessarily areas where glaciation reduces relief. It seems quite plausible that the excavation of glacial valleys/troughs could increase relief; indeed Montgomery (2002) reports that glacial valleys in the Olympic Mountains have 500 m more relief than fluvial valleys. Hence the text here needs reflect what is known empirically. Further, the following conclusion text (through line 423) is rather unsatisfactory, as these conclusions are not at all drawn from the findings presented in the manuscript. Such material could appear in the Discussion, albeit with less generalization, and a more robust discussion.

Line 424-425: References to mismatches between Holocene erosion and rock uplift seem better brought up in the Discussion; i.e., it is sufficient here to indicate there is a general agreement between erosion and rock uplift rates and to make conclusions based on that statement.

Tables: The topographic shielding factor should appear in one of the tables so that all data needed to re-produce the denudation rates are reported in the manuscript (see Frankel et al., 2010, EOS).

Editorial comments:

Line 68: there is a missing word 'pattern accreted materials'

Line 108-109 (and elsewhere): Several sentences begin with 'This'. Replacing 'This' with 'Equation 1...' or 'The value of 0.45...' would be clearer to the reader.

Line 144: lowercase 'v' in von.

Line 168: there is an extra word at the end of the sentence.

Line 220: it would be clearer to indicate that C and p are 'coefficients'

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Line 311-312: this sentence is missing an ending

Line 410: there is extra text here; initials, first names

Figure 2e. The scale bar for erosion rates isn't very useful, as it is difficult to determine the rates for the catchments with yellow-green color.

Figure 3. The legend (east-, west-side basins) should appear in the top panel, because the text pointing out the rain shadow effect does not make sense without this information.

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