

Interactive comment on “Growing topography due to contrasting rock types in a tectonically dead landscape” by Daniel Peifer et al.

Anonymous Referee #1

Received and published: 21 September 2020

This is an innovative approach to a problem that has been around for a long time, and is worthy of publication. I have three substantive comments and a few minor ones. The first substantive comment is that if the denudation rate data were stratified according to rock type it might then be that relief will be a correlate with denudation rate. After all I assume that the authors are not suggesting that the physics of erosion no longer applies, including the sine of slope function. To make the claim that you have contradicted established theory on the basis of this partial analysis is not supportable. The second is that the term 'equilibrium', 'steady state', and 'quasi-equilibrium' are used at many places without definition or explanation. This is a concern as, I am sure the authors know, the concept of equilibrium in geomorphology is, to say the least, vexed. What do you mean by these terms and how do you justify your usage? The third

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substantive issue concerns denudation rate vs. averaging time. With one exception the denudation rates have averaging times less than 0.35Ma and there are a lot much less than 0.35Ma. It is necessary in my view to stratify the denudation rate data according to various averaging times to see if you get different results. You are asking a lot of an analysis that uses such a range of averaging times (27ka to 1.1Ma). And either exclude the rate at about 1.1Ma or explain it. I have added a graph of these data. My minor comments follow: 1.line 39. Is it still called the Lachlan Fold Belt? 2.line 76 What is semitropical? It is either tropical or it is not. 3.lines 96-101. I would like to see a little more information about the accuracy of these estimates and whether or not this is a craton. It is called an ancient orogen at line 143. 4. line 342 You cannot claim that the denudation rate has persisted fro 1.1Ma on the basis of the existing analysis (see substantive comment three above).The 1.1Ma value may be an anomaly. 5. line 343. Can you make this claim about flexural-isostatic compensation without modelling of this landscape? Or are you making an argument from theory. If the latter please make this clear.

Interactive comment on Earth Surf. Dynam. Discuss., <https://doi.org/10.5194/esurf-2020-68>, 2020.

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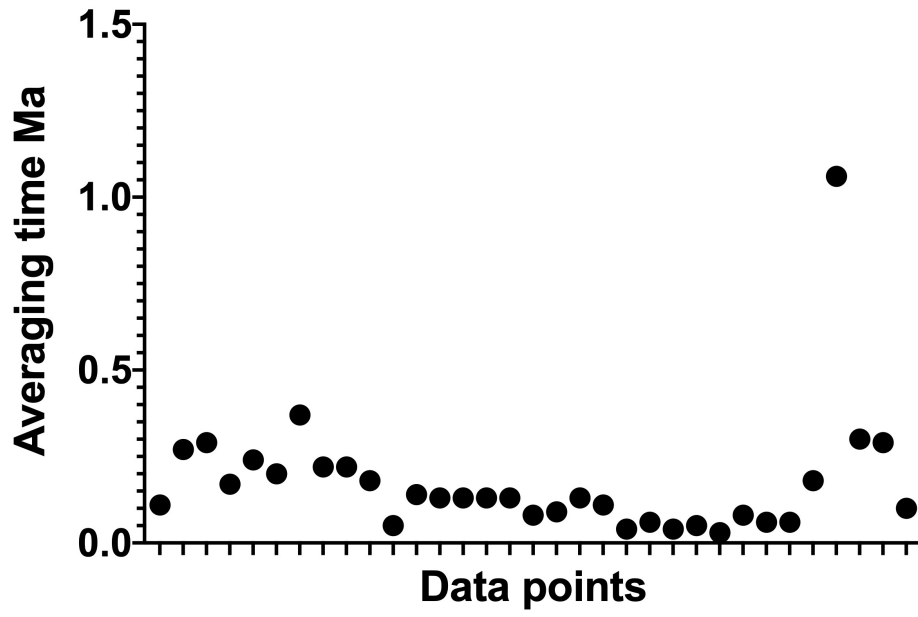


Fig. 1.