<u>Reviewer comment 1: *Interactive comment* on "Growing topography due to contrasting rock types in a tectonically dead landscape" by Daniel Peifer et al.</u>

Daniel Peifer et al.

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Response to review

We thank the reviewer 1 for her/his thoughtful comments on our manuscript. Below are our responses, which will be changed in the final manuscript submission following completion of the interactive review period. We have numbered and italicised each reviewer comment, while our responses are coloured blue.

1. 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.

We are pleased that the reviewer appreciates our work and perceives it as a valuable contribution. In the following, we address his/her comments and suggestions.

2. *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.*

All else being equal, steeper slopes should lead to more rapid denudation rates. Our study area, however, is characterised by considerable spatial variations in lithology, where resistant and more erodible rock types are exposed in a slowly eroding, humid environment. Our results show that catchments underlain by what we infer to be resistant rocks, such as physically robust and chemically inert quartzites, are linked to higher catchment-averaged topographic metrics and lower catchment-averaged denudation rates than catchments in what we infer as more erodible rock types, such as gneisses and granitic rocks with abundant feldspars which are readily weathered in such climate conditions. In this situation, we do not claim to have contradicted established theory. Instead, we show that substantial lateral variations in (inferred) rock strength in a post-orogenic setting obscure any regional relationships between catchment-averaged denudation rates and basin-wide topographic metrics and precipitation rates that might otherwise exist. Our contribution highlights that lateral and vertical variations in rock strength (in our case, inferred) are essential players in post-orogenic landscape dynamics, which have been overlooked to some degree despite widespread assertions that lithological resistance is of fundamental importance in landscape evolution. And we welcome the fact that such a viewpoint is now receiving more attention in modelling and empirical studies (e.g., Forte et al., 2016; Perne et al., 2017; Gallen, 2018; Bernard et al., 2019; Strong et al., 2019; Vasconcellos et al., 2019; Campforts et al., 2020; Gabet, 2020a, 2020b; Zondervan et al., 2020a, 2020b). Nevertheless, we will remove our statement "appear to be contradictory to established theory" [line 308] in the revision.

Following the reviewer suggestion, we have included a figure showing variations in catchment-averaged denudation rates with mean normalised channel steepness (extracted using a reference concavity of 0.45) for individual rock types (Fig. R1). As expected, we observe that catchment-averaged denudation rates and mean normalised channel steepness may increase together for several rock types, though with such small sample sizes no such relationships are statistically significant (at the $\alpha = 0.05$ level) except for catchments in phyllites. We conjecture that we did not find statistically significant positive relationships between these variables for every rock type because: (i) the relatively low range in values of topographic metrics for catchments underlain by the same rock types (for example, every catchment in gneisses and granite gneiss is characterised by low values of catchment-averaged normalised channel steepness); and (ii) internal variability in the fluvial erosion efficiency coefficient within each rock type (as discussed in the manuscript). Moreover, we note that the fluvial erosion efficiency coefficient incorporates controls other than rock type, which likely increases the internal variability in fluvial erosion efficiency in areas underlain by the same rock type. Nevertheless, we emphasise that we would expect denudation to increase together with topographic metrics in areas with the same fluvial erosion efficiency coefficient. We will add Figure R1 to the Supplemental Materials in the revision, referencing it in the Results section. Nonetheless, and we emphasise this fact, the relationships (for individual rock types) between catchment-averaged denudation rates and mean local relief, normalised channel steepness and annual precipitation rates are already apparent in Fig. 4.

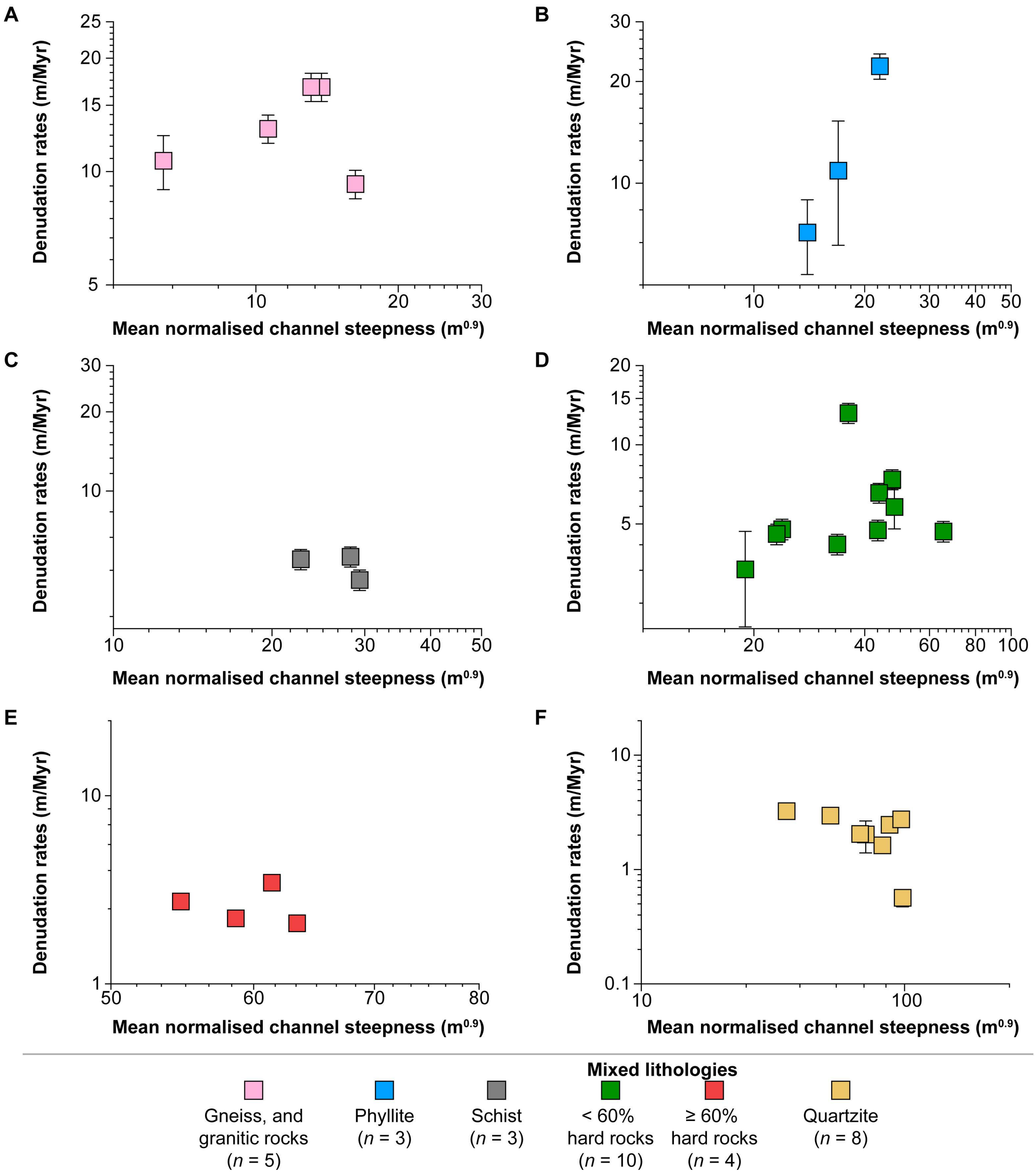

Figure R1: Variations in catchment-averaged denudation rates with mean normalised channel steepness for individual rock types. Y-error bars show measurement uncertainties in the nuclide concentration as well as uncertainties related to the scaling method. Mixed lithology refers to catchments where a single lithology does not account for ≥75% of the catchment area.

***3.** 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?*

We agree with the reviewer that concepts such as "equilibrium" and "steady state" are best used when clearly defined. In this contribution, we refer to "equilibrium" and "steady state" implying a "topographic equilibrium" in which topographic forms are constant through time and denudation rates are spatially invariant irrespective of differences in rock type or topographic relief; in this situation, rock uplift is balanced by erosion, and topographic relief is adjusted to rock strength so that everywhere is downwasting at the same rate (Hack, 1960; Montgomery, 2001). The "topographic equilibrium" concept is somewhat problematic for post-orogenic landscapes given that rock uplift is necessary to maintain equilibrium (e.g., Kooi and Beaumont, 1996), yet some post-orogenic settings have been interpreted as in a topographic steady state, perhaps driven by isostatic denudational rebound (e.g. Matmon et al., 2003; Mandal et al., 2015). We described these concepts and interpretations in lines 65-69, which we can rephrase for clarity. As a full topographic steady state is likely never achieved (Willet and Brandon, 2002), when discussing our results we used the term "quasi-equilibrium" [line 290] meaning a less strict "topographic equilibrium" (where denudation rates should be nearly spatially invariant); we interpreted our results as an indication that our landscape has not achieved "topographic equilibrium" or "quasi-equilibrium".

4. *The third 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.*

This remark is correct, and we did present one denudation rate estimate (sample ID: S5) with an average timescale much higher than the averaging timescale of all other denudation rate estimates. However, we do not consider such denudation rate estimate to be problematic for the conclusions of our study. First, measurements and averaging times (i.e., time taken for sand grains to be exhumed through the CRN production zone near the surface) are implicitly coupled and thus it is not possible to separate them; the slower the denudation rate, the longer the time averaged over. The “anomalous” denudation rate estimate (0.6 ± 0.1 m/Myr) was derived for a catchment in quartzite; all other estimates derived for catchments in quartzites yielded similarly low rates of denudation (ranging from 1.6 ± 0.2 to 3.3 ± 0.3 m/Myr). Thus there is no indication that such a denudation rate estimate is somehow incorrect. Second, when we remove the “anomalous” denudation rate estimate from our analysis, we find that all of the relationships previously found between catchment-averaged denudation rates and mean topographic metrics and precipitation rates hold (see the figure attached). That being said, our sentence “Persistence of these denudation rates (averaged over timescales up to 1.1 Myr; Table S1)” [line 341] is misleading, and we will remove such a statement in the revision.

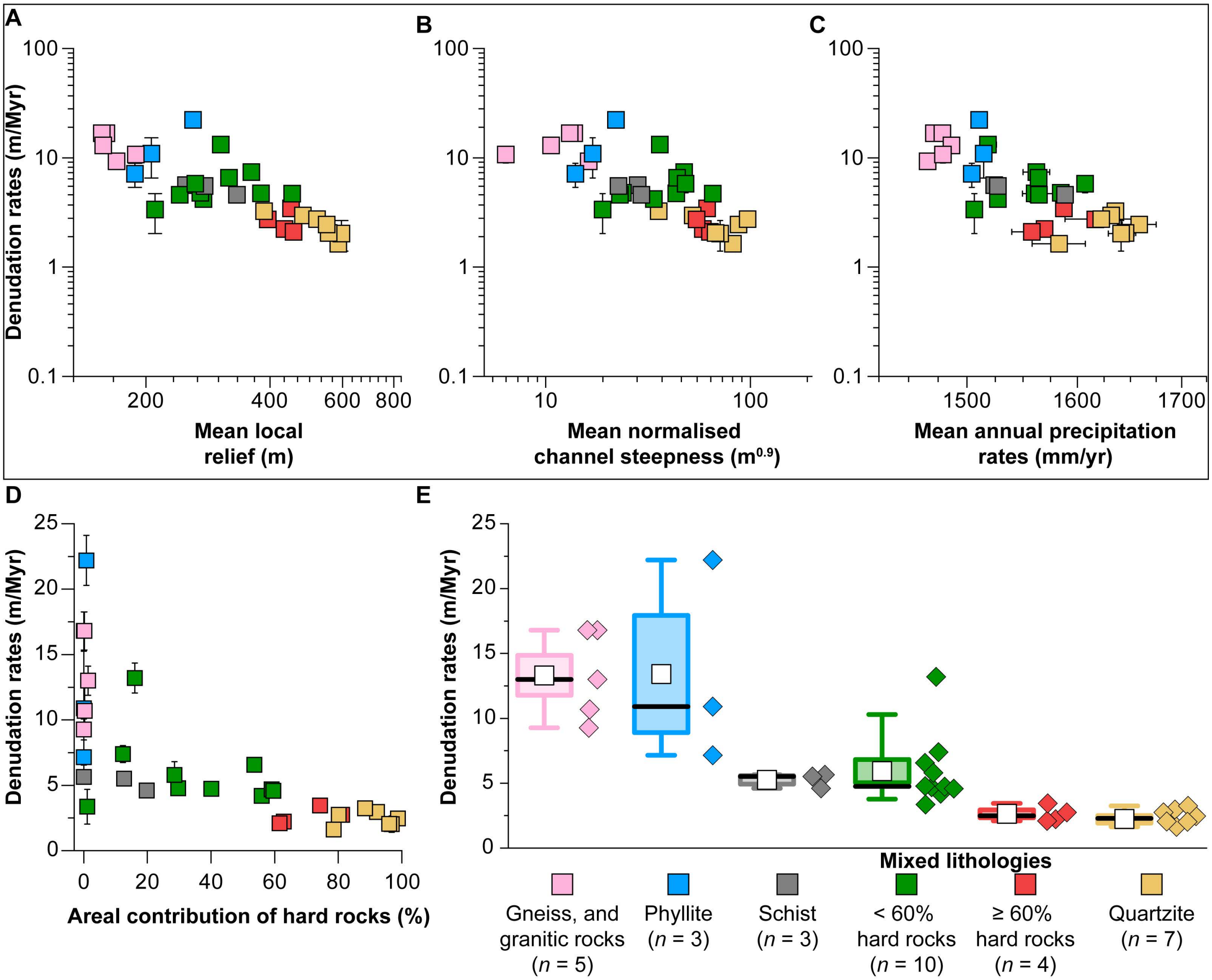

Figure R2: Links between denudation rates, geomorphic parameters, and rock type in the study area excluding sample S5. Variations in catchment-averaged denudation rates with (A) mean local relief, (B) mean normalised channel steepness, (C) mean annual precipitation rates, and (D) percentage areal contribution of resistant rocks. Y-error bars show measurement uncertainties in the nuclide concentration as well as uncertainties related to the scaling method, and X-error bars indicate the SE of the mean. (E) Variations in catchment-averaged denudation rates per rock type, with the box on the left and raw data (diamonds) on the right. Box range represents the SE of the mean, whiskers show the interval between the 10th and 90th percentiles of the data, white squares show mean values, and thick black lines exhibit median values. Mixed lithology refers to catchments where a single lithology does not account for ≥75% of the catchment area.

Minor comments

5. *Line 39. Is it still called the Lachlan Fold Belt?*

It is still referred to as Lachlan Fold Belt to the best of our knowledge.

6. *Line 76. What is semitropical? It is either tropical or it is not.*

We agree with the reviewer. We will refer in the revision as "humid subtropical" given that climate over the study area ranges from Cwa to Cwb in Köppen-Geiger's classification (Alvares et al., 2013); we will add this information in the revision.

7. *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.*

Thank you for this remark. We will add in the revision information about the accuracy of geochronological data cited. The study area is a post-orogenic landscape with a polyphase deformation history, the last episode of which was ~500-450 Myr ago, and thus it is not a craton (see lines 91-93).

8. *Line 342 You cannot claim that the denudation rate has persisted from 1.1Ma on the basis of the existing analysis (see substantive comment three above). The 1.1Ma value may be an anomaly.*

Agreed. We will remove such reference in the revision (see response 4).

9. *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.*

We are making this argument from theory, and we will make it clear in the revision.

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