Earth Surface Dynamics Discussions

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Comment on esurf-2022-54

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Referee comment on "Optimising global landscape evolution models with ^{10}Be" by Gregory A. Ruetenik et al., Earth Surf. Dynam. Discuss.,
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This study by Ruetenik et al. investigates parameters of diffusion and advection models for landscape evolution, based on a global compilation of CRN erosion rates. The authors use CRN data to optimize parameters in their landscape evolution models and investigate the distribution of parameters in respect to the different models, as well as environmental variables of precipitation and lithology. The authors advertise their method as a way to more objectively select parameter values for landscape evolution models (LEMs). I believe this study is of great interest for the geomorphological community and is generally suitable for Esurf. The parameter optimization results will be of value to justify model parameter selection and offer insights into the processes driving landscape evolution. Furthermore, the dependencies of coefficients on climate and lithology have been widely discussed, and this paper offers some valuable insights here. However, I have several points concern that should be addressed before publication.

The current approach incorporates catchments with a huge variation in drainage area. This may lead to a lot of different biases, which arise from the CRN erosion rate assumptions. I would like to see, how the results look like if you only use 10-1000km² catchments, or similar. Small catchments are prone to biases of sudden sediment input or anthropogenic disturbance. Large catchments (> 500km²) typically violate the uniform quartz fertility and sediment contribution assumptions, as well as having a negligible transport time without nuclide buildup. Also, production rate uncertainties grow significantly for large catchments due to the before mentioned reasons. I understand that this will substantially decrease the quantity of data points, but might on the other hand substantially increase the data quality.

No smoothing of the landscape is mentioned. Flow paths from DEMs especially in valley bottoms tend to have large errors. These errors typically lead to a lot of slope values = 0 and a bunch of very high values (Schwanghart and Scherler, 2017). This would bias the predicted erosion rates, if no smoothing is applied before running the LEM. I hope this was addressed and not mentioned, otherwise smoothing should be applied to the flow paths and the method described.

I like the interpretation put forward to explain the variation in coefficients with precipitation (MAP). However, without further analysis this should be stated as speculation and probably reduced in text. The problem I see is that variations in lithology with MAP are not accounted for. This means that if the catchment lithologies are not homogeneously distributed among climate zones, which can happen easily due to the high clustering of CRN measurements in certain regions, one could get significant biases. Either an analysis of MAP values with respect to the lithologies would need to be added, to show that the distribution is homogeneous, or this caveat needs to be clearly acknowledged.

Line 10-12: It should be mentioned that many LEMs simulate erosion processes with the Stream Power Incision Model (SPIM) and therefore need these parameters as input. It should not necessarily be assumed that all LEMs run this way, because there are also transport-limited or combined approaches.

Line 22: Somewhat outperforms? Please, be specific.

Line 73: Denudation is more correct than erosion in most cases. However, if you were to be strict, neither of the two terms would be correct (e.g. due to mass loss below the attenuation length of cosmogenic nuclides). The best strategy could be to have a very brief definition of what is meant by denudation or erosion, describe why a certain term is used, and then be consistent, and also remove the parenthesis in the abstract.

Line 79-81: This is more of a comment. It makes me slightly uncomfortable that two of the main assumptions are being highlighted here, for reasons that are not apparent to me, and other important assumptions (quartz fertility, uniform contribution of stream sediment proportionally to local denudation rate and area) are being folded away into the next sentence.

Line 107: There are more processes that can affect the value of n. For instance, incision process (Whipple et al., 2000), but also other flow resistance parameters, or other processes creating incision thresholds (Lague et al., 2005).

Line 155: Not sure it matters for the general outcome of the study, but SRTM30 vertical errors are many times higher than for COP30. I think the whole community should move away from using SRTM data.

Line 161-164: I think it would be more beneficial if you could make this comparison with COP30 and COP90 data, or simply use those.

Line 165: In general, very small catchments (< 10 km²) will be more prone to

disturbances by recent mass wasting (Yanites et al., 2009). It might be worth checking how/if results change if you include those catchments.

Line 190: Please list the used likelihood function as an equation. As a reader, I do not want to look it up in a separate paper.

Are uncertainties of observed erosion rates taken into account? Do you draw normally distributed observed erosion rates, or do you use the observations uncertainty in the likelihood function? Uncertainties on the observations should be taken into account in some way.

Line 230: I am not a climate specialist, but from what I get WorldClim is a bit outdated and newer, higher-resolution rasters are available (e.g. CHELSA).

Line 237: This equation includes the assumption that precipitation scales linearly with discharge. This caveat should be acknowledged.

Figure 4a: I suggest to change the color map to a perceptually uniform, colorblind-friendly color map.

Figure 5: This figure is very interesting! A color bar for MAP is missing. Also, it might be more informative to have 3 panels instead of one combined. Then the uncertainties can be shown. The current representation does not allow to assess how robust this pattern with MAP really is compared to the scatter.

Section 4.3: Also these findings are very interesting. Similarly, to figure 5 , having individual plots with uncertainties would give the reader a better understanding of the uncertainties in the analysis. Some of your findings here are quite surprising, such as the highly erodible carbonate rocks. Some of the general tendencies differ from similar studies relating topography to rock erodibility on a global-scale (Moosdorf et al., 2018; Ott, 2020). The findings here, should be discussed in light of previous work.

References: I think a mistake happened with the Marder and Gallen 2022 reference. It’s cited as published in JGR:Solid Earth, when in fact it seems like it is only available as prepint on EarthArxiv. https://eartharxiv.org/repository/view/3139/

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