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Earth Surface Dynamics
Discussions

Interactive comment on "Box canyon erosion along the Canterbury coast (New Zealand): A rapid and episodic process controlled by rainfall intensity and substrate variability" *by* Aaron Micallef et al.

Anonymous Referee #1

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Micallef et al present an intensive study of the erosion of the Canterbury Coastline of New Zealand. The authors identity what they call "box canyons" along the coastline and investigate what may be the controlling factors driving this erosion. Their methods range from geophysical surveys, landscape evolution and slope stability modeling, mapping and aerial photography, and luminescence geochronology. The authors conclude that the emission of groundwater at the surface and the resulting slope instability from the increase in pore pressure drives failure and subsequent erosion and that this can be modeled with a linearly diffusing landscape evolution model.

I think this paper should be rejected to give the authors plenty of time to reorganize the paper and reperform the science to address the problems in the paper. Here is why:

1) The Landscape evolution modeling, on which the authors' interpretation's rely on heavily, is wrong. Details are given further on in this review.

Because the evidence provided from the landscape evolution modeling is not robust, I fear the authors made need to redo their entire LEM analysis including reperforming model runs, including calibrating their parameter values and conducting sensitivity analyses. I fear that the authors will need more time than is offered during Esurf's review process. As of now, I find the authors statements in the discussion unconvincing.

2) The data presented are not fully synthesized into a clear and complete story or analysis. The challenge here is that it is hard to understand how each of the data the authors present ties into the overall picture of 'box canyon' erosion. As such, it is not clear as the paper is currently written what the contribution of the paper is towards understanding Earth surface dynamics. After reading the paper, the only things I got out of it are that the authors did a lot of work collecting a lot of data, but I can't say I understand what the contribution is. Two good examples are the luminescence ages and the geophysical resistivity surveying. The ages are used mostly in a passing way and the geophysical resistivity doesn't seem like it was very successful, so it not clear why it is included?

3) The term Box Canyon doesn't bring to mind the features that the study focuses on. I understand box canyons to be canyons with vertical walls, flat bottoms, and generally ingress and egress is only possible through the mouth of the canyon. The classic box canyon are the narrow canyons typically associated with the arid western United States. I think what the authors are studying are actually gullies that progress into canyons when they collect enough drainage area. As the authors note in Lines 383-385, the younger and lower-discharge/drainage areas/stream orders of these features have V and U shaped cross sectional profiles. Whereas the typical box canyons I have

seen in the field continue to have flat bottoms and vertical walls even at low stream orders.

I think this will be a problem for the authors in that researchers looking for information on box canyons will not consider this work relevant, and those studying coastal gully erosion will miss this paper due to the title and mislabeling of the features of interest. Instead, I think the authors should rewrite this paper while avoiding the use of the term box canyon. Either referring to these as coastal cliff gullies, headward cliff erosion, or some new term the author's come up with.

4) The details and protocol on the luminescence age laboratory methods are great; the information on what was dated and why is not.

After reading the paper, I could not tell what landform the authors dated using luminescence. This is pretty important for interpreting the ages.

As I do not have expertise in slope stability modeling or geophysical resistivity surveys, I am not going to comment on them.

Landscape evolution modeling:

The authors use two models to simulate the erosion of the Canterbury Coast. The first is the stream power model and the second is a linear diffusion model. Neither of these is likely appropriate to simulate the erosion here, nor do the authors provide the necessary analysis to justify their use. First, the stream power model is really for detachment-limited landscapes. However, the authors do not make a case that the erosion and sediment transport conditions here justify the use of the stream power model. Particularly, as the presence of major slope failures and alluvium in the channels suggests that a significant sediment cover effect may be happening here.

Next, the authors use a linear diffusion model as another endmember. However, with the presence of major slope failures, which are significantly non-local and thus not diffusive, it is hard to justify its use and the authors do not provide this justification in

the manuscript. The discussion section completely disregards this fact and overall is not convincing.

Another problem in the methodology occurs with the values used in the model. One problem is when the authors equate the values for K and D in the stream power model and the linear diffusion model. These values represent very physically processes and are likely very different, by orders of magnitude in some cases. Their use of also assuming that K/D is proportional to the seepage flux and surface water shear stress, while I am open to the idea, isn't backed up with a reference or proof of concept. Next the authors state that they obtain the values for M and Tau_t "by trial and error." What does this mean? Did they just pick values until they obtained behavior they wanted? Because the the authors picked values by trial and error, rather than by constraining them from some means, it is not clear if the modeling results presented are meaningful. Normally, when one does not know what input parameters should be, a sensitivity analysis is needed to evaluate the outcomes from a full range of possible values for a specific parameter.

Finally, there is no sensitivity analysis given on how model outputs change with change in the input parameters. This is needed to interpret the results from landscape evolution modeling. On line 297, it says that the authors are testing the models, but these methods are not really a formal test of these LEMs as currently written.

One line 337: Actually, there probably is a way in landlab to incorporate sand lenses, but this would essentially involve developing a new landlab component to do so.

Why is Equation 10 is the same as Equation 7?

Luminescence Dating I think that the laboratory methods in the supplemental is generally well done. I like the level of detail and the well-documented dose recovery and fading tests. It seems like these might have been challenging samples.

Dose recovery tests:

These values don't seem all that great to me. I generally try changing my protocol if the dose recovery ratio comes out worse than 1.10 or 0.9. What could be causes of this not-great dose recovery? Is this typical for feldspar from New Zealand?

Those g-values might not be insignificant. If you perform the fading correction on the k-feldspar for NZ14A, do the ages agree with the polymineral?

Perhaps the discrepancy in age between the polymineral fine grain and the k-feldspar (for NZ14A) is due to the presence of unknown minerals in the polymineral aliquots? It is possible that there are minerals in there with unknown pIRIR properties that might be transferring charge in unexpected ways?

One other thing that might be useful is if the authors could report the distribution of the residual dose. If there is large variance, maybe this could explain the discrepancy of the polymineral and feldspar ages.

One fairly big issue is that there is limited reporting of what landform the luminescence samples were collected from. The locations are given on the aerial photo, but I cannot determine if they were collected from sediment in the channel or from the walls of the canyon, etc. Because of this, it is hard to interpret the interpretations of partial bleaching fully. As the pIR290 ages overestimate the pIR225 ages, there is likely to be partial bleaching, but how much? Are the ages themselves suspect or not? How do you know? Do you need a lower temperature pIR signal to get a better bleached age?

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