Earth Surf. Dynam. Discuss., referee comment RC2
https://doi.org/10.5194/esurf-2022-47-RC2, 2022

Comment on esurf-2022-47

Alexander Densmore (Referee)

Referee comment on "Steady-state forms of channel profiles shaped by debris flow and fluvial processes" by Luke A. McGuire et al., Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2022-47-RC2, 2022

This is an interesting and timely manuscript that seeks to develop an approach for modelling the combined effects of debris-flow and fluvial incision on river long profiles. Debris flows have long been recognised as important agents of erosion and sediment transport in many mountain catchments, but they have typically been left out of attempts to model channel erosion or landscape evolution. The authors have taken some initial steps toward that goal. The manuscript is highly relevant to the journal and will, I think, be of interest to the journal readership.

The manuscript is well-written and well-presented overall, but I do have some comments on the text and figures. Most of these are fairly minor and should be easy for the authors to address. One more substantive comment is that I was surprised to see that neither the process-based nor the empirical model conserve mass, and only the empirical model considers flow volume variation downstream (although this is imposed as being monotonic). Work in the Illgraben catchment and elsewhere has shown that flows can both lose and gain volume downstream, so while the imposed rule from Santi and Morandi (2013) is certainly a place to start, it would be good to see a little more context around its usage. More importantly, I wondered about comparing the two models given that one assumes the flow volume is uniform downstream and the other does not.

The introduction, while clear and pretty easy to follow, lays out a few different motivations for the work that don't necessarily all track through the rest of the manuscript. I think this could be streamlined and focused on what the authors are actually doing here. For example, the mention of the need to 'identify robust topographic signatures of debris-flow erosion' isn't something that is addressed here – instead, they are taking a single measure (slope increases monotonically upstream but at a decreasing rate, as in Fig 1) as that signature. It would have been good to be more clear about this up front. Fig 1 is cited as an example of this, but it's not clear how widespread that morphology is; a parameter A_df and an equation are introduced in the caption but aren't actually described in the text until p. 10, which could be confusing for the reader. Elsewhere in the intro, there is a goal (lines 86-88) which isn't really clearly motivated at that point in the manuscript, and a separate set of objectives (91-97), but then there are two other goals on lines 239-242 that overlap with the third and fourth objectives. I’d encourage the authors to restructure the introduction to keep the focus on what they are going to do here (e.g., while I agree that flow frequency is critical as mentioned on lines 78-80, that’s not something they address), and to motivate the model comparison that is at the heart of this manuscript.

Some more specific comments by line number:

line 2: ‘rates and spatial patterns of landscape evolution by debris flows’ is ambiguous – should this be ‘…of erosion by debris flows’?

36-40: these sentences are written as if these are two different concepts, but aren’t they equivalent?

45: that identification has (apparently) already been made in line 34. This is repeated again in lines 49-50

50: here and throughout the manuscript, I would suggest using ‘rock uplift rate’ consistently. There are also places where U is variously used for ‘uplift rate’ or just ‘uplift’, and again I think this needs to be made consistent

71-73: while I agree, I think this sentence is also missing the idea that this will happen over multiple flows which themselves are drawn from distributions of volume and flow properties, if we are interested in landscape evolution. This sentence could be read as being about properties in a single flow that’s traversing the landscape

79: I think this should read ‘For example, the frequency…has been shown to be a key factor’

130: given that flow volumes and properties are not constant from flow to flow but follow distributions, I’m not sure what is meant by a ‘representative’ flow. I think it’s really important to be explicit that eqn 3 is defined for a single flow, and that it is being applied over a series of flows that are assumed (rightly or wrongly) to be identical. That’s a really restrictive assumption and I think it needs to be made more obvious. The authors are later clear about how future work could use these distributions (lines 235-237), which is great.

140: again this is assuming that h is constant for a given channel location in a single flow. And given that this is a representative value for h rather than the true flow thickness over time (as used in eqn 3) I wondered whether a different symbol would make sense…

167: this is potentially confusing because later D is allowed to vary – not sure why a single value is specified here

184-187: our work at the Illgraben (Schuerch et al. 2011 Geology) documented flow volume variations with distance downstream, both positive and negative as flows traversed the lower part of the catchment and the fan

187-189: in other words, mass is not conserved, right?

195-199: I think I followed this, but it could perhaps be more clearly explained

250-266: this partly repeats text in the intro, but this is actually clearer and introduces eqn 17 which has already been shown. I think this text should be merged with the intro so that Fig 1 can be better understood

274: up to here the tense has been present (We assess… we compute), but here it changes to past. This should be kept consistent

272-273: I don't disagree with this criterion… but it's kind of hidden here, despite the fact that this becomes the primary way in which the authors accept or reject model runs. I think this needs to be highlighted (perhaps in the intro where they are describing what they are trying to match)

277: parameter k_e is introduced here but eqn 2 is in the form of K – it's not obvious where k_e has come from. A reference to appendix A might help

288: I think I missed the flow frequency – is there a flow every year? Or every timestep?

327: 'of the steady-state channel profiles' – I'm not sure if this is referring to the model results or to observations

328: I think this should be 'minimum drainage area'?

336: ‘increases with rock uplift rate’. More broadly, while I agree with the overall sense of the argument here and this is certainly a reasonable supposition (all else being equal), I’m not sure what is gained by exploring a single, arbitrary relationship between U and k_df. This might just need some more contextual information. Flow frequency should also depend very strongly on where you are in the catchment…

343: I don’t think the ‘e.g.’ makes sense here, because k_df is explicitly defined as the product of an erodibility and flow frequency

350: ‘rock uplift rate U’

375: I’m not sure what is meant by ‘infrequent instances’ – is this referring to individual numerical experiments in the sensitivity tests, or localised parts of the profile within a single experiment, or…?

381: if E_df needs to decrease slightly with A, why does that imply that h must increase with A? That seems to run counter to eqn 4

383: reference to flow discharge is potentially confusing here, because gamma has been defined in terms of downstream changes in flow volume, not discharge. I agree that they might be related, but that relationship isn’t necessarily simple (or the same for all flows)

398: ‘rock uplift rate’

406: I’m a little confused by this leading statement – isn’t this conceptual model what has been assumed? In which case, how can the results be seen as supporting this conceptual model? I suppose what the authors are saying is that there are parameter sets for which this assumed model form can reproduce aspects of observed long profiles – that’s a more restrictive statement (which they make elsewhere). That doesn’t rule out other conceptual models which might also reproduce those long profile aspects, of course

472: because we are considering landscape evolution, then flow volume will change not just by sediment entrainment from the bed and banks but also by bedrock erosion

479: ‘where changes in flow volume can be neglected’ – I’m not sure when that would ever be the case, given that real flows have continuously-varying downstream lag rates which can be both positive and negative (and can both erode and deposit within a single channel cross section)

482-485: that’s true, although there isn’t much mention throughout of t_p in eqn 4 – this seems like a pretty big unknown if the goal is to model channel erosion over kyr-Myr time scales. I guess this comes back to the idea of a ‘representative’ debris flow, which elsewhere the authors also refer to as a ‘characteristic’ flow, and what that means

487: I’m not sure I’d call this a landscape evolution model – it’s really a model of channel profile evolution

Appendix A: D_eff has units of m which should be repeated here

Fig 1: the equation for the best-fit curves is going to be confusing for the reader because it doesn’t show up again in the text until p. 10 – as noted above I suggest moving that material to the intro.

Fig 4: the panels here seem to show the same thing as Fig 2 and don’t match the caption.

Fig 5,8: I think it would be useful to include the units for each parameter on the axis labels

Fig 6,9: again it would be useful to include the units for each parameter, either in the legend or caption

Fig 7,10: ‘Uplift’ should be ‘Rock uplift rate’ on the x-axis, and the units are given in m/kyr here but in m/yr in most other parts of the manuscript; the same comment applies to Fig 10.