Earth Surface
Dynamics
Discussions

Interactive comment on "Developing and evaluating a theory for the lateral erosion of bedrock channels for use in landscape evolution models" *by* Abigail L. Langston and Gregory E. Tucker

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This article aims at developing and implementing a model of lateral mobility of rivers in long-term landscape evolution model of mountain ranges. This is timely needed as the lateral mobility of river is now known to play a significant contribution in landscape reshaping, and as most current numerical models of landscape evolution predict valley bottom that are simply 1 pixel wide and fixed in time.

The article introduces two aspects:

-A theoretical formulation in which lateral channel mobility is assumed to be proportional to the centripetal energy expenditure of water.

-A numerical implementation on a fixed regular grid with a description of the solutions to overcome the limits in describing the migration of a vertical front (valley side) on a horizontal grid.

Then the model is used to explore some basic simulations (steady configuration, transient dynamics) to "see" how it looks. The paper does not try to address a specific scientific question, but more a technical/methodological issue, which is fine with me. The challenge is important, given that if one could have a realistic model for channel mobility in large scale/long-term landscape evolution models, one could properly addresses issues such as drainage capture, valley bottom formation, drainage network advection, fold bevelling etc. . .Overall the MS is well written and clear to follow.

The problem is that I find that the numerical implementation have several flaws which prevent me from trusting the model outcome at this stage. Numerical modellers all know that it is very easy to create landscapes that look ok if you have some large degree of freedom in choosing your model parameters (erodibility, runoff, channel width coefficient etc. . .). Here, the modelling results look ok, as the model is tuned to looks right, but that does not mean that the dynamics and timing are relevant to natural systems, which is what we ultimately expect from a landscape evolution model. And because there's no real attempt to validate model predictions against quantified observables, it is very difficult, given some of the flaw in the implementation, to infer reliable results pertaining to the dynamics of natural mountain valleys.

I've made a lot of comments in order to help the authors improve their model, and I really hope that they will sort out the issues I raise or demonstrate that they are not that important, as it is indeed important to tackle the issue of channel mobility in landscape evolution models.

GENERAL ISSUES ON THE MODEL:

On the model description there is a fundamental inconsistency that needs to be sorted out: it is the difference between the local channel width and the pixel size. The problem can be treated in 2 ways on a fixed grid:

- Hydrodynamic models (either operating on reach or landscape scale, e.g., CAESAR (Coulthard et al., 2013), EROS (Davy and Lague, 2009; Croissant et al., in press)): the pixel size is significantly smaller than the channel width (on which you actually resolve "true bank erosion") and for which channel width is a self-emerging property.

-Non-hydrodynamic models: the pixel size is ALWAYS larger than the channel width and channel width is imposed by an external equation. In this case your channel may actually sit anywhere in the pixel, and may not, for instance be in contact with the neighbouring pixel (in which case, I don't see why it would erode laterally).

The model presented here is a non-hydrodynamic model aiming at including a "channel mobility" component. This is a great idea, and indeed barely addressed by landscape evolution models. But it is not strictly speaking a "bank erosion model" as it does not resolve 2D flow hydrodynamics. Yet there are many instances in the paper, where the model has some kind of schizophrenic behavior between the two types of models:

- First it uses a relatively small pixel size (10 m), which assumes practically that the channel width must never be larger than 10 m. Unfortunately, this condition is not verified all the time (unless I've missed something in the calculations): the basic model uses a drainage area of 20000 m^2, which coupled with a runoff of 36 mm/hr, and kw=10, gives Wmin = 4.5 m. However, multiplying the drainage area to 160000m^2 (section 4.2.1) violates this assumption from the inlet of the model Wmin = 12.65 m. At this point flow should be partitioned over 2 pixels to correctly resolve the equations. I don't know how this bias affect the model predictions, and how such a model could be uspcaled to larger catchments where channel width would be several pixel wide (here we're dealing with small catchments of $\sim$km^2 size).

- There is no real notion of "bank" in the model given that the channel is defined at sub-

C3

grid, but rather some kind of "valley side". This makes it difficult to directly relate lateral erosion "end members" (fig.1 section 3.1) to actual physical processes. These are more numerical tricks to resolve vertical feature horizontal migration on fixed horizontal grid, but whose relevance to natural processes is quite debatable. They introduce artificial thresholds in model dynamics whose consequences are not explored thoroughly.

- The model implementation assumes that the channel is always in contact with the neighbour node (there is systematically lateral erosion), which contradicts the underlying assumption that channel width is smaller than the pixel size.

- The model does not account for lateral deposition which is an important driver of channel migration (but that's not the most critical point)

On top of this, there is an important limitation in the "undercutting- slump" model in assuming that flow depth only depend on discharge (eq. 30) while it must depend on slope (and width, but given that it is fixed by discharge in the model, there's no way to do better).

Hence I see at least two components missing in the model: 1 : A proper way to deal with cases in which the channel width becomes larger than the pixel size (as predicted by kwQˆ0.5): either you increase the pixel size (but this also increases the "numerical" threshold for channel migration), or you introduce some kind of flow partitioning/simplified 2D hydrodynamics (but then we're very close to existing models like CAESAR or EROS). I know width is lumped in the model through kw, but either you assume your channel width is never larger than 10 m (that's quite a limiting factor), or you have to partition the flow over several pixels. 2 : Adding a way to either explicitly or implicitly account for the sub-pixel position of the channel. For instance a kind of likelihood of bank erosion (which is a function of the ratio of channel width to pixel size) with an asymmetric probability related to alongstream curvature.

I also note that, even if it is not common practice in the litterature of landscape evolution models (it should), it is important for any numerical model implementation, to

C4

demonstrate that the model results do not systematically depend on grid size (within limits) and time-step, or to acknowledge this dependency and demonstrate how it impact results. Also, I would also like to see the model evolve from an initial condition with the lateral erosion "on", and not activated only when the landscape and drainage is already organized: if a model works, it works all the time, and actually exploring drainage development on a plateau could tell us whether you generate realistic patterns or not.

Other comments Title: it is currently slightly misleading as there is no real evaluation nor comparison of the model prediction with actual results, and the link with the mechanics of bedrock channel bank erosion are extremely tenuous or not really clear. Something like: "Implementing lateral mobility of channels in landscape evolution" models would more represent the actual content of the paper.

Missing literature:

The CAESAR numerical model, although dedicated to reach scale (but there are also a few catchment scale simulations) should absolutely be cited and studied as it is relevant for the bank erosion law and the use of curvature. For instance, Coulthard et al., 2013, ESPL, Integrating the LISFLOOD-FP 2D hydrodynamic model with the CAESAR model: implications for modelling landscape evolution.

Relevant literature that you may or may not want to include (very recent papers): Eros numerical model (new version) : Croissant et al., in press, Nature Geosciences : illustrating the critical role of dynamic channel width in exporting sediment in bedrock valleys.

Detailed comments: P2 L23 : I tend to disagree with this statement: some models of channel width adjustment have been proposed, but none can actually fully explain the variety of responses found in nature (see Lague, 2014 ESPL, for a synthesis). As for incision thresholds, which can only been adequately accounted for if discharge variability is explicitly modelled, only two models that I know of properly account for it (CHILD, EROS and LANDLAB ?).

P2 L24: rarely: could you specify which models actually includes it ?

Section 2.1 : in this section, the author should emphasize more systematically that the "theory" presented is an assumption of the model. Too often, it is presented almost as a fact or acknowledged theory: P4L23: "vertical erosion rate is derived from", p5L2 "the rate of vertical erosion scales as".

P5L7 : given the emphasis in the introduction of the role of dynamic width, I'm surprised that you introduce a fixed width scaling with discharge without more justification. The width scaling should appear as an independent equation number so that it can be discussed much more extensively in the paper.

P6L16 : I fail to follow the logic in relating a higher Kl/Kv to the work of Harsthorn et al. 2002 (who studied only one reach with variable discharge, and highlighted the role of bed cover not runoff per se) and to the increase in climate storminess described by Stark et al., which is not accounted for in your description of R (knowing that an increase in climate storminess can very likely affect kw too).

P6L20 : kw : we need more info on the range of possible values. Is this value extracted from alluvial channels (as would suggest the Leopold & Maddock, reference) which is inconsistent with your approach of "bedrock channels" as stated in the title, or from bedrock channels (which your model description seems to imply) ? You should also state at some point that kw is assumed fixed, which is a very strong assumption given that width variation with incision rate are very often observed or predicted in models explicitly modelling bed and bank erosion via an hydrodynamic model (e.g., Lague, 2014; Croissant et al., in press).

Questions on numerical implementation

CRITICAL : Is there an internal "safety check" that verifies that the actual channel width in the primary node (kw x Qw^0.5) is systematically smaller than the pixel size ? otherwise you violate some of your assumptions.

Figure 1: the legend is quite hard to follow. Similarly there are several black arrows so it's hard to clearly understand which one you're referring to in the legend. Please revise this significantly for better clarity. There is also a typo ("after after" L 6). I suggest for instance to give a different color to the area being eroded in the lateral node to make it clearer.

Figure 1b: it is not clear why you choose to have the neighbouring node set to the downstream elevation node (Z_d), not the primary node (Z_n). It seems to me that this probably drives artificial mobility in the model without a real justification.

Lateral erosion : If I understand well, lateral erosion only occurs on a D4 grid, never for diagonal pixels ? Would this not generate asymmetric behaviour between orthogonal and diagonal directions favouring one orientation but not the other ?

P7L25: I note that if you add a subpixel description of the actual channel position, you would have a much more continuous description of the curvature (albeit with the issue of scale remaining).

P7L25: I fail to really understand this part ? how can you get a curvature with a straight channel ? Again this seems like assuming that you have a sub-pixel variability in the channel position, yet, you do not explicitely account for it and you do not have a model for it.

P7L30 : H only dependent on Q : incorrect assumption to have H independent of slope which can vary alongstream and through time. Why can't you use your local width, slope and friction to backcalculate the actual local flow depth ?

P7L32 : does all the sediment behaves according to eqs (1) to (6) or is there specific treatment for the collapsed material as mentioned in Fig 1d: 'collapse material" behaves as washload , which would potentially imply that it nevers redeposit in the channel ? More generally, I find that the behaviour of the sediment is not always clear. (note having reread the MS several time, I now understand, but it's really not clear on

the first or second read)

End-member formulation

P8L10: I think it would be way more justifiable to present the end-member as exploring lateral erosion laws scaling with bank height (as in Coulthard et al., 2013) or flow depth (as in many hydrodynamic models, Delft3D etc. . .), and using this terminology all along the paper, and trying to relate these to actual natural processes in the discussion section, rather than the other way around. Because, the link with actual processes is quite tenuous, and there is some kind of untold story that the actual erosion model is dependent on the rock resistance chosen in the model. It would be great to beef up the literature here, discussing for comparison how bank erosion is calculated in CAESAR or EROS.

Model experiments:

P8L22: Why cannot you use the model with lateral mobility from the beginning ? what kind of hillslope erosion law is used ?

how were the parameters chosen ? e.g., erodibility, alpha as well as the Kl/Kv ratio and a runoff rate of 14 mm/hr or 36 mm/hr ? I note that 36 mm/hr amounts at 315 m/yr of runoff. . . Given, that nowhere on earth you have this kind of mean annual runoff, I suspect that this is some kind of effective runoff, but it is really not clear. Given that you do not chose the runoff, ending up with such large values should be better discussed. Seems that to get results that look good, you have to end up using boundary conditions that are unrealistic More generally, it is not clear if your choice of parameter is such that the landscape & mobility looks "ok", or if at least, some can be independently chosen ? Maybe you should present a reference catchment on which model results could be compared.

Given that your parameter choice seems quite ad hoc, I find it quite misleading/dangerous to present "real ages" in the numerical simulations and in the results.

P11L14: maybe you could cite Davy and Lague (2009) in which there's the first derivation of the slope-area relationship in the general case of erosion-deposition with a transport distance.

P11L15: If you had an independent calibration of your elementary laws, which, when implemented in the numerical model, generates realistic geometries, then you would demonstrate that your new lateral erosion theory and its implementation successfully produce bedrock valleys significantly larger than the channel that created them. But right now, the model is calibrated and constructed to generate these wide valleys, so obviously...you get them... We are really bordering circular reasoning here.

P12L21: which hillslope processes, you did not describe them and in the discussion you seem to imply that there are no hillslope processes operating.

P13L13 : careful with the notion of threshold: this is not a true threshold in terms of physical processes (there are no thresholds in the constitutive equations of the problem), but solely an artificial threshold introduced by the numerical implementation and which depends on grid size.

Section 4.2.1 : this section needs to be revised in the light that the predicted channel width is very likely larger than the actual pixel width which violates a fundamental assumption of the model (see general comments)

P13L27 : this is an interesting feedback.

P14L25 : the increase in lateral erosion rate could be quite dependent on the incorrect assumption that H only varies with discharge (while it varies also with slope), and the flow partitioning errors as at this stage the "channel" theoretically occupies at least 2 pixels which means that discharge should not be as high than predicted given that it is focused in a single pixel.

P15 & P16 : in this section, assuming that channels only accommodate the increased sediment flux by varying their slope without varying their width (in that case kw), is a

pretty strong simplification. Croissant et al., in press at Nature Geosciences have recently demonstrated how important are dynamic width variations (i.e., kw variations) in boosting the transport capacity of mountain rivers, slope variations having secondary effects. This effect, important in driving channel reincision of deposits, terrace generation and channel mobility cannot be captured in your modelling framework if you assume kw is fixed.

P16L25 : here, assuming that water depth does not depend on slope overpredicts lateral erosion with respect to vertical erosion as water depth should decrease with slope for given discharge.

Discussion: P17L31: the valley width emerging from any of the lateral erosion model completely depends on the model parametrization which is not properly justified at present. You could obtain narrower valleys with the undercutting-slump model algorithm if the lateral erodibility is much smaller.

P19L20: this is debatable: alpha depends on runoff and settling velocity which can easily be estimated for natural systems. Only d* is more tricky. Setting runoff and settling velocity should set the value of alpha, not the other way around. At least you're sure to evolve in a range of parameters that is realistic.