Authors' Response to Reviewer 2

Reviewer 2: *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.*

Authors: Thank you for recognizing the importance of the issue we address in the paper and taking the time to make many relevant and helpful comments to improve our manuscript.

Reviewer 2: *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.*

Authors: We readily admit that this is a model without a quantitative test... yet. Qualitative reproduction of commonly observed landforms may be a weak test, but it an essential one: if a model does not pass that bar, then it is clearly a failure (albeit possibly an instructive one). Moreover, the history of science is full of examples in which theory precedes empirical confirmation. Nonetheless, it is fair to expect us to provide some ideas on how this model could be tested, and we now do so in the discussion section.

Reviewer 2: *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 m2 , which coupled with a runoff of 36 mm/hr, and kw=10, gives Wmin = 4.5 m. However, multiplying the drainage area to 160000m2 (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 affects 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 km2 size).

Authors: It is important to recognize that channel width is not explicitly represented in the model we describe. Rather, it is one element of the lumped parameters K_v and K_l. The channel-width scaling parameter values we discuss, and which the reviewer quotes, are used only in the estimation of reasonable ranges for these parameters. So it is not really correct to say that the model channels are wider in some instances than their grid cells, because channels have no explicitly defined width (though the possibility exists that the "implied width" could potentially be wider than a cell: a problem common to all non-hydrodynamic LEMs). We have added text to section describing vertical and lateral erosion equations to make this point.

The reviewer remarks that grid cells are "relatively small". The word "relatively" is important here. Presumably he means small relative to what one would expect for channel width. If we consider that channel width tends to scale as the square root of drainage area, then all else equal it should also scale with the characteristic length of the drainage basin, or in the case of a model, the side length of the domain. Double this characteristic length scale, and you should also double the expected width of the largest channel. Given this scaling relation, it does not really make sense to speak in terms of the absolute size of model grid cell. Rather, it makes more sense to consider grid size in relation to the scale of the largest drainage basin. In that respect, our model resolution (considered as the ratio of cell size to domain size) is not notably different from that of most other non-hydrodynamic LEMs.

Nonetheless, the reviewer raises an important general critique of LEMs that use single-direction flow-routing schemes: it is possible in principle to have an "implied width" (implied, that is, by the width-discharge relation embedded in K) that is larger than a grid-cell size. This issue is not unique to our particular model; any non-hydrodynamic LEM with sufficient resolution would face the same inconsistency. We agree that it is an issue that should be resolved (interestingly, the same kind of issue arises in other fields, such as the representation of convection cells in atmospheric models or turbulent eddies in 3D flow models). However, our intent here lies not in re-writing the hydrology parameterization for LEMs, but rather with the more narrow goal of investigating how lateral erosion might be implemented within the context of an otherwise fairly generic and common model formulation, without excessive complexity. Therefore, while we acknowledge the "channel in cell" issue as a general problem for non-hydrodynamic LEMs (and indeed related to similar issues we don't think this paper is the right place to roll out a proposed solution. However, we have added text in the supplementary materials section that notes the existence of this issue and the need for an ultimate solution.

Reviewer 2: - *There is no real notion of "bank" in the model given that the channel is defined at sub 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.*

Authors: Perhaps so, but note that the physical sciences are full of such "tricks". What, for example, is the "true" meaning of viscosity in a liquid? The linear viscosity law is just a parameterization ("trick") too, which happens to work well for certain materials under a certain range of conditions (and fails for others). Maybe our trick will ultimately prove to perform poorly when compared with data, yet by introducing it we draw attention to what we hypothesize is an important process in valley widening: the physical disaggregation of material due to erosional undermining and collapse.

The model end member section was revised (P9L16-30) to emphasize that in one formulation (total block erosion), lateral erosion scales with valley wall height and in the other (undercutting slump), lateral erosion is independent of valley wall height. All discussion of relevance to natural processes is moved to the discussion section.

Reviewer 2: *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.*

Authors: See response below under "two components missing in the model", item 2.

Reviewer 2: *The model does not account for lateral deposition which is an important driver of channel migration (but that's not the most critical point)*

Authors: Sustained deposition on surfaces dipping more than several degrees is so rare that we consider it a reasonable thing to neglect.

Reviewer 2: *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).*

Authors: We neglect the influence of slope on water depth because its influence is much less than that of discharge. For example, the Manning equation states that depth scales like $Q^{3/5}$ but scales like $S^{3/10}$.

Reviewer 2: *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* $kwQ^{0.5}$*): 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.*

Authors: See prior responses regarding the treatment of width in our model.

Reviewer 2: *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 along stream curvature.*

Authors: We are aware of course of the excellent work by Hancock and Anderson (2002) that relates valley widening rate to the ratio of channel to valley width. We had originally avoided implementing such a rule because, as noted earlier, the model does not explicitly define channel width. However, even without tracking width explicitly, one could assume $W \sim A^{1/2}$ scaling and therefore allow a similar scaling in lateral erosion rate. One way to address this issue in the model is to multiply the erosion rate by the ratio of channel width/dx so that lateral erosion is decreased in narrow streams and enhanced in larger streams. We have created a version of the model that implements this rule, and run a series test models to evaluate the result. As expected, there is less lateral erosion in the smaller streams in the upper parts of the model, but little change in valley width and channel mobility in the lower parts of the channel. These figures and discussion of the modified model are included in the supplementary materials.

As to the notion of tracking sub-cell channel position: We are delighted that the manuscript is already provoking new ideas about how to address the problem that we've set out to highlight. Indeed, we spent a long time considering various approaches, including one in which channel position within a cell is explicitly tracked. We ultimately settled on the alternative method the paper describes out of considerations for simplicity. Complexity in theory and models comes at a cost. Our philosophy is that the goal of science is to understand things, and if a model becomes too complex to understand, well then all we've succeeded in doing is creating yet another thing we don't understand. In our view, the justification for adding something to a model should be a clear demonstration that the model doesn't "work" (i.e., account for an observed phenomenon) without that thing. So, we've leaned on the side of simplicity. If this paper stimulates others to come up with a demonstrably better approach, then we'll have succeeded in one of our main objectives.

Reviewer 2: *I also note that, even if it is not common practice in the literature of landscape* *evolution models (it should), it is important for any numerical model implementation, to 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.*

Authors: A brief overview of model runs with the same domain size and grid size of 15 m and 20 m is included in the supplementary materials.

Reviewer 2: *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.*

Authors: The models can be run from an initial condition with lateral erosion. There is no observable difference in model topography. Figures and a brief discussion are included in the supplementary materials.

Reviewer 2: *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.*

Authors: We have changed the title slightly to more accurately reflect the content of the paper. The new title of the paper is: Developing and exploring a theory for the lateral erosion of bedrock channels for use in landscape evolution models.

Reviewer 2: *Missing literature: The CAESAR numerical model...*

Authors: Missing literature added in background section and throughout the manuscript as noted below.

Reviewer 2: *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 ?).*

Authors: Changed text to read: While theories that account for dynamic adjustment to bedrock channel width continue to be refined (Lague, 2014), landscape evolution models that include a relationship between sediment size and cover (e.g. Sklar and Dietrich, 2004), and incision thresholds in bedrock channels (Tucker et al, 2001; Crave and Davy, 2001; Tucker et al., 2013) are available and widely used (Tucker and Hancock, 2013).

Reviewer 2: *P2 L24: rarely: could you specify which models actually includes it?*

Authors: Changed text to read: "existing models do not address the lateral erosion of bedrock channel walls"

Reviewer 2: *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:*

Authors: Changed text to clarify to readers that vertical incision in our model is represented by the stream power model and added text about we chose this model in the discussion section.

Reviewer 2: *P5L7 : given the emphasis in the introduction of the role of dynamic width, Im 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.*

Authors: As explained above, the model does not explicitly calculate channel width. Rather, a discussion of width scaling is presented in the paper simply as a consideration of what parameter values might be considered reasonable. We have added text to the section following the lateral erosion equations to clarify this point.

Reviewer 2: *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).*

Authors: Moved these references to Hartshorn et al. and Stark et al. to the discussion section and expanded discussion of the effects of Kl/Kv ratio.

Reviewer 2: *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 explicitely modelling bed and bank erosion via an hydrodynamic model (e.g., Lague, 2014; Croissant et al., in press).*

Authors: Updated text to discuss use of fixed kw and range of possible values of kw (P8L1-5).

Reviewer 2: *CRITICAL : Is there an internal "safety check" that verifies that the actual channel width in the primary node (kw $Q^{0.5}$) is systematically smaller than the pixel size ? otherwise you violate some of your assumptions.*

Authors: As explained above, there is no explicitly defined width in our model. Text was added to the manuscript in the section describing vertical and lateral erosion equations to make this point.

Reviewer 2: *Figure 1: the legend is quite hard to follow. Similarly there are several black arrows so its 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.*

Authors: Typo fixed and figure revised for clarity.

Reviewer 2: *Figure 1b: it is not clear why you choose to have the neighbouring node set to the downstream elevation node (Zd), not the primary node (Zn). It seems to me that this probably drives artificial mobility in the model without a real justification.*

Authors: Setting the elevation of the lateral node equal to the elevation of the primary node would make the valley slightly wider, but the channel immobile. That is because water flow in that case would continue to prefer going from upstream through primary to downstream, because the "detour" through lateral would have a lower slope (same altitude difference, more distance covered). With flow continuing to prefer the shorter route, that is where the erosive action would be, and the just-eroded lateral node would be left at the original altitude of the primary node.

Setting the elevation of the lateral node equal to the downstream node gives the opportunity for flow to be rerouted through the lateral node, but does not require it.

Reviewer 2: *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?*

Authors: A new supplementary materials document has been written and includes a figure detailing how lateral nodes are chosen in the model. To briefly answer the question, a lateral neighbor node can only be the E,S,W,or N neighbor of the primary node, but the lateral node can be to the diagonal direction of a flow line in the case of 45 degree bends or two straight segments that flow across diagonals.

Reviewer 2: *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).*

Authors: Yes, but as noted earlier, that would defeat the purpose of having a simple, low-dimensional model formulation.

Reviewer 2: *: 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.*

Authors: A figure detailing how radius of curvature is calculated is included in the supplementary materials document. Yes, you are correct in that the way radius of curvature for straight channels is calculated is a simple way to account for sub-pixel variability in channel position.

Reviewer 2: *: 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 back calculate the actual local flow depth?*

Authors: You are that it is possible to calculate flow depth in the model for each cell based on width, slope, and friction, but we choose to use a simpler hydraulic geometry relationship for flow depth as many other non-hydrodynamic landscape evolution models do (CHILD, etc). We emphasize that our goal in developing this model of lateral bedrock erosion is to start with the simplest reasonable erosion model so that we can focus on understanding the dynamics of lateral erosion. Additionally, as noted above, H scales more strongly with Q than with S.

Reviewer 2: *: 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).*

Authors: Text has been added to clarify the treatment of sediment in the model at the end of the numerical implementation section (P9L9-14).

Reviewer 2: *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.*

Authors: Revised this section to emphasize the end member models as representing valley widening as a function of wall height and moved links to natural processes to the discussion section. Discussion of bank erosion in CAESAR and EROS has been added to the background section.

Reviewer 2: *P8L22: Why cannot you use the model with lateral mobility from the beginning ? what kind of hillslope erosion law is used ?*

Authors: It is possible to use the model with lateral erosion from the beginning. Figures comparing model runs with lateral erosion from the beginning and lateral erosion started after topography was initialized are shown in the supplementary materials section and show no difference in model topography.

Reviewer 2: *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.*

Authors: A range of values for K and alpha were chosen to explore model behavior, specifically channel mobility and valley width. References were added supporting the range of K and alpha values chosen in the model runs. In order to demonstrate the range of possible lateral erosion and valley widths in our new model actually works, we used a high, but justifiable value for runoff on event time scales. Text on how runoff values were chosen was added to the paper. High values of runoff, which are meant to represent peak values, not mean annual values, were chosen to get Kl/Kv ratios of 1 and 1.5 in order to demonstrate the lateral erosion that emerges from the model. The parameter values were chosen from a range of reasonable values found in nature. In some cases, significant channel mobility and lateral erosion occurred (these cases are highlighted in the manuscript), but in some cases, little observable lateral erosion occurred, see Figures 5a,6a; 9a,c;

Reviewer 2: *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.*

Authors: The choice, really, is whether to present figures like this in dimensional or non-dimensional form. The latter is of course more elegant, and has the advantage of demonstrating the role of multiple variables and their interactions. However, feedback from colleagues and students indicates that many find dimensional plots more intuitive, so we have stuck with them. As regards danger, all we can say is that no students or colleagues were harmed in the writing of this paper, and we don't think anyone will be harmed by reading it.

Reviewer 2: *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.*

Authors: Thank you, this paper is now cited here.

Reviewer 2: *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.*

Authors: Not at all! The reviewer seems to assume that ANY set of rules or equations could reproduce any desired set of landforms as long as the right parameters are chosen. We disagree. When a model for a particular natural pattern is proposed, it may either succeed or fail at the basic test of qualitative reproduction of the pattern in question. We have shown that our model succeeds at the basic task of qualitative reproduction. This might be a weak test, but does not mean success is inevitable. Of course, it would be wonderful to have independent constraints on parameter values. Nonetheless, we adamantly disagree that demonstrating qualitative consistency between a model and observations, given certain parameter ranges, constitutes circular reasoning.

Reviewer 2: *P12L21: which hillslope processes, you did not describe them and in the discussion you seem to imply that there are no hillslope processes operating.*

Authors: Text changed to reflect that indeed there are no hillslope processes in this model.

Reviewer 2: *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.*

Authors: The word threshold is removed and text clarified.

Reviewer 2: *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)*

Authors: We have run the models in section 4.2.1 with with grid size of 15 m and 20 m and compared them to the original models runs with dx=10 m. The new models with larger grid size shows some differences with the original models, but are largely similar in the amount of lateral erosion accomplished and width of bedrock valley created. Figures and discussion of these model runs is included the supplementary materials section.

Reviewer 2: *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.*

Authors: See responses above regarding scaling relationships and treatment of channel width used in this model.

Reviewer 2: *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.*

Authors: Added text in this section to remind readers of the fixed width scaling in this model that prevents channels from changing width in response to sediment flux and added a paragraph in the discussion section detailing the implications for the model (P21L8-13).

Reviewer 2: *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.*

Authors: Model parameters have been more thoroughly explained in model experiments section. We acknowledge that in this initial version of the lateral erosion model, valley width is often strongly related to the imposed Kl/Kv ratio. But we note that the model produced narrow valleys in undercutting slump models with high values of K and low values of alpha for both values of Kl/Kv that were tested in this paper (Figure 3c,d).

Reviewer 2: *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.*

Authors: Text here reworded to clarify our intention to note the limitations of the current erosion/deposition model in future work that may address spatial and temporal changes in runoff and changes in and/or multiple grain sizes.