Earth Surf. Dynam. Discuss.,
https://doi.org/10.5194/esurf-2019-31-AC1, 2019

Earth Surface Dynamics Discussions Open Access EGU

Interactive comment on "Estimating the disequilibrium in denudation rates due to divide migration at the scale of river basins" *by* Timothée Sassolas-Serrayet et al.

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Received and published: 4 October 2019

RC1: I've completed my review of Sassolas-Serrayet et al's paper entitled 'Estimating the disequilibrium in denudation rates due to divide migration at the scale of river basins. 'In this manuscript, the authors analyze a series of landscape evolution models to try to ultimately assess the ability for one to measure the background rock uplift rate through basin averaged erosion rates in the presence of divide migration and find that there is a drainage area dependence on the ability to do so. Using this, they are able to provide some recommendations for planning of sampling for things like cosmogenic erosion rates. This study seems timely and interesting and is a good fit for Earth

Surface Dynamics. While I think the general results are strong and consistent with what others have shown (or rather what is implied by the results others have shown), I do have some concerns about whether they've biased their detailed results with the specifics of their model setups. Much hinges on whether or not the models were ran with an imposed threshold area or not (it is a little ambiguous in the text, so it's possible that my concerns are for naught and I simply misunderstood what they meant). These concerns (along with other comments that the authors hopefully find helpful) are outlined in my detailed comments below.

AC: Thank you for your constructive comments. The remarks concerning the potential bias associated with the use of a threshold area Ac greater than zero leads to a great number of modifications, especially in the result and discussion sections. However, the results we obtained by using a Ac value equal to zero does not affect the main finding of our study: divide mobility can lead to significant differences between tectonic uplift and basin-wide denudation rates even if topographic steady state is achieved at large scale. We updated the figures in order to represent the new results and take into account the referee's comments. We hope this offers an improved and clearer version of our manuscript.

RC1: L37-38: Probably important to clarify that the divides are migrating in response to the same change (it's implied, but not explicitly stated in the way you word it).

AC: Thank you for this remark. Indeed, the persistence of migrations is not related with change of other factors (i.e. tectonic or climate) during the landscape evolution. Action: We modify the concerned paragraph as following: "Although rivers exhibit a rapid adjustment to tectonic or climatic changes to maintain their profiles, Whipple et al. (2017) show that divides continue to migrate over time periods of 106-107 years as response to the same changes. This suggests that long-term transience might be pervasive in the planar structure of landscapes, even in the absence of new variations in landscape characteristics or forcings (e.g. tectonic or climate) (Hasbargen and Paola, 2000; Hasbargen and Paola, 2003; Pelletier, 2004)."

RC1: Equation 1: I think the top expression (U + (dz/dt)_fluv) should be for A > A sub c, correct?

AC: Thank you for pointing out this inconsistency. We use a value of Ac equal to 0 in the revised version of the manuscript in order not to separate hillslope and fluvial domains. That way diffusion and fluvial erosion affect simultaneously every pixel in the model. Action: we removed the condition that regards Ac in the equation 1.

RC1: L119 – 121: Is the stated critical drainage area applied in the model (i.e. the model is run with the rule set such that diffusion is only applied where A<A sub c and incision is only applied where A>A sub c) or is this simply the threshold area used for extracting the channels (and thus the channel heads) for analysis? My (anecdotal) experience has been that running TTLEM (or really any model that allows you to do so) with an explicit critical drainage area that is built in where it defines where incision/diffusion is applied can produce some odd behaviors and very odd drainage networks. If you were running TTLEM with the critical drainage area option turned on, did you experiment with the sensitivity of your results to turning this off (i.e. setting it to 0)? It's important to note that this option is kind of atypical, i.e. it is not something allowed in CHILD, LandLab, etc. I think more importantly, if you are running with Ac (or AreaThresh as it's named in the TTLEM setup) set to a value greater than zero you are artificially controlling the length of the hillslope (it would otherwise be set by the combination of K and D values you provide) and thus controlling the length scale over which the landscape responds to the divide migration (as this is happening mostly in the hillslopes based on prior results). This may in turn reflect some of your other results (e.g. erosion rates as a function of drainage area, etc).

AC: We agree. We tested the sensivity of our results with Ac equal to 0. Results confirm the referee's intuition, namely that a value of Ac greater than zero controls the length of hillslopes and thus produces odd behaviors. In the revised version of the manuscript, we use Ac = 0 and modified the text and figures accordingly.

RC1: L125-126: This is the theoretical time to steady-state following a perturbation and assuming a fixed drainage area, which is not really applicable to the time to steady-state for a model ramping up (i.e. running from an initial random noise, low elevation topography to a stabilized topography). I don't think this necessarily matters that much to your results as you are basically just exploiting the fact that this portion of a model run has a lot of drainage reorganization, but I would be careful about equating these.

AC: We agree. Action: We removed the sentence.

RC1: L205-207: This pattern in erosion rates as a function of drainage area pretty much follows from the observation discussed in Forte & Whipple 2018, namely that if considering simulated landscapes experiencing progressive divide motion (as opposed to discrete captures), the erosion rate contrasts across divides is very spatially limited to areas very near the divides (essentially hillslopes), thus as you move to larger drainage areas, the ability to 'see' this across divide contrast in erosion rate in basin averaged values would be expected to decrease as the signal is diluted by more and more of the drainage basin eroding very near the background erosion rate / uplift rate.

AC: We agree. Action: We added the sentence "As exposed by Forte & Whipple (2018), the erosion rate contrasts across divides is spatially limited to areas very near the divides."

RC1: L232-236: This all seems logical, however it might be important to note that basin averaged statistics like this work best when a basin is either uniformly (or at least consistently) either expanding or contracting. This is probably (usually) the case in homogeneous models like the ones you use here, but in either more heterogeneous models or when applied to real landscapes, there is the danger of a basin appearing to be neutral because it is expanding in one direction and contracting in another (i.e. the metrics counterbalance when averaged over the whole basin). This could be especially noticeable when divide migration is driven by a lateral gradient in uplift rate with respect to the main drainage direction. Thus, I would (maybe somewhat self importantly) argue that along-divide metrics are still quite useful to consider.

AC: We agree. Even in homogeneous models like the ones we use, basins may experience both expansion and contraction on different parts of their perimeter and a single metrics may not reflect compound behaviors. Action: We added an analysis of the standard deviation of cross-divide differences in metrics along basin perimeter (Figure 7) and associated details in the main text ("Figure 7c shows that the dispersion is related to the standard deviation of aggressivity metrics, $\Delta\chi$_std, ΔG_std and ΔH_std. In other words, basins where different divide segments migrate at different rates or in different directions are more scattered.").

RC1: L249: This is also consistent with what Forte & Whipple 2018 saw in natural landscapes, i.e. across divide contrasts in elevation were usually equivocal in terms of indicating potential for drainage divide motion compared to the other metrics.

AC: We agree. Our updated model shows the metric based on local slope seems less sensitive to basin-wide denudation from divide migration for area with rock uplift rates ($\leq$ 0.1 mm/yr). Conversely, even if it depends on the average elevation at regional equilibrium stage, the metric based on elevation seems more relevant. However, this metric would likely display significant noise in natural landscapes.

RC1: L254-255: Why do any of these basins have knickpoints? You're applying a constant uplift rate and progressive divide motion shouldn't really impart knickpoints onto any profiles. Are they coming from captures? While I understand the logic of ignoring basins with knickpoints, it's more that I wonder if the presence of knickpoints in this is suggesting that there may be some stability issues. One of the behaviors of the TVDFVM algorithm is to keep and accentuate any knickpoint (even if those are developed through numerical instability), so it would be good to try to diagnose why there are knickpoints in the first place to rule out model instability. You could try running one of your models with the same exact setup but using the implicit (fastscape) algorithm that's built into TTLEM and see if you also are getting knickpoints.

AC: We agree, for uniform models knickpoints appear counter-intuitive. However, we observed two processes that produced actual knickpoints (1) the uplift of the initial plateau leads to the propagation of major knickpoints controlled by the edge of the plateau until ~2Myr and (2) as you suggest, discrete stream captures occur occasionally. Overall, the great majority of basins (and therefore drainages) are not affected by any knickpoints. Action: We clarified this point by adding the sentence "In our simulations, knickpoints may develop due to (1) the dissection of the initial flat surface or (2) discrete drainage captures (see Sec. 3.1).".

RC1: L274-278: I'm a bit confused by this statement. From my own simulations with TTLEM, if you're starting with the same random noise and keeping everything else the same (i.e. not changing the length of timestep, etc), the drainage network evolution will be pretty much the same regardless of uplift rate. I would expect changes in the effective drainage density to manifest more in response to changes in the diffusion constant or ratios between K and D.

AC: We agree. This behavior was driven by Ac > 0 and our updated model shows a different pattern. We now observe no differences in general drainage geometry. However, we observe an inverse relationship between uplift rate and drainage density consistent with previous theoretical works. Tucker and Bras (1998) found a similar relationship when using a formulation of diffusive processes that includes a threshold slope Sc, as we use in our simulation.

RC1: L283-285: Again, this might be a result of setting the area threshold to a non zero value (assuming you did). I'm also not sure how to interpret Sc if the area threshold is set to a non zero value, because you're artificially controlling the hillslope length and thus (I think) artificially controlling the maximum slope that can develop anyways. If I misunderstood your discussion of A sub c earlier and you were not running with A sub c set to a non zero value, feel free to ignore this comment.

AC: We agree, this is indeed driven by Ac>0. Action: We modified this part of the discussion based on the updated results.

RC1: L340-345: This is neat, though (and as you mention in the following sentence) it would be interesting to think about the applicability of this in more heterogeneous environments where, as I mentioned before, there's a greater chance that the basin averaged metric could be misleading (i.e. a low average metric because of coexisting drainage area loss and gain on different sides). Maybe a valuable approach to consider would be including the standard deviation (or min and max) in the basin averaged metric to try to capture this potential variability without having to look at the divide segments in detail?

AC: We agree. As we explained in the previous comment concerning L232-236, we assess the variability of cross-divide contrast in metrics by calculating the standard deviation of metrics along basin perimeter. This allows to discriminate between basins that are homogeneously stable and those that display both expansion and contraction along their perimeter. Action: See RC1: L232-236.

RC1: L349-372: This is a good thing to focus on, but I think you could add an interesting discussion here of considering the sampling strategy with regards to the goal of the study. At present, this gives a (valuable) set of ideas for how large a basin needs to be to get an accurate assessment of the uplift rate from the erosion rate, but alternatively you could point out that this gives you a sense of the size of basin you need to target if you're explicitly interested in measuring divide migration rates, i.e. larger basins are not going to be helpful. Similarly, this speaks to the need to pre assess basins for their potential divide mobility before sampling (if your intention is to get at background uplift rate), i.e. if either the divide segment or basin average metrics suggest no divide mobility, you don't need to worry about the size of the basins as much (except for all the other concerns we already have, that you mention).

AC: We thank you for this pertinent suggestion. Action: We developed this aspect as a new full paragraph in the discussion section.

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