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Earth Surface Dynamics Discussions

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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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 Dy-

namics. 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.

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).

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

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).

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.

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.

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.

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.

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 TVD-FVM 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.

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.

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.

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?

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).

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