Review of 'Estimating the disequilibrium in denudation rates due to divide migration at the scale of river basins' by Sassolas-Serrayet et al.

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The paper in review presents a series of numerical models set up to investigate the impact of drainage divide migration on landscape denudation rates, as well as both presenting new topographic metrics and testing the ability of previous ones to detect divide migration. The authors then apply these metrics to provide some useful constraints for basin size for CRN sampling. I think the paper is interesting, well written, and builds well on previous work that quantifies the extent of drainage divide migration across landscapes, given the many papers that have been published in the last few years on the topic. However I have some concerns about the setup of the model, in particular the use of a parametrised critical area threshold. I therefore think it will be very suitable for publication in ESURF provided that the comments detailed below can be addressed.

Line 34 - 35: repetition of 'modern landscapes' twice in the sentence.

Eq 1: I think the fluvial expression should be $A > A_c$?

Eq 1: Similar to Reviewer 1, I agree that the model setup described in equation 1 makes it seem like the position of channel heads and resulting drainage divide metrics will be determined by the critical area threshold (A_c) parameter. I'm not convinced that, in real landscapes, there is a critical area threshold for determining channel head location or, if there is, that this should be fixed across the landscape as a whole. This setup seems a bit unintuitive to me - why not just combine fluvial incision with hillslope diffusion at each node and eliminate the need for an A_c parameter in the LEM at all?

Line 118: Much work has shown that in many real landscapes it is most likely that $n \neq 1$ [e.g. *Lague*, 2014; *Harel et al.*, 2016]. This will again have a significant impact on the distribution of slopes and erosion rates within the model landscapes, and may influence the calculation of the divide migration metrics. I think it would be useful to run some test landscapes where $n > 1$ to determine i) the impact this has on the variability of erosion rate with basin area; and ii) the impact on the aggressivity metrics.

Line 121: Following on from this, I would suggest running a sensitivity analysis changing A_c in the model setup to see what effect this has on the calculation of divide migration metrics.

Line 125: How is it determined whether the model runs have indeed reached steady state?

Line 135: It's a bit unclear how the elevation is calculated. Is this the average elevation for all pixels in the basin at each time step?

Line 162: It would be good to include some more details here of how this averaging is carried out. Is local slope calculated using a moving window (including hillslopes), or is this just the slope of the first order channel? It wasn't clear to me whether the elevation at each channel head was averaged, or whether the first order channel downstream of the channel head was included.

Fig S2: I think it would be useful to edit this figure and move it to the main paper, as it was difficult to follow how the aggressivity metric is calculated from the text. I realise some of this is based on previous work, but the averaging of the cross-divide metrics to produce a new metric for each basin is novel in this paper. I didn't understand how the segmenting shown in Fig S2 was done, or how the segment length over which the averaging should be performed is determined.

Line 221: As well as drainage divide migration, variability in erosion rate between basins could simply result from the transient propagation of knickpoints, especially in the earlier model runs. This seems to be supported by the fact that the variability decreases significantly through time as shown in Figures 4 (a) - (c).

What is the evidence that this variability is in fact due to divide migration and not due to transient knickpoint propagation? In the text it's stated that this is shown from Fig 2(e), but I didn't understand how this figure shows that.

Figure 6 and 7, and Lines 259 - 262: Is there any physical meaning/theoretical prediction for the linear trends on these figures, and how significant are they? If indeed there is a non-linear relationship between S and E, then I wouldn't expect a linear relationship between ΔG or ΔH and E/U.

Figure 7: I was quite surprised how noisy some of these data are, especially for the smaller basins, considering that these are all from LEMs and not real landscapes. Maybe this could do with a bit more discussion in the text as to potential reasons for this noise? In the text it's stated that it is due to the presence of significant knickpoints, but I was confused as to whether basins with knickpoints were excluded or not. It raises the possibility that some of these metrics, especially ΔH, would be too noisy in real landscapes when additional factors such as variations in lithology, rainfall or uplift are taken into account.

Lines 275 - 277: I'm also surprised that there is an increase in drainage density with increasing uplift when $n = 1$ in the model runs. Previous theoretical work by *Tucker and Bras* [1998] predicted that drainage density should be independent of erosion rate when $n = 1$, which was then shown by numerical modelling using CHILD by *Clubb et al.* [2016] (Figures 4 and 5). Furthermore, and more qualitatively, when I have run LEMs with detachment-limited stream power in the past I have generally found that the geometry of the network remains fixed when increasing uplift rate, and only the slopes of the network increase. I think this could be discussed in more detail as to why there is this discrepancy with previous theoretical predictions and numerical modelling results.

Line 279: 'we obtain no significant changes in the relationship between the calculated aggressivity metrics and the E/U ratio for uplift rates...' I'm not sure I agree with this statement - from Fig 7, some of the distributions look quite different for U = 0.5 and U = 2 mm/yr, especially for ΔH. This may be just because of the smaller basins, or that it's difficult to get a sense of how dense the data are in the centre of the plot.

Lines 276 and 287: I think it would strengthen the paper to quantify this change in 'river channelization' proposed by the authors. At the moment this appears to be a qualitative statement which is difficult to verify from the current figures and analysis. It's an interesting result that changing the value of S_c influences drainage density, which makes sense from the theory and has implications for real landscapes where S_c is difficult to determine. I think more could be made of this, and suggest simply calculating drainage density for the different model runs. This would put some more weight behind the statement that increasing drainage density is the mechanism by which changing U and S_c impact the aggressivity metrics.

Lines 314 - 315: I don't understand what are the 'expected quadrants' that the basins are being compared to here. Is this compared to the reference model, or compared to Willett et al. (2014)?

References

Clubb, F. J., S. M. Mudd, M. Attal, D. T. Milodowski, and S. W. Grieve, The relationship between drainage density, erosion rate, and hilltop curvature: Implications for sediment transport processes, *Journal of Geophysical Research: Earth Surface*, p. 2015JF003747, 2016.

Harel, M. A., S. M. Mudd, and M. Attal, Global analysis of the stream power law parameters based on worldwide 10be denudation rates, *Geomorphology*, *268*, 184–196, 2016.

Lague, D., The stream power river incision model: evidence, theory and beyond, *Earth Surface Processes and Landforms*, *39*, 38–61, 2014.

Tucker, G. E., and R. L. Bras, Hillslope processes, drainage density, and landscape morphology, *Water Resources Research*, *34*, 2751–2764, 1998.