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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.

Timothée Sassolas-Serrayet et al.

timothee.sassolas-serrayet@umontpellier.fr

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RC2: 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.

AC: Thank you for your positive and constructive comments. We carefully addressed every remark you formulated. Your concerns about threshold area Ac to distinguish hillslope and fluvial domains were justified. In the revised manuscript we produce a new set of results using simulations with no threshold area (Ac=0).This change largely corrects the atypical behaviors underline by both reviewers concerning the length of hillslope, the dependency of drainage network evolution to rock uplift, and the meaning of Sc, but it does not modify the major results concerning the impact of drainage migration on basin-wide denudation rates. We provide a revised version of the manuscript where we present our new results and addressing the referees comments.

RC2: Line 34 - 35: repetition of 'modern landscapes' twice in the sentence.

AC: We agree. Action: We reworded the sentence accordingly.

RC2: Eq 1: I think the fluvial expression should be A > Ac?

AC: We agree. Action: As modified our model with Ac = 0, we removed this condition in Equation 1. See RC1 for details.

RC2: 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 (Ac) 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 Ac parameter in the LEM at all?

AC: We agree. See RC1's comment. Action: See RC1.

RC2: Line 118: Much work has shown that in many real landscapes it is most likely that n > 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.

AC: We agree this would be a very interesting alley to explore. However, this would constitute an entirely new study that would go far beyond the scope of the present study. Action: We mentioned this as a perspective in the conclusions.

RC2: Line 121: Following on from this, I would suggest running a sensitivity analysis changing Ac in the model setup to see what effect this has on the calculation of divide migration metrics.

AC: We agree this could be a worthy path to follow. However, following comments from both referees we opted to set Ac = 0 in line with the majority of works published previously in order to produce results that can be compared.

RC2: Line 125: How is it determined whether the model runs have indeed reached steady state?

AC: As expected, the model does not actually reach a perfect steady state but a quasi-static equilibrium. First, we study the reorganization of the drainage after an initial perturbation keeping in mind that the drainage network shall not be affected by the initial model geometry. Next, in our simulations, we consider that the landscape reaches a regional steady state once transient topographies are completely eroded, i.e. once the mean model elevation remains constant and when the average denudation rate and the imposed rock uplift rates deviate by less than 2%. Finally, transient events such as discrete stream captures susceptible to form knickpoints are carefully detected and discarded from the results.

RC2: 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?

AC: Basin-wide denudation rate is calculated using the average of differences of elevation in the basin borders during a 10 kyr interval.

RC2: 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.

AC: We agree we did not provide enough detail on this. All metrics are measured at a reference drainage area. Across-divide metric differences are only calculated along divide segments shared by two reference basins. Hence, aggressivity metrics are the averaging of these documented segments along the perimeter of the sampled basin. Action: We reshaped section 2.3.2 and transferred Fig. S2 from the Supp. Mat. to the main text (now Fig. 2).

RC2: 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). 1 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.

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AC: It is true that transient propagation of knickpoints can affect basin-wide denudation rates. To avoid any erosion signal associated with these transient features, we do not take into account the first stage of the model (< 2Myr for the reference model), when as you highlighted it, major knickpoints are retreating along the edge of a plateau. We also discard from the analysis all basins that contain knickpoints generally formed by discrete stream captures.

RC2: 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 _G or _H and E/U.

AC: We agree that the relationship between aggressivity metrics and E/U may be more complex than a simple linear relationship (especially when considering the different trends between aggressor and victim basins). We do not propose any physical meaning to explain this relationship, but we simply use it to compare results in regard to the different parameters. Action: We agree this may be misleading and we decide to remove this aspect from our analysis.

RC2: 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 _H, would be too noisy in real landscapes when additional factors such as variations in lithology, rainfall or uplift are taken into account.

AC: We agree the level of noise observed in Figure 7 is surprisingly high. However, it cannot be explained by knickpoints retreats because we excluded all associated basins from our analysis. Action: Following RC1, we now assess the standard deviation of

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cross-divide metric differences along basin perimeter and find that it explains part of the dispersion. We also assess via a “confidence index” the proportion of documented segments when calculating aggressivity metrics. These results are now presented in section 3.

RC2: 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.

AC: Both Tucker and Bras (1998) and Clubb et al. (2016) found that drainage density is independent of erosion rates with n=1 (and therefore, uplift rate in steady-state topography) when using a linear formulation for diffusion. However, when using a formulation that takes into account a threshold slope for hillslope, both studies show an inverse relationship between drainage density and uplift rate. Thus, the results exposed in our study are consistent with these previous theoretical works.

RC2: 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 _H. 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 center of the plot.

AC: We agree the assertion may be misleading. We observe a clear decrease of dispersion for higher uplift rates. However, the trend remains the same regardless of the uplift rate value. In any event, results must be affected by a threshold area Ac fixed to a non-zero value. Action: We present detailed results concerning sensibility to uplift rate using a Ac = zero in section 4.1.1.

RC2: 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 Sc influences drainage density, which makes sense from the theory and has implications for real landscapes where Sc 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 Sc impact the aggressivity metrics.

AC: We agree. However, the updated model using Ac = 0 does not display significant discrepancies when Sc varies. As pointed out by RC1, Ac > 0 can artificially control hillslope length and drainage density. Letting U vary does have an effect on drainage density, though, and we agree this should be explored. However, we think that this topic may deserve a dedicated study that is beyond the scope of the present work.

RC2: 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)?

AC: We agree we did not state our terminology clearly. We meant ‘expected when comparing to the reference model’. Action: We clarified this by adding the following paragraph: “In agreement with cross-divide metrics tested by Forte and Whipple (2018), graphs in Figure 7 must be divided into four quadrants. Aggressor (victim) basins have negative (positive) $\Delta\chi$_av and ΔH_av values and conversely positive (negative) ΔG_av value (Fig. 2). Theoretically, aggressor (victim) basins have higher (lower) denudation rates than the underlying uplift rate.” We also explicitly labeled the aggressors and victims quadrants in the associated figures.

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