Review of Carretier et al.—Esurf

The authors present a landscape evolution model that tracks clasts as they are eroded and deposited. During this erosion and deposition, the model estimates the chemical weathering rate during initial exhumation and exposure in the regolith and once again during storage in colluvial deposits before the clast is transported out of the model domain. The authors find that as mountains uplift, colluvial weathering contributes a measurable weathering flux, even for regimes where uplift outstrips the production rate of new regolith. The study is an excellent first-step toward resolving the controversy surrounding the role of mountain uplift in increasing weathering. A critique of previous models is that they have only considered weathering at the regolith-scale, largely ignoring the importance of deposition in mountainous areas as a reservoir in which weathering can occur. This study takes a first stab at adding such a process into a model.

The manuscript is well-written and easy to follow and the figures are well-made and support the conclusions of the text. The manuscript is largely a modeling study, and I suppose for such an ambitious study, many assumptions could be critiqued. However, here, I limit my review to what seem to be the most important drawbacks of the current study. I hope that this study (and the model presented) spur more efforts to test different scenarios and assumptions. Ultimately, I think the study is appropriate for Earth Surface Dynamics with moderate-to-minor revisions.

1. The introduction seems to somewhat confuse silicate weathering and total weathering (particularly lines 17-44). For example, the "uplift" hypothesis (Raymo and Ruddiman, 1992) concerned the increase in silicate weathering as the mechanism behind declining CO_2. However, the authors cite Larsen et al. (2014) and Emberson et al. (2016a, 2016b) as evidence that weathering may continue to increase above a hypothesized "weathering limit" and that landslides constitute a significant weathering reservoir. However, these three studies did not find evidence for increasing silicate weathering; instead, these studies (particularly the Emberson studies) found evidence that the more labile phases (such as carbonate and sulfides) do weather as fast as they can be supplied by uplift, but how silicate weathering is affected remains inconclusive. A similar critique can be made of Figure 4 (and any of the figures that present W vs. D data). In this figure, data from Larsen et al. (2014) and the total W data from West et al. (2005) include weathering of more labile phases (ie, more than just silicates). Yet, as best as I can tell, the model only considers weathering of common silicate minerals. Thus, why should this data be comparable? Indeed, it would seem that the model overpredicts silicate weathering, since some of the scenarios seem to best match the total D data instead of the silicate only data. It would be helpful for all readers if these nuances were explained, considering that silicates, sulfides, and carbonates have very different climatic forcings. If anything, the total D data (and probably the Larsen et al. (2014) soil data) should probably be removed, since it includes very different minerals than considered in this study.
2. It's unclear why 7000 meters is chosen for the steady-state height of the range. It would seem that substantially different erosional processes (glaciation, peri-glacial processes, frost-cracking, etc.) that aren't currently represented in the model might operate over large areas of the model domain. I understand that this paper is mostly a presentation of the model and some sensitivity tests, but at least an acknowledgment or discussion of how these processes might impact the results would be useful.
3. Is chemical weathering the only method by which clasts can be broken down? It's a bit unclear from the manuscript, but I would suspect that there is substantial comminution

and disaggregation of clasts during transport from the regolith to the colluvium and also as the clasts are transported within the colluvium.

4. Equation 9 assumes that precipitation scales with the residence time of the water in the weathering zone (Maher, 2010). This is a decent first-pass assumption, but why should the scaling be the same in both regolith (with perhaps dominantly vertical flow) and in the colluvium, which should experience far more lateral flow. A sentence addressing this assumption would help make this clear (and perhaps how this assumption might affect the results.
5. In general, the model must make a number of assumptions, and the authors are fairly upfront about what these assumptions are (for example, no consideration of changes in soil pH or pCO_2 as rainfall changes, formation of secondary minerals, etc.). However, it would be helpful if the authors more directly addressed each of these assumptions (perhaps building on lines 477-481) and outlined in which direction consideration of these assumptions would affect the results. For example, does the exclusion of peri-glacial and frost-cracking erosional processes result in an over- or under-estimate of the weathering flux?
6. Finally, a definition of colluvium in the intro would be helpful. I'm not a geomorphologist, but it seems that much of what the authors are modeling is actually alluvium, given that it seems confined to deep-valley bottoms. Regardless, I think many chemical weathering folks are not used to thinking of colluvium, so a definition would be helpful.
7. I quite liked the conclusion regarding the trade-off between a cooling climate (and therefore lower P) and longer residence time of clasts in colluvium. Very interesting!

Minor Comments:
Lines 31-32: Dixon and von Blanckenburg (2012) only argue for a maximum erosion rate in soils, though not in watersheds as a whole.

Equation 9: While this is probably a reasonable first approximation, the authors should note that this excludes possible kinetic limitations arising from low fluid residence times. While such kinetic limitations are not often observed, they may become important in some of the orographic forcing scenarios, which seem to concentrate rainfall at specific altitudes, thereby decreasing fluid residence time substantially. Also, is P equivalent to the infiltration or does the model impose an approximate partitioning of P between evapotranspiration and infiltration?

Lines 392-393: Maher and Chamberlain (2014) are just citing other work here. Would be best to cite the original work (Manabe et al., 2004) unless noting that this number is consistent with previous weathering studies.

Figure 2: It is difficult to differentiate the "green" and "red" clasts in panel c.

Figure 3a: Coloring the axes in 3a would help to match the lines to the appropriate axis (same critique applies to similar panels in many of the figures).

References cited in review:
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