Referee 3 (J. Caves)

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.
Thanks for these constructive comments.

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 CO2. 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.
Yes, the data reported in our paper do not represent all the same thing. This is specified in corresponding figures for the West's data, and to be clearer, we added in the caption of Figure 4: "*Data from Dixon et al. (2009) and Larsen et al. (2014) correspond to local soil production rates in rapidly eroding settings. They thus represent some maximum weathering rates, to which it is useful to compare our model results. Note that Cidre results correspond to silicate weathering rates, not total weathering rate. We show both sets of data to emphasize that the modelled silicate weathering rate is probably overestimated but remains in the range of measured total weathering rates*". In the discussion, we specified (lines 516 517 518 of the previous manuscript) that the Emberson's data correspond to total weathering (Emberson et al., 2016a) and mostly associated with the dissolution of pyrite and carbonates for the cases studied in Emberson et al. (2016b). That said, we did not removed the Larsen, Moquet or Dixon data from the corresponding figures because we think they are useful for the following reasons:
(a) Data from Dixon or Larsen correspond to local soil production rates in rapidly evolving settings. They thus represent some maximum values for catchments mean weathering rate. As our model probably overestimates the silicate weathering outflux from mountains, it is useful to see that our catchment-scale model predicts consistently lower or equal silicate weathering rates than the total weathering rates of these pedon scale examples.
(b) Illustrating that our model probably overestimates the silicate weathering is just the reason why we wanted to show the total AND silicate weathering of the West's data. Indeed, the presented modellings seem to fit better the total weathering rate rather than the silicate weathering rate only, although using larger grain size for example, would

probably have decreased the weathering fluxes closer to the silicate weathering data. We hope that the added text in caption of Figure 4 clarifies our point.
(c) Although we specified a mineralogical assemblage, the results presented in our paper are actually broader and may apply to other rocks. Indeed, the weathering rate evolution depends strongly on the non-dimensional number N_{clasts}. This number is proportional to $\lambda\, k_m / r_r$, the ratio between the mineral rugosity times the mineral specific-dissolution parameter over the mineral radius (Supplementary Material Equation 11). This trade-off between the kinetic dissolution parameter and the reactive mineral size means that our results apply to other mineralogical assemblages sharing the same N_{clasts}. In other words, our results are not strictly restricted to granitoid rocks.

`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.`
Yes... The rational for such high relief is that we wanted to evaluate the effect of large elevation-temperature gradients on the evolution of the regolith. This gradient matters during the transient relief growth and controls part of the rate of regolith stripping and weathering outflux evolution. But it is not crucial once the cold period is installed and the topography has reached a stage close to dynamic equilibrium. The main outcomes of our paper would not change with a lower relief for experiments without regolith on the hillslopes. On the contrary, glacier erosion may changes things or not. In the discussion, we added the following text: "*Glacier erosion and associated physical weathering is not modelled. Glaciers would provide fresh sediment eroded from high elevations to the fluvial system. This is already the case in our simulations with cold climate but glaciers may generate more and finer sediment. In addition, frost-cracking at high elevations produces sediment. Both phenomena should increase the weathering contribution of sediment stored in valleys.* "

`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.`
OK. In the first paragraph of the discussion, we added: "*We also neglected the fragmentation of clasts during hillslope and river transport by physical weathering and crushing. This fragmentation should increase the weathering contribution of sediment trapped in the valleys as smaller grains weather faster.*"

`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.`
You are right, I would say that applying the same law is penalizing for colluvial deposits where the porosity is probably higher than in regolith. But I think this is even worse because we are using the runoff as a proxi for subsurface drainage. As we already warned in section 2.2 (Clasts weathering), "*(...) the linear dependency between the regolith production rate and runoff and the weaker dependence on temperature are consistent with that view, although our model clearly misses the control of water flux partitioning between the surface and ground on the regolith development rate and pattern (...)*".

`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 pCO2 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?`
Thanks. To make this clearer, we rewrote the first paragraph of the discussion as: "*Cidre does not model the precipitation of secondary minerals, or variations in the pH, pCO2 and changes in the chemical equilibrium related to the*

water-rock interaction (Oelkers et al., 1994; Brantley et al., 2008; Maher et al., 2009; Lebedeva et al., 2010). Neglecting chemical equilibrium and the precipitation of secondary phases overestimates the predicted dissolved fluxes. Predicting the effect of pH variations is difficult because it could increase the weathering rate (pH decrease by sulfure dissolution for example) or decrease it (pH increase by carbonate dissolution for example). Accounting for pCO2 would require to model soil-vegetation interactions, which remains a challenge at mountain scale. Th effect of neglecting pCO2 is not easy to predict. The groundwater circulation is also neglected, although it can contribute significantly to the weathering outflux (Calmels et al., 2011; Maher, 2011; Schopka and Derry, 2012). Allowing water to infiltrate would probably increase the predicted weathering outflux. We also acknowledge that the elevation dependent rainfall and temperature models used here are simplified. The feedback between relief growth and the evolution of precipitation and temperature patterns may be much more complicated. Even using a simplified orographic model, Cidre produces different and complex responses in terms of regolith, relief and weathering evolutions (Figure 8). More complex elevation-rainfall-temperature relationships may have significant implications for the evolution towards steady-state topography. Glacier erosion or associated physical weathering is not modelled. Glaciers would provide fresh sediment eroded from high elevations to the fluvial system. This is already the case in our simulations with cold climate but glaciers may generate more and finer sediment. In addition, frost-cracking at high elevations produces sediment. Both phenomena should increase the weathering contribution of sediment stored in valleys. We also neglected the fragmentation of clasts during hillslope and river transport by physical weathering and crushing. This fragmentation should increase the weathering contribution of sediment trapped in the valleys as smaller grains weather faster."

`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."`

At the end of the introduction we added: "*(...) and the associated colluvial deposits (unconsolidated sediment that have been deposited at the base of hillslopes or colluvium) (...)*".

`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!"`

Thanks ! Yes this could apply to foreland basins and be a simple explanation for observed long-term stability of weathering outfluxes. We plan to study this with our model but we need to implement a groundflow model to define the layer thickness that weathers in the foreland.

`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."`

OK corrected.

`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 substantial Also, is P equivalent to the infiltration or does the model impose an approximate partitioning of P between evapotranspiration and infiltration?"`

We agree. We need a groundwater model to evaluate this. The model does not account for dynamic ground water circulation yet.

`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."`

OK, we added Manabe et al. (2004).

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

We increased the size of this panel.

`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).`

Difficult as many of these figures have curves of different colors for different weathering flux curves.

Thanks again for these constructive and helpful comments.