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To the Associated Editor of ESurf

Dear R. G. Hilton

We are pleased to submit a revised version of the manuscript "Colluvial deposits as a possible weathering reservoir in uplifting mountains", by Sébastien Carretier and co-authors.

We want to thank you and the three reviewers for very useful and constructive comments. They identified the points that "hurt" and we hope that the changes we made help to clarify them.

The added, moved or modified text is in red in the revised manuscript. We added 3 figures in the Supplementary Material (S3, S4, S5). In the following we respond to the comments point-by-point, beginning by your comments.

Best regards,

Sébastien Carretier on behalf of the co-authors

Associated Editor (R. G. Hilton)

I have now had the opportunity to read your manuscript in detail, before examining the comments made by the three referees. My apologies for the delay in posting these comments online. The referees and I are in broad agreement ? this is a worthwhile contribution, providing novel insight into the role of colluvium (and alluvium) in setting weathering fluxes at the river catchment scale in mountains. The modelling framework has caveats, but in general these are well explained, and the numerical experiments provide impetus for future field, laboratory and modelling based studies into the links between tectonics, climate and the carbon cycle. The work is a very good fit for Earth Surface Dynamics.
The three reviews contain very thoughtful and detailed comments. These need to all be considered thoroughly in your revision. In some cases, moderate to major modifica- tions may be necessary, some are quick fixes. Please provide a detailed point-by-point reply to the referees? comments. While all the referee comments are valid and need to be considered, the ones which come to the front, based on my own reading of the paper and the reviews are:
- revising the abstract to better explain the numerical experiments which have been run, and thus provide more context to the wider implications. As it is, it tends to simplify and generalise a bit too much some of the discussion elements, and caveats.

Thanks. We rewrote the abstract as (new text in italic):
The role of mountain uplift in the evolution of the global climate over geological times is controversial. At the heart of this debate is the capacity of rapid denudation to drive silicate weathering, which consumes CO_2. Here we present the results of a 3D model that couples erosion and weathering during mountain uplift, in which, for the first time, the weathered material is traced during its stochastic transport from the hillslopes to the mountain outlet. *At this stage, the model does not simulate the deep water circulation, the precipitation of secondary minerals, variations in the pH, below ground pCO2 and the chemical affinity of the water in contact with minerals. Consequently, the predicted silicate weathering fluxes represent probably a maximum. We explore the response of weathering fluxes to progressively cooler and drier climatic conditions, we simulate weathering fluxes accounting for the decrease in temperature with or without modifications in the rainfall pattern based on a simple orographic model. The predicted silicate weathering rates are within the range of silicate and total weathering rates estimated from field data. In all cases,* during mountain uplift, the erosion rate increases and the climate cools, which thins the regolith and produces a hump in the weathering rate evolution. *This model thus predicts that the weathering outflux reaches a peak and then falls, consistently with predictions of previous 1D models.* Nevertheless, lateral river erosion drives mass wasting and the temporary storage of colluvial deposits on the valley borders. This new reservoir is comprised of fresh material which has a residence time ranging from several years up to several thousand years. During this period, the weathering of colluvium sustains the mountain weathering flux at a significant level. The relative weathering contribution of colluvium depends on the area covered by regolith on the hillslopes. For mountains sparsely covered by regolith during cold periods, colluvium produce most of the simulated weathering flux for a large range of erosion parameters and precipitation rate patterns. In addition to other reservoirs such as deep fractured bedrock, colluvial deposits may help to maintain a substantial and constant weathering flux in rapidly uplifting mountains during cooling periods.

- In the main text, making it more clear what experiments were run, and why these were run (i.e. justifying them).

We extended a lot section 2.7 "Description of experiments" in order to describe and justify the experiments.

- explaining better the role of physical breakdown of particles (during weathering, but also during transport) and its absence from the model (?)

In the Discussion section, 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.*"

- commenting on glacial/periglacial processes (given that even without cooling the lapse rate of 6 degrees/km would mean sub-zero temperatures at >4.1km elevation). This is in terms of some classic papers on this from a weathering perspective, and in terms of particle production (e.g. frost shattering etc,.).

This should be the topic of an entire paper ... Nevertheless, in the Discussion section, we added:
"*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.*"

`A final additional minor comment which I have, which I did not see made by the referees, regarded the relief of the simulations. 7 km seemed quite high. Comparisons to the Andes and Himalaya are broadly fair, but these regions have longer-wavelength topography which contribute to these peak elevations. Mountain ranges with faults at sea level, such as Taiwan and the Southern Alps, tend to have much lower peak elevations (>4 km). This comment also relates to the glacial/periglacial processes issue.`
You are right. As we explain extensively bellow in response to similar comments from referees, we designed experiments with high elevations because we thought that temperature gradient should control the regolith pattern evolution, which is partly the case but not crucially. The drawback of such a choice is that simulations should include a glacier erosion and frost-cracking model, but they do not. Nevertheless, we explain in the Discussion that both phenomena should rather increase the weathering flux from sediment temporarily stored in valleys at lower elevations. This deserves to be studied in the future, and we are very careful about non-intuitive behaviour, but adding these phenomena may actually reenforce our conclusions.

`Thank you for submitting to ESurf, and I look forward to seeing the revised manuscript.`
Thanks !

Referee 1 (S. Mudd)

`This paper describes experiments from the CIDRE landscape evolution model. The model uses an erosion and deposition module that allows particles not only to be ex- humed but also deposited. Particles can be traced from upland sources, along rivers, and the model allows them to be stored in terraces or colluvial deposits. The approach is novel and the model is very much a step in the right direction toward understanding how sediment pathways may influence chemical wreathing fluxes in actively eroding mountain ranges. I have annotated a pdf with most of my comments. There are a few comments of a general nature that I will make here. Firstly, combining particles with a landscape evolution model is not simple and it necessitates choices about how particle evolutions proceeds. There are some simplification that could be relaxed at a later stage or that could be significant to weathering rates in real landscapes. One that strikes me as possibly important is the physical weathering processes that may fragment grains as they move from hillslopes to rivers and on to sedimentary deposits. The authors might make a comment about this component.`

Thanks for these comments. Physical weathering is a good point we have been thinking. Fragmentation could be possible to incorporate but would require another choices. We decided to not include this process for the moment. The small clasts (<2 mm) used in the presented experiments may not break significantly during hillslope and river transport. Nevertheless, if this process was significant in the real word, it would increase the weathering rate in colluvium and along the valleys because smaller grains would weather faster, thus increasing the contribution of colluvium to the mountain weathering rate. We added a sentence in the discussion: *In addition, we 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.*

`The governing equations use combined erosion and deposition rules. The equations describing these rules are somewhat different to the typical equations that focus on the divergence of sediment transport, especially for hillslopes. However a previous ESURF paper by Carretier et al have shown how the model is able to reproduce analytical solutions of hillslope sediment transport so I think that should be mentioned in this current paper.`

OK, in the model description, we added: *Note that the erosion-deposition hillslope model leads to similar solutions as the critical slope-dependent hillslope model studied for example by Roering et al. (1999) (Carretier et al., 2016).*

`(...) The fluvial transport law seems to use a slope exponent of n = 1. This is not the first paper to do so but there is little field evidence to suggest n = 1 and quite a lot of evidence to suggest it is frequently 2 or greater. The authors should at least mention this, although I suspect that, since the model is run to a steady condition, the exponent mainly controls the timing of weathering fluxes but not the overall pattern.`

Indeed, a larger-than-one n value may indicate that detachment thresholds for sediment and bedrock play a significant role (e.g. Snyder et al., 2003; Lague, 2014). We added this sentence: *Note that the duration of the weathering peak may depend on the choice of $n = 1$ in Equation 2. The n exponent is known to control the response time of the topography to uplift (Tucker and Whipple, 2002) and the time required to develop the drainage network on the initial uplifted surface (Carretier et al., 2009). In regions where detachment threshold is significant, $n > 1$ (Lague, 2014). Using a $n > 1$ would thus increase the period during which a thick regolith covers the initial uplifted surface. It would thus probably affect the duration of the weathering peak, but not the contribution of colluvial deposit once this regolith has been eroded.*

`(Another component that concerned me was that most of the simulations are conducted with a very large grid spacing. A smaller spacing is used to show results do not depend on grid spacing (500m vs 20m) but I do think some more detail beyond simply the time series of weathering fluxes should be used to reassure readers that the grid scale is not changing the results.`

Yes this kind of models can be pixel size-dependent (Passalacqua et al., 2006). Note however that the mean transport rate (and thus residence time) of clasts does not depend on pixel size (Carretier et al., 2016 in Esurf). Here we reason by comparing fluxes and relative contributions of different landscape elements between experiments. We show that using a small scale landscape leads to the same conclusion for a small peace of a mountain (OROGRAPHYdx=20m). Would it be the same if we had used a smaller pixel size but keeping the same domain size ? In this case, narrower valleys would probably limit the residence time of colluvial deposits, but at the same time we would get a much denser drainage network, thus increasing the volume of colluvium. We expect that both effects would compensate themselves. In order illustrate this point, we ran a complementary experiment based on the OROGRAPHIC simulation but using dx = 200 m instead of 500 m and keeping the same domain size (700x500 cells instead of 300x200). We divided the lateral erosion parameter by 2.5 in order to decrease the local amount of colluvium. We still observe that the weathering of colluvial deposits produces a bit smaller but significant weathering outflux during the cold period where regolith is absent. We added Figure S3 as supplementary material to illustrate these experiments.

`The choice of simulations are slightly puzzling to me: after the WARM scenario, the simulations result in either entirely or mostly bedrock hillslopes with sediment concentrated in valleys. These modelled landscapes do not feel that representative of most mountains, where regolith is present.`

We agree that most mountains have a regolith, but the experiments COOLING and OROGRAPHIC just following the WARM scenario are end-members, as stated at the beginning of section 4.2. That is why we then run the experiments COOLING REG. and OROGRAPHIC REG. which should be more realistic as they produce a regolith at least at low elevations.

`It also seems strange to set the model parameters such that 7000m mountains are formed within a model that does not contain glacial processes. I would have used lower mountains.`

We understand this concern. We designed experiments for high mountains in order to analyse the effect of wide range of temperatures on chemical weathering. Initially we wanted to evaluate the control of temperature on the distribution of regolith production rates with elevation for high mountains. We also wanted to evaluate the impact of the temperature decrease associated with increasing relief through time. Finally, although temperature decrease with increasing elevation contributes to the stripping of the regolith on the hillslopes, this effect is not crucial for our main conclusion. Indeed, in our experiments with dx =20 m, mountains reach a maximum of ∼1200 m (for which no glacier erosion is expected). Still, the effect of colluvial deposits is observed. Adding glacier erosion for high mountains would probably help to remove the regolith at high elevations and would deliver more fresh sediment to the valleys. Concerning the ratio between weathering flux produced by in situ regolith on the hillslopes versus that produced by colluvial deposits, we expect that glacier erosion would not contradict our conclusion. In our modelings producing a regolith under a cooling climate (COOLING REG and OROGRAPHIC REG), the regolith is located at low elevations, not at high elevations where glaciers dominate. Thus glaciers would not increase the contribution of hillslopes weathering. Furthermore, the weathering of sediment eroded by glaciers could increase the weathering contribution of sediment spending time in the valleys, consistently with our results. Nevertheless, we keep very cautious about the effects of paraglacial processes which remains to be evaluated.

The paper is not really trying to recreate a real landscape, but rather explore the consequences of some simple weathering rules combined with different erosion scenarios. The model runs give insight into just how important the sedimentary reservoirs in valleys or terraces are in locations where material is escaping the hillslopes incompletely weathered. I wonder if this effect is only noticeable because there is no regolith on the hillslopes. Perhaps the authors can comment on the importance of river deposits on the weathering fluxes as a function of N_{depo} versus N_{reg}. Presumably if N_{depo}/N_{reg} is small, the colluvial and fluvial deposits become far less important in the overall weathering signal.

Simon Mudd is right, the contribution of colluvium is maximum when no regolith cover the hillslopes, which must be considered as an end-member situation, or to the elevation range in a mountain without significant regolith. In order to weight the effect of colluvium weathering, we designed more realistic experiments COOLING REG. and OROGRAPHIC REG including a regolith at low elevations during the cold period. These experiments show that colluvial deposits still play a significant role, even if their contribution is smaller than that of the regolith covering the hillslopes (Figures 9 and 10).

Concerning the effect of N_{depo}/N_{reg}, we carried out another experiment based on COOLING in which N_{depo} is divided by 3 by multiplying the river transport length coefficient (ξ) by 3. We added Figure S4 as supplementary material to illustrate the following. Increasing the transport length, there is less deposition. Consequently, the sediment thickness in valley borders is smaller and the weathering outflux associated with colluvium is lower. The second effect of increasing the transport length is to increase the lateral erosion. Indeed, lateral erosion is proportional to the sediment flux. The sediment flux is greater if there is less deposition, thus the lateral erosion increases. Consequently, rivers are more straight and the residence of sediment in the river is shorter. This second effect contributes also to lower the weathering outflux associated with the weathering of colluvium. Note that the relief in the experiment with ξ x 3 appears less realistic than in the COOLING experiment (Figure S3). Yet, the colluvial deposits produce a significant weathering outlfux during the cold period. This comparison illustrates that the colluvium contribution can depend on N_{depo}, but that colluvial deposits still controls the weathering outflux in these experiments.

`Overall I think this paper contains a number of interesting innovations and will be useful to those trying to understand how weathering evolves as mountains grow. I am suggesting moderate revisions.`
We thank a lot Simon Mudd for his constructive comments that helped us clarify our paper. We took all the comments of the annotated manuscript into account.

Referee 2 (Anonymous)

Charretier et al. present a new landscape scale model to test the hypothesis that clasts in regolith, colluvium and rivers contribute substantially to weathering rates from a land- scape, particularly during cold periods. The modeling equations and setup are based on previous work with the Cidre model and this paper adds the clast weathering com- ponent. The model does have shortcoming that are acknowledged in the text, such as not including precipitation of secondary (clay/oxide) phases, lack of a dependence on pCO2 and pH, and lack of groundwater weathering components, but provides a constructive and useful set of thought experiments for how clast weathering, the distribution of clast residence times and the stochasticity of transport influence weathering (and denudation) rates from an uplifting landscape. Potentially I think that this is a bit oversold or is the maximum effect because of the design of the experiments.
The paper is well written and figures are very dense/informative. Referencing is thorough, although I do suggest more thorough referencing of some of the original work on this problem be included. The transitions in the methods could be smoother and reference to Table 1 (table of model simulations) more often would help the reader keep track of what scenario is being discussed.The transitions in the methods could be smoother and reference to Table 1 (table of model simulations) more often would help the reader keep track of what scenario is being discussed.
Thanks for these comments. We added calls to this table in the results section.

My comments are substantive but I believe they are mostly easy to address and will result in a refined manuscript. My comments related to the orographic precipitation and precipitation scaling are likely outside the scope of what the authors can do in revision but I hope can serve as a path forward in the development of this type of landscape modeling.
Thanks for having taken time to discuss these points. We come back to them later in our response.

General comments. Section 2.6 Model parameters that matter. Inclusion of a section like this has utility but ultimately was not well explained. The three model parameters that matter are the three that matter for question/hypothesis of this paper. This paragraph, as currently written, does not actually justify which parameters matter in reference to the above equations or show this (or cite previous work that might show this). Therefore I suggest greatly expanding this section (could be supplemental) in order to better explain to the reader why those three model parameters are of primary interest for the purposes of this study.
We understand the reviewer's point. We thought it was useful to inform the reader as soon as possible that not all parameters were important, so that he could better tackle the following description of results. But it is true that the reader does not know the hundreds of experiments we have carried out to be sure about what we are claiming ... In order to illustrate our point, we ran other experiments based on experiment COOLING with lateral erosion, but with different values and N_{hill} (x100) (Supplementary Figure S3) and N_{depo} (/3) (Supplementary Figure S4). In both cases, we observe that the weathering outflux follows the same humped evolution passing by a maximum weathering outflux and then that colluvial deposits produce a significant weathering outflux. Larger N_{riv} would decrease the final mountain elevation. Consequently, the difference in temperature between the top and foot of the mountain will be smaller. Hotter temperature at mountain top would thus increase the weathering outflux compared to a mountain corresponding to a smaller N_{riv}.
We rewrote the "Model parameters that matter" section as: "*The number of parameters that matter in this contribution can be reduced to three, namely the valley widening parameter α, the Damköholer number N_{clast} and the uplift-to-weathering number N_{reg}. The other four non-dimensional numbers S_c, N_{riv}, N_{depo} and N_{hill} affect the final relief, drainage density and hillslope roundness, and the response time for denudation to reach the uplift rate value. Nevertheless, whatever the value of these four parameters in the following experiments where the climate cools, the weathering outflux follows the same evolution characterized by a period of increase followed by a period of decrease. This evolution is primarily controlled by the evolution of the regolith layer, itself mainly controlled by the decrease in temperature and precipitation rate and by the increase in erosion rate, but not by S_c, N_{riv}, N_{depo} and N_{hill} (Supplementary Figures S3, S4, S5). Thus, the main results of this contribution do not depend crucially on these four parameters. Conversely, α, N_{clast} and N_{reg} determine the regolith thickness evolution and the time spent by the clasts in the different weathering reservoirs (regolith, colluvium, valley). Thus, we primarily vary the parameters included in these numbers to study the different behaviours of the model with regards to the long-term trend of the weathering*

outflux."

Organization of experiment descriptions in text. The authors currently include a section detailing the "Reference experiment WARM", which follows the method. Subsequently, additional experiments are run and brought up as they are discussed. This organizational flow is difficult for the reader, I suggest following the methods expanding Section 3 to include a description of all the experiments that are run and presented in the results and discussion. A nice table is included in the main text (Table 1) and reference to that table should be made in this section.

Thanks. We renamed this section "Description of experiments" and we add a description of the other experiments and their rational. To avoid repetitions, we moved to this "Description of experiments" section some text describing the experiments initially set in the "Results" section.

Conclusion. The introduction is set up to focus on the climate-tectonic link to silicate weathering and the carbon cycle. Currently, the conclusion (and discussion) do not circle back very thoroughly to provide implications or ways forward regarding the larger hypotheses invoked in the introduction. Further, I think this rigor modeling exercise afford the authors the space to suggest how modern (and maybe paleo) observations/measurements from rapidly eroding regions will help to 1) better parameterize landscape models like Cidre and 2) test the hypotheses invoked towards the end of the first paragraph of the introduction.

Thanks for that suggestion. In the conclusion, we added "*The model predicts that the contribution of colluvial deposits should vary according to valley width, latitude (temperature) and elevation. The model also predicts the mineral and elementary depletion of clasts. In order to test these outcomes, we need systematic measurements of weathering outflux (e.g. Emberson et al., 2016) and weathering grade of hillslope regolith, colluvium and river terraces within different catchments. In addition, proxis of paleo-denudation and paleo-weathering rates foreland basin deposits are still needed to validate or not the humped evolution of the weathering outflux during the growth of a mountain range.*"

Specific Comments and Questions

Framing and introduction

Missing from the introduction as it relates to the importance of constraining silicate weathering and the long-term carbon cycle are two aspects that deserve mention: 1. First is the role of plants and vegetation, and I would suggest also the feedbacks related to plants and vegetation with respect to mountains. A sentence or two acknowledging this and citing appropriate work (Berner, 1992, GCA; Drever, 1994, GCA; Banwart et al., 2009, GBC; Andrews et al., 2016, GCA; as well as papers from Donnadieu/Le Hir) would suffice. 2. Second, the role of the lithology in particular andesitic and basaltic lithologies (Dessert et al., 2003, Chemical Geology; Bluth and Kump, 1994, GCA; Ibarra et al., 2016, GCA) and possible related mechanisms and calculations proposed by Kent and Muttoni (2008, PNAS; 2013, CoP), Li and Elderfield (2013) and others. Further, short-circuiting of the long-term carbon cycle by various processes as pointed out by recent papers by Torres et al. (2015, GCA; 2016, EPSL) is a possibility where depending on lithology of uplifting mountains mountain ranges can be a CO2 source. Similarly, short-circuiting is now being proposed by based on new data from Iceland and elsewhere (Rive et al., 2013, ESPL; Andrews et al., 2017, GCA; Jacobson et al., 2015, EPSL). 3. Some of the original work on weathering-erosion relationships are not cited, those include: Waldbauer and Chamberlain, 2005; Hilley and Porder, 2008 4. Lines 483-492: At the beginning of the discussion the key distinction between this work and that of Ferrier and Kirchner (2008) and subsequent more recent modeling is stated. This was not clear until this point of the paper.

We rewrote the beginning of the introduction as : "*Since the contribution of Walker et al. (1981), the chemical weathering of continental silicate rock is known to be at the heart of the geological regulation of the carbon cycle and climate, through the existence of a negative feedback between climate and silicate weathering (Berner et al., 1983; Franois and Walker, 1992). The associated consumption of atmospheric carbon is indeed pending on the air temperature and continental runoff (Brady, 1991). Since those pioneering works, numerous studies have investigated the role of other parameters than climate on the silicate weathering efficiency. Those parameters include the key role of the vegetation cover (Berner, 1994; Drever, 1994; Lehir et al., 2011), of the lithology (Dessert et al., 2003; Bluth and Kump, 1994; Ibarra et al., 2016), and of the paleogeography (Gibbs et al., 1999; Marshall et al., 1988; Donnadieu et al., 2006; Kent and Muttoni, 2013; Goddris et al., 2014). But the most debated issue remains the link existing between* *chemical weathering and physical erosion. Raymo et al. (1988) proposed that the uplift of major mountain ranges over the course of the Cenozoic triggered the global climatic cooling, assuming that enhanced physical erosion promotes CO2 consumption by chemical weathering...*".

Clarifications concerning the model

Primary concern related to precipitation scaling: Lines 64-65: Is the precipitation effectively an infiltration rate? Does the model include evapotranspiration (ET) or does all P go to runoff (subsurface or surface)? If ET is included does it scale with temperature, slope and/or vegetation? This comes up again at line 392 with the rainfall decrease of 5%/K rainfall. Maher and Chamberlain (2014) used a lower scaling than this with runoff from climate model experiments from Manabe. Similarly, Labat et al. (2004) is actually river discharge/runoff not precipitation.

Your are right, we did not specified that the modellings presented here do not include ET. All P go to runoff, which means that what we call precipitations here is actually runoff. The decrease of 5%/K rainfall is thus consistent with Labat et al. (2004). We use the same scaling between weathering and precipitation as Norton et al. (2014), who showed that their model fits well with data. The lower scaling used by Maher and Chamberlain (2014) means that our dependancy on precipitation may be more extreme, but applies equally to the hillslopes and colluvium. In order to clarify that we model actually runoff, we add at the beginning of section 2.1: "*Note that we do not include evapotranspiration in our simulations. Thus P is actually the net precipitation (runoff) although we call it rainfall or precipitation in the following for simplicity.*"

Regardless this 5%/K and the scaling is of a reasonable magnitude but should be discussed in more detail and I assume has substantial bearing on the results? I suspect the decisions that the authors made that have the most substantial effect on their model output and interpretation are 1) uniform precip across the domain, 2) lack of an evolving precipitation feedback (in amount and peak precipitation location) with uplift/denudation, and 3) lack of an evolving precipitation feedback over space with cooling.

Actually points 1), 2) were treated in the previous manuscript. We showed simulations with orographic precipitation specifically to test the influence of heterogeneous regolith production rate through space and time (experiments OROGRAPHIC, OROGRAPHIC REG, OROGRAPHIC Exponential, OROGRAPHIC dx=20m, OROGRAPHIC 1/B). In these models, there is a feedback between rainfall and denudation (but not with uplift) as the transient hypsometry has a direct influence on the rainfall pattern and rainfall pattern evolution has an impact on the relief development. Concerning point 3), it is true that there is no specified relationship between rainfall and temperature strictly speaking. Nevertheless, this relationship is partly and implicitly taken into account in the imposed gaussian relationship between rainfall and elevation, as orographic precipitation with rainfall maximum results partly from condensation associated with temperature. We acknowledge that a more physical orographic model (e.g. Roe et al., 2002; 2003; 2004; Anders et al., 2008; Han and Gasparini, 2015) including temperature dependency would be a better model to explore this temperature feedback. We let this for future works.

Cooling changes precipitation distributions and the amount of precipitation over mountains due to several different effects: 1) the locus of precipitation is shifted downslope (so assigning a maximum of the distribution to a fixed elevation on cooling may be over-simplified). 2) mountains become more effective at capturing precipitation (less is sent over the mountain) which means that at low elevations it is possible that cooling could increase mountain precipitation (especially at lower relative humidity locations in mid-latitude mountain ranges on the west side of continents), and 3) cooling generally decreases total precipitation in orographic settings (except, see 2) but this doesn't happen linearly over space. Instead, the percentage change in precipitation rate is typically inversely proportional to the initial precipitation rate. This matters because: - Changing the location of peak precipitation and the distribution of precipitation should impact the time and ability for the model reach steady state. - Changing the location of maximum precipitation will also be expressed in the geomorphology and the erosion processes modeled by Cidre. - Figure S1 shows that normalized erosion and weathering rates reach a steady state along the same trajectory regardless of the height/width set up of the mountains. Orographic precip feedbacks listed above plus the feedback of precipitation evolution with uplift would likely change the response time to steady state for weathering and erosion between domains mountains of different height and widths. Such limitations should be discussed. I believe some of these effects were also addressed by Willett (1999, JGR) and references within.

Yes, your point is very important. We agree with all these statements and we acknowledge that the orographic model used in our modellings is oversimplified and that the similarity between experiments using similar non-dimensional numbers at z = 0 m can be lost in case of orographic precipitations. We explained this in the Supplementary material, but we added a paragraph at the end of section 2.5 in the main manuscript to be clear: "*For fixed values of temperature and precipitations, the complexity of this model is actually reduced to seven non-dimensional numbers reflecting a great diversity of natural climatic, weathering and erosion situations. Nevertheless, there are limitations to this similarity in some of the following experiments that use elevation dependent temperatures and precipitations. For example, the cooling of the surface temperature imposed by a mountain uplift decreases the regolith production rate through time. This decrease will be more pronounced in high mountains than in low mountains. During the rise of high mountains, the initial regolith that formed at low (warm) elevations may rapidly disappear. On the contrary it may continue to cover the low mountains. The weathering outflux will evolve differently in both cases. In these cases,* N_{clast} *and* N_{reg} *are given for the temperature and precipitation at base level of the final topography*".

That said, part of the feedback between rainfall peak locus and relief development is already illustrated in the presented orographic experiments using different rainfall peaks at 1000 or 2000 m. When the maximum elevation is larger than the elevation corresponding to the max rainfall, the relief above that elevation increases faster because it receives less rainfall. There is a kind of decoupling between the hypsometry evolutions below and above the elevation of max rainfall. This difference appears in the drainage density of 3D topo at 20 Ma illustrated by Figures 8b and 8c. As you postulated, this complex feedback influences the denudation and weathering rates, as illustrated by Figure 8b. We do not insist on these effects here (focus of a following up paper). In the present contribution, our point is to test the robustness of our main conclusion regarding the colluvium illustrating different situations, not exhaustively, but including variations of rainfall and temperature patterns through time.

Overall, the relationship between orographic rainout and temperature probably depends on (among other things) the shape/hypsometry of the mountain range, its height (especially relative to the scale height) and the location of its steepest slope (where vertical velocity is the greatest). While accounting for all of these complexities is beyond the scope of the model, the authors should at least recognize that feedbacks associated with orographic precipitation and its interaction with 1) evolving topography and 2) evolving temperature are far more complex than their model demonstrates, and may have significant implications for the progression of feedbacks and especially the evolution towards steady state profiles.

OK. At the beginning of the Discussion section 4, we added: "*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.*"

Since the mountain range being built in the model is as high as 7km, it is safe to say that a mountain directly intercepting moisture will pull a majority of moisture out of the atmosphere. One simple modification the authors could make is to constrain the total precipitation over the domain by Clausius-Clape and assuming it all gets removed, instead of imposing a 5%/K decrease. This approach assumes that humidity is 100%, and therefore may over-represent total precipitable water, but may give a more realistic change in precipitation change per degree K change over the domain.

Thanks for this suggestion, although I am not sure to fully understand why the total precipitation over the domain should be removed.

Additional clarifications: Equation 8: This equation looks similar to some of the GEOCARB equations, is the form of this equation originally from White and Blum (1995) and Dixon et al. (2009)? Or from original Berner, Walker, Brady and/or Lasaga papers from the 80s and early 90s?

We added the following citations: "*(White and Blum, 1995; Oliva et al., 2003; Dessert et al., 2003)*".

Line 223-232: The authors acknowledge that secondary mineral precipitation is not included in their model and that the clast weathering relationship is dissolution only. Using the same Santa Cruz study site Maher et al. (2009) demonstrated that secondary minerals are as important as transport on determining long-term chemical weathering rates. Further, weathering rind work (see work from Brantley, Navarre-Sitchler Sak and coauthors) and soil based work (see for example recent paper by Buss et al., 2016, GCA) demonstrate the importance and presence of secondary minerals. Further, as pointed out by Bouchez and Gaillardet (2014, Geology), the specifics of the net weathering stoichiometry are extremely important in setting the expected weathering flux from a denuding landscape. These points need to be discussed as a qualifier of this model as currently implemented. I suggest an expansion of the paragraph starting line 230 with a discussion of the results in the supplementary material. Is the inclusion of the humped law in regolith production to the clast weathering not feasible?

We agree with that. Precipitation of secondary minerals should decrease the total weathering outflux. This flux is thus maximum in the present Cidre version. At the end of section 2.2, we added: "*In particular, the precipitation of secondary phases is known to strongly modulate the weathering front advance (Navarre-Sitchler et al., 2011), soil weathering rate (Maher et al., 2009; Vazquez et al., 2016) and catchment scale weathering rate (Bouchez and Gaillardet, 2014; Buss et al. 2017). Neglecting the precipitation of secondary phases overestimates the weathering outflux*". In the future, this could be accounted for by, for example, modulating the weathering rate by the same humped law used for regolith production (Vanwalleghem et al, 2013), "*and allowing the precipitation of secondary phases within the pores of clasts*".
The inclusion of the humped law for clast weathering is feasible. We also think that we could reprecipitate secondary phases directly within clasts, filling their porosity that is traced trough time.

Questions and specific comments

Line 63: Is the Cidre c++ code available in previous publications or made available online?

It is available upon request, not directly online. A complete user-guide is still lacking and I (SC) prefer to communicate directly with potential users.

Line 108: Rainfall rate or infiltration rate? See later comments on consistent terminology.

It is rainfall. We specified it more clearly at the beginning of section 2.1.

Line 115-117: Have other references or modern observations suggested this optimum thickness?

There are very few as far as we know. This was suggested by Gilbert (1877) and the data from Heimsath et al. (2001) and Wilkinson et al. (2005) may show such an optimum thickness. We added the latter as reference.

Line 148: Transition needed here between sections before launching into the new coast weathering parameteriz

We added "*The previous regolith production model allows the dynamic coupling between denudation and weatherable material production, but it is not used to model weathering flux. This flux is calculated by tracing clasts that dissolve*".

Line 174: Provide examples from literature of others who have defined reactive surface areas (for example papers by Navarre-Sitchler, Maher, Brantley etc.)

We added "*see other definitions for example in Godderis et al. (2006), Brantley et al. (2008), Maher et al. (2009), Navarre-Sitchler et al. (2011)*"

Line 277: Here P is "effective precipitation rate (runoff)" elsewhere it is just stated as precipitation. Please clarify this throughout and ensure consistency.

Right, we responded above.

Line 328: This length and width is similar to the average Andes and Himalayan catchments? At what scale was this determined? Are there references analyzing the catchment sizes and hypsometries showing that this is the correct scale?

This is an order of magnitude. We modified this sentence by "*(...), an order of magnitude of catchment size that can be found for example in Himalayan or Andes*".

`Line 331: As noted by the other reviewer this is very high and would result in glacier formation, a process not included in this model.`
Yes, we responded above. We agree that the lack of glacier erosion is a drawback of our study. We wanted to have large relief in order to study the effect of large temperature gradient on the regolith pattern development.

`Line 370: "parametrical model"?`
we removed "parametrical".

`Lines 507-509: This appears to be the emerging paradigm rather than the alternative.`
Nothing against that, except that the variability of soil cover, subsurface rock structure, water table shape etc is such in a mountains that things may be more complicated.

`Line 527: How does this compare to other U series studies from Puerto Rico and elsewhere?`
Are you referring to Dosseto et al., GCA (2014) ? In Puerto Rico, erosion rates are one order lower than erosion rates considered in our reference cooling experiments (Bierman et al., 1995, 1998; Riebe et al., 03) and the mean weathering age range between 5 to 20 kyrs (Dosseto et al., 2014), namely one order of magnitude larger than in our reference cooling experiments. We would like to see consistency in these ratios, but it seems that in the Puerto Rico case, weathering occurs dominantly on the hillslopes whereas it occurs mainly in the valleys in our simulations. Note however that Dosseto et al. (2014) interpret these weathering ages as reflecting the removal of deep low-depleted bedrock and depleted saprolite by landslides. They suggest that such fresh sediment can then weather during river transport, although this river contribution is not quantified (if we understand well).

`Line 538-541: Absolutely agree, models of fluid residence time and the impact of colluvium are not mutually exclusive but actually complementary. To that point, does the Cidre track water residence time? If so, a figure showing how fluid residence time distributions change through time would be of interest and would make this point that they are complementary.`
Unfortunately, not yet...

`Line 555: Maher and Chamberlain (2014) - I believe their residence time should be interpreted as the residence of water in the weathering zone to the river, not just ground- water.`
Yes, you are right, we modified this sentence.

`Figure 2: Difficult to see red/green in c, suggest reducing to even less than half of the clasts randomly shown or blowing c up relative to other panels a and b.`
OK we increased the size of panel c.

`Figure 3: The regolith thickness is the average over the domain?`
Yes. We added this information in the caption.

`Figure 4: Difficult to read the grey text with the actual catchment data. - Why was the Dixon and von Blanckenburg (2012) compilation not used as well? - Is the West (2012) line the ?best fit? value from his Figure 2A? Suggest also including the confidence intervals.`
We increased the darkness of the text. Unfortunately, we were unable to get the table associated with the Dixon and von Blanckenburg paper.

`Figure 8: Are the orographic models 1 and 2 supported by data? I assume the elevations are still getting up to 7 km? Which means there is almost no precipitation occur at the mountain peaks?`
This model is based on Colberg and Anders, Geomorphology (2013). At high elevation, the precipitation is low. That is why the relief looks higher and the drainage density lower at high elevation in Figure 8c and d for 20 Ma.

Technical Corrections

Line 19: Please change ?silicates? to ?silicate minerals? or ?silicate bedrock?
Done.

Line 20: Walker et al. (1981) citation - more precisely the weathering of silicate min- erals imposes a negative feedback on the long-term carbon cycle of the Earth system over 10?5 to 10?6+ years. Please also include additional classic and more recent refer- ences such as Urey (1952), Berner et al. (1983, AJS) and references cited later in this paragraph.
We modified this sentence as "*The weathering of silicate minerals, in particular, imposes a negative feedback on the long-term carbon cycle of the Earth system over timescales of* 10^5 *to* 10^6 *years (Walker, 1981; Berner et al., 1983)*".

Lines 22: suggest also including additional references on this debate, including Misra and Froelich (2012, Science), Raymo and Ruddiman (1992, Nature), Torres et al. (2014, Nature), Caves et al. (2016, EPSL) and references therein.
All these references are very useful to illustrate the debate about the C source or sink role of mountains. Nevertheless, our paper is not a review paper. The reference to Caves et al. (2016, EPSL) was added elsewhere.

Please re-order Dixon et al. (2009) references, this should be ?a?
Done.

Line 66: change "or" to "and"
Done

Line 336: Need citation for this Ea value and give the values for arbiter and biotite.
Brantley et al., (2008) is cited for these values.

Line 337: Add citation demonstrating that these minerals control the weathering front advance rate in granitic regolith production.
From our knowledge, there is some debate, as these minerals can correspond to a large range of kinetic parameters (biotite in particular). Albite to Kaolinite is usually used to model weathering front advance (e.g. Lebedeva et al., ESPL, 2019). Buss et al. (2004) and White et al. (2008) among others have provided evidences that dissolution of albite controls the rate of the weathering front advance.

Line 338: Previously this To was given as just 298 K
Thanks, we corrected it.

Line 360: Change "in" to "on"
Done.

Line 361: Remove "as"
Done.

Line 409: ?This? change to ?These
Done.

Line 431: ?a? should be ?an?
Done.

Line 481: Key weathering outflux via groundwater citations are Shopka and Derry?s work from Hawaii.

Thanks we added this ref.

`Line 576:  I think you mean ?outside the scope?  rather than ?within the scope?`
No, actually we think that this particular question can be tackled by using our model in a next study.

Thanks for this detailed and constructive review.

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.