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.