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Earth Surface Dynamics Discussions

Interactive comment on "Colluvial deposits as a possible weathering reservoir in uplifting mountains" *by* Sébastien Carretier et al.

Anonymous Referee #2

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Summary and General Comments

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 landscape, 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 component. 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

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Discussion paper

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.

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.

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.

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.

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.

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.

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.

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.

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 w/ 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.

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.

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-Clapeyron 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.

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?

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?

Questions and specific comments

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

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

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

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

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

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

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?

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.

Line 370: "parametrical model"?

Lines 507-509: This appears to be the emerging paradigm rather than the alternative.

Line 527: How does this compare to other U series studies from Puerto Rico and elsewhere?

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.

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 groundwater.

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.

Figure 3: The regolith thickness is the average over the domain?

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.

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?

Technical Corrections

Line 19: Please change “silicates” to “silicate minerals” or “silicate bedrock”

Line 20: Walker et al. (1981) citation - more precisely the weathering of silicate minerals imposes a negative feedback on the long-term carbon cycle of the Earth system over 10ˆ5 to 10ˆ6+ years. Please also include additional classic and more recent references such as Urey (1952), Berner et al. (1983, AJS) and references cited later in this paragraph.

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.

Lines 24-25: Please re-order Dixon et al. (2009) references, this should be “a”

Line 66: change "or" to "and"

Line 336: Need citation for this Ea value and give the values for arbiter and biotite.

Line 337: Add citation demonstrating that these minerals control the weathering front advance rate in granitic regolith production.

Line 338: Previously this To was given as just 298 K

Line 360: Change "in" to "on"

Line 361: Remove "as"

Line 364: Change "in" to "on", "Then" not needed.

Line 409: “This” change to “These”

Line 431: “a” should be “an”

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

Line 576: I think you mean “outside the scope” rather than “within the scope”

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