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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.

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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 exhumed 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.

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.

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. 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.

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. 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. 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

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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.

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.

Please also note the supplement to this comment:
https://www.earth-surf-dynam-discuss.net/esurf-2017-48/esurf-2017-48-RC1-supplement.pdf

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