

Reviewer 2:

We would like to thank reviewer 2 for their overall positive response to the paper. We have replied to each comment below with the reviewers comments in bold followed by our replies.

1. In general I think that model development has to have a clear motivation and underlying question. The authors claim that the development of their model is motivated by “several important global-scale questions”. However, I think that one cannot make a general pedogenesis model that can be used to answer all questions. Only with a concrete aim or question the modeler can decide on the level of complexity and which processes have to be included, while accounting for the computational cost and data availability. Therefore, I suggest the authors to define a clear motivation at the beginning and in the discussion and conclusion to link to the general motivation, clearly stating what are the relevant processes that still need to be considered on the one hand and state the advancement of understanding on the other.

If the authors were considering using their model for other settings than those of Hawaii, which is an erosional landscape, I would argue tectonic uplift should be added in the list of missing processes, and evaluating their results in other sequences on continental regions would be necessary.

If the motivation is to build a model that allows understanding the effect of weathering on the long-term carbon cycle, I think one has to include processes at longer time scale as well, for example tectonic uplift, sea level rise and erosion in a more mechanistic way.

The main aim of this paper is to introduce the model and highlight the potential future uses of the model. For this first paper we do not aim to answer a specific hypothesis but rather aim to demonstrate the potential of the model and examples of what we can learn from such a model (e.g. the role that vegetation plays in accelerating nutrient release from minerals). In a subsequent study we will use the soil profile model to explore interactions between chemical weathering, physical weathering and vegetation, which has implications for the long-term carbon cycle. For example studies suggest that vegetation accelerates silicate mineral weathering by a factor of 1.5 - 10 (Moulton et al., 2000; Berner et al., 2003) which can explain abrupt changes in atmospheric CO₂ concentrations and temperatures in the past. Particularly the large drawdown events associated with the onset of vascular plant colonization ~360 million years ago (Berner, 1997). We will explore how vegetation influences silicate mineral weathering for different weathering regimes e.g. transport limited or weathering limited. This will be the first time that a dynamic weathering model has been used to quantify these processes. The initial conditions, climate and parameters would of course be adapted to the necessary continental region. We would also formulate tectonic uplift in a similar manner to the way we formulate surface erosion (i.e. a

shifting coordinate mechanism). The ease with which these parameters and processes can be introduced is a major advantage of our model

However we agree that for this study we should focus the introduction and so we have rewritten this section.

For approaching questions related to mineral nutrient limitation in the lowland Amazon Basin (P limitation), I think that one has to consider tectonic uplift and more explicit vegetation dynamics, such as mycorrhizal uptake, root exudation, occlusion processes and exogenous P inputs. I was surprised to find a figure relating their model results to Amazon soils, because I find no reasoning that would allow to use the model framework proposed and tested for Hawaii to the Amazon, which is quite distinct in its geologic settings.

Because we don't include these complex vegetation-nutrient interactions in the model we are able to deduce in this study that these are indeed an essential component of many nutrient cycles.

With regards to the comparison with soil organic carbon from Manaus, please see response to reviewer 1. In addition to our reply, we would also like to make clear that soil carbon is simulated at steady-state due to the shorter timescales that these dynamics operate over compared with other soil forming processes. And although soil organic carbon feeds into modeled pedogenesis via increased acidity, at the moment processes of soil formation do not feed into modeled soil organic carbon. So for the soil organic carbon comparison study which we use to demonstrate the influence of different mixing scenarios, whether the model uses initial conditions for Hawaii or the Amazon will make no difference at this stage. Once the model has been adapted to include vegetation which evolves with the nutrient status of the soil, then the initial conditions and therefore site will be important.

The authors use the method of Cosmogenic nuclides to estimate surface erosion rate. From the paper I understood that this method could be used in places where soil have reach a steady-state (P 9 L11). Contradictorily, the authors parameterize this in a merely denudation landscape, where soil production from bedrock does not balance rates of loss due to surface erosion. This is evidence by fact that after few millions of years of soil development the islands in Hawaii disappear.

We merely refer to these cosmogenic nuclide studies in order to inform the reader of the ranges of erosion rates which occur worldwide. When comparing the model with the Hawaiian sites we find erosion rates from the literature which have been measured on the Hawaiian islands see Page 5833, Line 12-17

3. I was not able to fully understand how vegetation dynamics are represented in the model. The soil model drives changes in nutrient availability over time; however, I do not understand how at the same time that the model assumes a constant nutrient carbon stoichiometry in

vegetation (and SOM) the productivity is kept constant in over time. Could the authors please explain better how nutrient are balanced in vegetation and how the assumption of constant stoichiometry relates to gross primary productivity, biomass production and soil organic matter decomposition.

We thank the reviewer for highlighting our poor explanation of nutrient dynamics. We do indeed state that the model assumes constant nutrient stoichiometry in the vegetation, we should actually say that the model assumes constant *optimum* nutrient stoichiometry, this is now revised. If the nutrient concentration in solution is too low or if the rate of evapotranspiration is too low then this optimum ratio will not be met and nutrient stoichiometry in the vegetation will deviate from the optimum. We have also added the following sentence to page 5826 Line 14 “In the case of the soil not being able to supply enough of nutrient *i* to meet the optimum C:*i* ratio, then the nutrient stoichiometry will deviate from the optimum”.

Primary productivity (N_p), is, however, constant regardless of whether the optimum amount of nutrients is taken up from the soil. We are aware that for a more realistic representation in the future, vegetation productivity should respond to nutrient availability. For this model introduction paper we have tried to keep the processes simple, introducing realistic dynamic vegetation is beyond the scope of this study, and in fact, even the most sophisticated Dynamic Global Vegetation Models (DGVMs) do not include nutrient interactions because of the great complexity. We believe this paper, however, can provide a means of introducing nutrients to such models.

4. I personally like modeling studies that provide an overview over the processes that are build in the model and the assumptions they are based on. I think including a diagram (e.g. flow chart) may further help to get an overview over the model structure. Therefore, I suggest including such a diagram and clearly state the model assumptions and processes considered (also with respect which ones have been developed and which ones were already incorporated in Kirkby (1985)).

We include a model schematic in the revised version of the paper.

References

Berner, E. K., R. A. Berner, and K. L. Moulton (2003), Plants and mineral weathering: present and past, *Treatise on Geochemistry*, 5, 169–188.

Berner, R. A. (1997), The rise of plants and their effect on weathering and atmospheric CO₂, *Science*, 276(5312), 544–546.

Moulton, K. L., J. West, and R. A. Berner (2000), Solute flux and mineral mass balance approaches to the quantification of plant effects on silicate weathering, *American Journal of Science*, 300, 539–570.