Marcel Hoosbeek

The authors wish to thank Marcel Hoosbeek for his careful review of the manuscript. We have tried to clarify and justify each of his concerns below with the reviewers comments in bold followed by our replies.

Section 2.1.2. In case evapotranspiration surpasses precipitation, it an upward water flow possible? For instance, is calcium accumulation in the top soil possible in an arid environment?

For the current model formulation upward water flow will not occur. However the simplicity of the model structure means that such a process can be included if studying arid systems is of interest.

The following four comments relate to the vegetation dynamics in the model, we would like to clarify that for this demonstration of the model we have kept vegetation very simple so that we can clearly identify the feedbacks between pedogenesis and vegetation. We believe that for further model studies the vegetation can easily be adapted and developed to suit the needs of the study in question.

Section 2.5.1. With nutrient cycling included in the model it seems tempting to make biomass production (Np) dependent on nutrient availability (through stoichiometry). As stated in op page 5823, line 25, this may improve early-stage ecosystem development.

We agree that this will need to be included, particularly when using the model to study the early stages of soil development, however, this will form part of a more dynamic vegetation module which we believe is beyond the scope of this current paper.

Section 2.5.2. Root respiration (Rc) is now a number taken from the literature. But, Rc is of course related to Np. And with Np related to nutrient status, vegetation-soil interactions may become even more dynamical. Not a necessity for the current model (and manuscript), but rather a thought for the future.

Again this is an important observation of the modeled vegetation. The simplification of the vegetation dynamics involves keeping Np at steady-state (production equals litter losses) and time invariant. Hence root respiration also remains time invariant.

Section 2.6. Do I understand correctly that nutrients are released into soil solution based on the stoichiometry of fresh litter? So, SOM does not approach, for instance, the C:N ratio of microbial populations of over time? Because, it takes nutrients to store C in the soil (lower C:nutrient ratios over time), the nutrient availability may be overestimated in the model.

Yes this is correct. Because we are concerned primarily with the modelling of pedogenesis we have not at this stage developed a sophisticated vegetation model, so for example we do not model N dynamics, which although is linked to pedogenesis in terms of the type of vegetation able to colonise young, N poor soils and as suggested is important for SOM stocks, it does not play a large role in influencing long-term pedogenetic processes. We believe that in the future such processes can be included with a coupled dynamic vegetation model.

Section 4.4. The belowground C stocks presented in figure 6 are compared with data from forest plots near Manaus. This seems a bit odd. Earlier in the model description section (and later in section 5) I had gotten the impression that the model input data were taken from a chronosequence on Hawaii. Moreover, based on figure 6 it was concluded that "the decreasing decay rate with increasing soil depth is perhaps the most realistic formulation". But, if a soil with less SOM below 1 m had been selected, would the conclusion have been the opposite?

We agree that choosing to compare the modeled below ground carbon stocks with observations from the Amazon may seem a 'bit odd'. We would however like to point out that for the below ground carbon comparison the input data e.g. N_P and allocation of N_P to the four carbon pools is parameterized with those of the Manaus site. This is a unique site where vegetation processes have been intensively recorded and soil carbon measured. We believe therefore that using these measured vegetation parameters in the model and comparing the below ground carbon from the same site allows us to evaluate the model's belowground mixing and decomposition parameters, as all else is the nearly same (i.e. the amount of carbon entering the soil). We realize that we have not explained the reasoning for comparing the organic carbon to this Manaus site in the text so we have amended the caption of figure 6 to "The total N_P and allocation of N_P to the four carbon pools for both runs is the same as the previous simulations which is equal to that estimated by Malhi et al (2009) for the Manaus plot (Table 1). The modeled carbon entering the soil should therefore be equal to that entering the Manaus site" in the revised version. If however, the Editor would prefer us to use published data from Hawaii to maintain the flow of the paper, we will endeavor to do this.

Section 4. The evaluation of the effects of the step-by-step addition of processes (Figs 2 – 3) on simulation results makes sense to people with sufficient pedological knowl- edge and experience. But, it is rather subjective and hard to verify.

We believe that the figure labels along with the figure caption provides enough detail for non pedological scientists to be able to interpret the figure.

Section 5, page 5833, line 27. "The depth of the vertical model layers is increased to 0.25m . . ." Should this be 0.025 m? As compared to Zr and other parameters, 0.25m seems too thick.

0.25m is correct to achieve numerical stability of the advection equation particularly for the longer and wetter runs. An alternative is to decrease the time step, however, for the long 170 and 350 kyr simulations this becomes very time expensive. This resolution is a close match to that of the observations.

Page 5836, line 11. "is still is still"

Revised.

Section 5. The model evaluation based on the Hawaiian chronosequence is informa- tive. Model advancements and limitations are well described, although in this type of study the evaluation of results is inherently subjective.

Because model evaluation is limited, I think emerging "insights" should be taken cau- tiously. I think the title is overstating this aspect. New "insights" are not the major result of this study, as suggested by the title. Still, the presented study is an interesting addition to earlier work by Kirkby and others in this field of science.

We appreciate that the reviewer finds this work an interesting addition to earlier modeling attempts.

If possible we would prefer not to change the title.

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 un- derlying 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 be- ginning and in the discussion and conclusion to link to the general motivation, clearly stating what are the relevant processes that still need to be consider on the one hand and state the advancement of understanding on the other.

If the authors were considering using their model for other settings then 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 cronosequences on con- tinental 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 CO_2 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 re-worded some of this.

For approaching questions related to mineral nutrient limitation in the lowland Amazon Basin (P limiation), 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 quiet 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 (Np), 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 [proof-of-concept] 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 flow chart in the revised version of the paper. Figure 1 details all of the major processes, inputs and outputs in the model. We have not complicated the figure further by discussing the assumptions made as we believe this figure and the discussed assumptions in the text combined provide a solid overview of the model.

References

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