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.